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OFFICERS.

For year ending February, 1895.*

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(Term expires February, 1895.)

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* The following officers were elected at the Annual Meeting, February, 1895: *President*, Joseph D. Weeks, Pittsburgh, Pa.; *Vice-Presidents* (to serve two years), Wheaton B. Kunhardt, New York City, James F. Lewis, Chicago, Ill., Charles A. Stetefeldt, Oakland, Cal.; *Managers* (to serve three years), Levi Holbrook, New York City, Albert R. Ledoux, New York City, William R. Webster, Philadelphia, Pa.; *Treasurer*, Theodore D. Rand, Philadelphia, Pa.; *Secretary*, Rossiter W. Raymond, New York City.

† Died at Freiberg, February 18, 1895.

‡ Died at Vienna, March 27, 1895.

LIST OF THE MEETINGS OF THE INSTITUTE AND THEIR LOCALITIES FROM ITS ORGANIZATION TO OCTOBER, 1894.

Number.	Place.	Date.	Transactions.
I.	Wilkes-Barre, Pa.,*	May, 1871,	i. 3
II.	Bethlehem, Pa.,	August, 1871,	i. 10
III.	Troy, N. Y.,	November, 1871,	i. 13
IV.	Philadelphia, Pa.,	February, 1872,	i. 17
V.	New York, N. Y.,*	May, 1872,	i. 20
VI.	Pittsburgh, Pa.,	October, 1872,	i. 25
VII.	Boston, Mass.,	February, 1873,	i. 28
VIII.	Philadelphia, Pa.,*	May, 1873,	ii. 3
IX.	Easton, Pa.,	October, 1873,	ii. 7
X.	New York, N. Y.,	February, 1874,	ii. 11
XI.	St. Louis, Mo.,*	May, 1874,	iii. 3
XII.	Hazleton, Pa.,	October, 1874,	iii. 8
XIII.	New Haven, Conn.,	February, 1875,	iii. 15
XIV.	Dover, N. J.,*	May, 1875,	iv. 3
XV.	Cleveland, O.,	October, 1875,	iv. 9
XVI.	Washington, D. C.,	February, 1876,	iv. 18
XVII.	Philadelphia, Pa.,†	June, 1876,	v. 3
XVIII.	Philadelphia, Pa.,	October, 1876,	v. 19
XIX.	New York, N. Y.,	February, 1877,	v. 27
XX.	Wilkes-Barre, Pa.,*	May, 1877,	vi. 3
XXI.	Amenia, N. Y.,	October, 1877,	vi. 10
XXII.	Philadelphia, Pa.,	February, 1878,	vi. 18
XXIII.	Chattanooga, Tenn.,*	May, 1878,	vii. 3
XXIV.	Lake George, N. Y.,	October, 1878,	vii. 103
XXV.	Baltimore, Md.,*	February, 1879,	vii. 217
XXVI.	Pittsburgh, Pa.,	May, 1879,	viii. 3
XXVII.	Montreal, Canada,	September, 1879,	viii. 121
XXVIII.	New York, N. Y.,*	February, 1880,	viii. 275
XXIX.	Lake Superior, Mich.,	August, 1880,	ix. 1
XXX.	Philadelphia, Pa.,*	February, 1881,	ix. 275
XXXI.	Staunton, Va.,	May, 1881,	x. 1
XXXII.	Harrisburg, Pa.,	October, 1881,	x. 119
XXXIII.	Washington, D. C.,*	February, 1882,	x. 225
XXXIV.	Denver, Col.,	August, 1882,	xi. 1
XXXV.	Boston, Mass.,*	February, 1883,	xi. 217
XXXVI.	Roanoke, Va.,	June, 1883,	xii. 3
XXXVII.	Troy, N. Y.,	October, 1883,	xii. 175
XXXVIII.	Cincinnati, O.,*	February, 1884,	xii. 447
XXXIX.	Chicago, Ill.,	May, 1884,	xiii. 1
XL.	Philadelphia, Pa.,	September, 1884,	xiii. 285
XLI.	New York, N. Y.,*	February, 1885,	xiii. 585

* Annual meeting for the election of officers. The rules were amended at the Chattanooga meeting, May, 1878, changing the annual election from May to February.

† Begun in May at Easton, Pa., for the election of officers, and adjourned to Philadelphia.

Number.	Place.	Date.	Transactions.
XLII.	Chattanooga, Tenn.,	May, 1885,	xiv. 1
XLIII.	Halifax, N. S.,	September, 1885,	xiv. 307
XLIV.	Pittsburgh, Pa.,*	February, 1886,	xiv. 587
XLV.	Bethlehem, Pa.,	May, 1886,	xv. lxiii.
XLVI.	St. Louis, Mo.,	October, 1886,	xv. lxx.
XLVII.	Scranton, Pa.*	February, 1887,	xv. lxxvii.
XLVIII.	Utah and Montana,	July, 1887,	xvi. xvii.
XLIX.	Duluth, Minn.,	July, 1887,	xvi. xxiv.
L.	Boston, Mass.,*	February, 1888,	xvi. xxviii.
LI.	Birmingham, Ala.,	May, 1888,	xvii. xix.
LII.	Buffalo, N. Y.,	October, 1888,	xvii. xxiv.
LIII.	New York, N. Y.,*.	February, 1889,	xvii. xxxi.
LIV.	Colorado,	June, 1889,	xviii. xvii.
LV.	Ottawa, Canada,	October, 1889,	xviii. xxiv.
LVI.	Washington, D. C.,*	February, 1890,	xviii. xxx.
LVII	New York, N. Y.,	September, 1890,	xix. vii.
LVIII.	New York, N. Y.*	February, 1891,	xix. xxv.
LIX.	Cleveland, O.,	June, 1891,	xx. xvi.
LX.	Glen Summit, Pa.,	October, 1891,	xx. lxi.
LXI.	Baltimore, Md.,*	February, 1892,	xxi. xix.
LXII.	Plattsburgh, N. Y.,	June, 1892,	xxi. xxxiii.
LXIII.	Reading, Pa.,	October, 1892,	xxi. xliv.
LXIV.	Montreal, Canada,*	February, 1893,	xxi. lii.
LXV.	Chicago, Ill.	August, 1893,	xxii. xiii.
LXVI.	Virginia Beach, Va.,*	February, 1894,	xxiv. xvii.
LXVII.	Bridgeport, Conn.,	October, 1894,	xxiv. xxxv.

* Annual meeting for the election of officers.

PUBLICATIONS.

THE publications of the Institute comprise:

PAMPHLETS.

1. The minutes of the Proceedings of each Meeting.

2. Such of the papers presented or read by title at each Meeting as are furnished by the authors and approved by the Council for full publication. (In nearly all cases in which papers, the titles of which appear in the Proceedings, are not subsequently published, they have been withdrawn by the authors.) These papers are published separately in pamphlet form, and are marked "subject to revision." Beyond the edition distributed without charge to members and associates not in arrears, a small supply is retained to meet subsequent demand. There are no copies on hand of papers read before 1880. The stock is nearly complete from 1880. These papers are for sale at the office of the Secretary, or are sent to purchasers by mail or express, charges paid, on receipt of the price, as follows;

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The volumes of *Transactions*, which are published annually, contain the list of officers, rules, etc., the Proceedings, and the papers revised for final publication. (In this revision, after the preliminary publication, authors are permitted to use the largest liberty; and the changes and additions made in papers are sometimes important. It should be borne in mind, by those who study or quote a paper in the preliminary edition, that they may not have in that form the ultimate and deliberate expression of the author's views. It should be added, however, that in the majority of cases there is no essential change, the correction of typographical errors and additions of later information being the usual alterations.)

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All communications and remittances should be addressed to R. W. Raymond, Secretary, P. O. Box 223, New York City.

RULES

ADOPTED MAY, 1873. AMENDED MAY, 1875, 1877, AND 1878, FEBRUARY, 1880, 1881, 1887, AND 1890.

I.

OBJECTS.

THE objects of the AMERICAN INSTITUTE OF MINING ENGINEERS are to promote the arts and sciences connected with the economical production of the useful minerals and metals, and the welfare of those employed in these industries, by means of meetings for social intercourse, and the reading and discussion of professional papers, and to circulate, by means of publications among its members and associates, the information thus obtained.

II.

MEMBERSHIP.

The Institute shall consist of Members, Honorary Members, and Associates. Members and Honorary Members shall be professional mining engineers, geologists, metallurgists, or chemists, or persons practically engaged in mining, metallurgy, or metallurgical engineering. Associates shall include all suitable persons desirous of being connected with the Institute, and duly elected as hereinafter provided. Each person desirous of becoming a member or associate shall be proposed by at least three members or associates, approved by the Council, and elected by ballot at a regular meeting (or by ballot at any time conducted through the mail, as the Council may prescribe) upon receiving three-fourths of the votes cast, and shall become a member or associate on the payment of his first dues. Each person proposed as an honorary member shall be recommended by at least ten members or associates, approved by the Council, and elected by ballot at a regular meeting (or by ballot at any time conducted through the mail, as the Council may prescribe) on receiving nine-tenths of the votes cast; *Provided*, that the number of honorary members shall not exceed twenty. The Council may at any time change the classification of a person elected as associate, so as to make him a member, or *vice versa*, subject to the approval of the Institute. All members and associates shall be equally entitled to the privileges of membership; *Provided*, that honorary members shall not be entitled to vote.

Any member or associate may be stricken from the list on recommendation of the Council, by the vote of three-fourths of the members and associates present at any annual meeting, due notice having been mailed in writing by the Secretary to the said member or associate.

III.

DUES.

The dues of members and associates shall be ten dollars, payable upon their election, and ten dollars per annum thereafter, payable in advance at the annual meeting. Honorary members shall not be liable to dues. Any member or associate not in arrears may become by the payment of one hundred dollars at one time a life-member or associate, and shall not be liable thereafter to annual dues. Any member or associate in arrears may, at the discretion of the Council, be deprived of the receipt of publications, or stricken from the list of members when in arrears for one year; *Provided*, that he may be restored to membership by the Council on payment of all arrears, or by re-election after an interval of three years.

IV.

OFFICERS.

The affairs of the Institute shall be managed by a Council, consisting of a President, six Vice-Presidents, nine Managers, a Secretary and a Treasurer, who shall be elected from among the members and associates of the Institute at the annual meetings, to hold office as follows:

The President, the Secretary, and the Treasurer for one year (and no person shall be eligible for immediate re-election as President who shall have held that office subsequent to the adoption of these rules, for two consecutive years), the Vice-Presidents for two years, and the Managers for three years; and no Vice-President or Manager shall be eligible for immediate re-election to the same office at the expiration of the term for which he was elected. At each annual meeting a President, three Vice-Presidents, three Managers, a Secretary and a Treasurer shall be elected, and the term of office shall continue until the adjournment of the meeting at which their successors are elected.

The duties of all officers shall be such as usually pertain to their offices, or may be delegated to them by the Council or the Institute; and the Council may in its discretion require bonds to be given by the Treasurer. At each annual meeting the Council shall make a report of proceedings to the Institute, together with a financial statement.

Vacancies in the Council may occur by death or resignation; or the Council may, by a vote of the majority of all its members, declare the place of any officer vacant, on his failure for one year, from inability or otherwise, to attend the Council meetings or perform the duties of his office. All vacancies shall be filled by the appointment of the Council, and any person so appointed shall hold office for the remainder of the term for which his predecessor was elected or appointed; *Provided*, that the said appointment shall not render him ineligible at the next annual meeting.

Five members of the Council shall constitute a quorum; but the Council may appoint an Executive Committee, or business may be transacted at a regularly called meeting of the Council, at which less than a quorum is present, subject to the approval of a majority of the Council, subsequently given in writing to the Secretary, and recorded by him with the minutes.

V.

ELECTIONS.

The annual election shall be conducted as follows: Nominations may be sent in writing to the Secretary, accompanied with the names of the proposers, at any time not less than thirty days before the annual meeting; and the Secretary shall, not less than two weeks before the said meeting, mail to every member or associate (except honorary members), a list of all the nominations for each office so received, together with a copy of this rule, and the names of the persons ineligible for election to each office; and if the Council, or a Committee thereof, appointed for the purpose, shall have recommended any nominations, such recommendation may also be sent to members and associates with the said list of all nominations made, but not upon the same paper. And each member or associate, qualified to vote, may vote, either by striking from or adding to the names of the said list, leaving names not exceeding in number the officers to be elected, or by preparing a new list, signing said altered or prepared ballot with his name, and either mailing it to the Secretary or presenting it in person at the annual meeting; *Provided*, that no member or associate in arrears since the last annual meeting shall be allowed to vote until the said arrears shall have been paid. The ballots shall be received and examined by three Scrutineers, appointed at the annual meeting by the presiding officer; and the persons who shall have received the greatest number of votes for the several offices shall be declared elected, and the Scrutineers shall so report to the presiding officer. The ballots shall be destroyed, and a list of the elected officers, certified by the Scrutineers, shall be preserved by the Secretary.

VI.

MEETINGS.

The annual meeting of the Institute shall take place on the third Tuesday of February, at which a report of the proceedings of the Institute and an abstract of the accounts shall be furnished by the Council. Two other regular meetings of the Institute shall be held in each year, at such times and places as the Council shall select, and notice of all meetings shall be given by mail, or otherwise, to all members and associates, at least twenty days in advance. Special meetings may be called whenever the Council sees fit; and the Secretary shall call a special meeting on a requisition signed by fifteen or more members. The notices for special meetings shall state the business to be transacted, and no other shall be entertained.

Every question which shall come before any meeting of the Institute, shall be decided, unless otherwise provided by these Rules, by the votes of a majority of the members then present. Any member or associate may introduce a stranger to any meeting; but the latter shall not take part in the proceedings without the consent of the meeting.

VII.

PAPERS.

The Council shall have power to decide on the propriety of communicating to the Institute any papers which may be received, and they shall be at liberty, when they think it desirable, to direct that any paper read before the Institute, shall be printed in the Transactions. Intimation, when practicable, shall be given, at each general meeting, of the subject of the paper or papers to be read, and of the questions for discussion at the next meeting. The reading of papers shall not be delayed beyond such hour as the presiding officer shall think proper; and the election of members or other business may be adjourned by the presiding officer, to permit the reading and discussion of papers.

The copyright of all papers communicated to, and accepted by, the Institute, shall be vested in it, unless otherwise agreed between the Council and the author. The author of each paper read before the Institute shall be entitled to twelve copies, if printed, for his own use, and shall have the right to order any number of copies at the cost of paper and printing, provided said copies are not intended for sale. The Institute is not, as a body, responsible for the statements of fact or opinion advanced in papers or discussions at its meetings, and it is understood that papers and discussions should not include matters relating to politics or purely to trade.

VIII.

AMENDMENTS.

These Rules may be amended at any annual meeting by a two-thirds vote of the members present; *Provided*, that written notice of the proposed amendment shall have been given at a previous meeting; *and Provided, also*, that the amendment or amendments so adopted shall be printed upon a ballot and sent, not later than the next distribution of printed matter, to all members and associates not in arrears for the preceding year (except honorary members and foreign members elected before February, 1880), and each person receiving the same shall be requested to return it to the Secretary with his written vote of Yes or No to each amendment, and his signature; and the President shall appoint as scrutineers three members or associates, who shall examine all of the said ballots which shall have been returned within one month from the date of their distribution, and shall report the result; and the Secretary shall publish and distribute to members, not later than the next distribution of printed matter, an announcement of the said result so reported, together with the text of the additional or amended rule or rules so adopted; and the amendment or amendments approved by the majority of the ballots so returned and reported shall become part of these rules from and after the publication of said announcement by the Secretary.

PROCEEDINGS OF THE SIXTY-SIXTH (TWENTY-FOURTH ANNUAL) MEETING, VIRGINIA BEACH, VA., FEBRUARY, 1894.

Local Committee.

John Graham, Jr., *Chairman;* Hon. C. T. O'Ferrall, Governor of Virginia; Hon. A. B. Cooke, Mayor of Norfolk; Rear-Admiral George Brown, Commandant, Navy Yard; Lieut.-Col. R. T. Frank, Commandant, Fort Monroe; M. Glennan, proprietor *Norfolk Virginian;* K. C. Murray, proprietor *Norfolk Landmark;* Col. William Lamb, President Norfolk Chamber of Commerce; W. T. Brooke, Norfolk City Engineer; Colonel E. M. Henry, Major A. Myers, Judge Legh R. Watts, Judge Tucker Brooke, Capt. N. M. Osborne, C. B. Orcutt, Horace See, Jed. Hotchkiss, David T. Day, J. Harry Lee, W. W. Coe, Barton Meyers, R. B. Cooke, James Riddle, Walter Sharp, W. H. Adams, Walter Graham, Dawson McCormick, W. B. Brooks, Jr., Wm. H. White, R. B. Tunstall, Fergus Reid, Alfred P. Thom, Walter H. Taylor, Albert Grandy, Thom W. Henderson, C. R. Boyd.

Hotel Headquarters.—Princess Anne, Virginia Beach.

On arrival in Norfolk on Tuesday morning, February 20th, the members of the Institute were the guests of the Norfolk Chamber of Commerce at breakfast at the Atlantic Hotel. At nine o'clock they were escorted to the rooms of the Chamber, where addresses of welcome were made by Mr. John Graham, Jr., Chairman of the Local Committee, Major Jed. Hotchkiss, of Staunton, on behalf of the State; Mayor A. B. Cooke, on behalf of the municipality; Mr. Barton Meyers, a director of the Chamber of Commerce; Col. E. M. Henry, President of the Business Men's Association, and Major A. Myers, President of the Real Estate Exchange. Carriages were then taken for a ride about the city, and at 12.30 o'clock the members and their friends left by special train for Virginia Beach, eighteen miles distant.

The opening session was held (as were all subsequent sessions) at the Princess Anne Hotel, Tuesday evening, February 20th, at 8 P.M.

The Institute was welcomed on behalf of the Local Committee by Dr. David T. Day, of Washington, D. C., Mr. Graham, the chairman of the Committee, being absent by reason of illness in his family. President H. M. Howe, after suitably acknowledging this greeting, proceeded to deliver the Presidential address (see pp. 742 and 746).

The President announced the appointment of Messrs. W. E. C.

Eustis, J. E. Johnson and P. N. Moore, as scrutineers, to examine the ballots received for officers and to report the result of the election.

The remainder of the session was occupied with a talk by R. W. Raymond, of Brooklyn, N. Y., on Mining and Metallurgy, as represented at the Columbian Exposition, illustrated with lantern views. (This will not be published).

The second session, held Wednesday evening, February 21st, was occupied with a lecture by W. H. Jaques, of South Bethlehem, Pa., on the Manufacture and Testing of Guns and Armor, illustrated with lantern views. (This will not be published.)

The third session was held Thursday morning, February 22d, when the following papers were read and discussed :

Close Sizing Before Jigging, by R. H. Richards, Boston, Mass.

The Torsional Theory of Joints, by George F. Becker, Washington, D. C.

Correlations in the Coal-Rocks West of Pocahontas, Flat Top, Virginia, by C. R. Boyd, Wytheville, Va.

The fourth session was held Thursday afternoon, February 22d, when the following papers were read and discussed :

Notes on the Re-working of Anthracite Culm-Banks, by Arthur W. Sheafer, Pottsville, Pa.

Geological Relations of the Southern Appalachian Bauxite-Deposits, by Dr. C. Willard Hayes, Washington, D. C.

The Bauxites: a Study of a New Mineralogical Family, by Francis Laur, Paris, France.

Artesian Well Prospects in Eastern Virginia, Maryland and Delaware, by N. H. Darton, Washington, D. C.

The remainder of the session was occupied with the continued discussion of the physics of steel, based upon papers presented at the Chicago meeting.

The following papers were presented in print, and opportunity was given for their discussion, but no oral discussion was had. (Such written communications in discussion of these or other papers as have been received by the Secretary will be found in the latter part of this volume, under " Discussions.")

Some Experiments for Determining the Refractoriness of Fire-Clays, by H. O. Hofman and C. D. Demond, Boston, Mass.

Product and Economical Results of the Marsac Refinery for the Year 1892, by C. A. Stetefeldt, Oakland, Cal.

Notes on the Unwatering of a Flooded Mine and on the Permeability of Natural Strata to Air, by Beverley S. Randolph, Frostburg, Md.

Ore-Washer at Longdale, Virginia, by Guy R. Johnson, Longdale, Va.

Survey of Underground Connection at Leavenworth, Kansas, by Edwin A. Sperry, Gothic, Col.

Refining of Gold Sulphides Produced by Precipitation of Gold from Chlorine or Bromine solution with Sulphurous Acid and Hydrogen Sulphide, by Werner Langguth, Portland, Oregon.

The Cerro de Pasco Mining Industry, by Otto F. Pfordte, Jersey City, N. J.

Solids Falling in a Medium.—I., by F. M. F. Cazin, Hoboken, N. J.

Note on Certain Magnetic Phenomena in Gold-Bearing Slates, by C. A. Mezger, Charlotte, N. C.

The Stetefeldt Furnace, by C. A. Stetefeldt, Oakland, Cal.

The Determination of Phosphorus in Coal and Coke, by Jacob Lychenheim, Swedeland, Pa.

The Franklinite Deposits of Mine Hill, Sussex County, New Jersey, by Frank L. Nason, New Brunswick, N. J.

Iron-Ores of East Texas, by William Kennedy, Austin, Tex.

Mine-Explosions Generated by Grahamite-Dust, by William Glenn, Baltimore, Md.

Further discussions of the Chicago papers of Messrs. Rickard, Christy, Posepny, Emmons and Potter were presented in manuscript, the writers being absent.

The fifth and closing session was held on Friday morning, February 23d.

The Secretary presented the report of the Scrutineers, showing the following officers to have been elected:

PRESIDENT.

JOHN FRITZ, ...	Bethlehem, Pa.

VICE-PRESIDENTS.

(To serve for two years.)

J. F. HOLLOWAY, ...	New York City.
J. C. PLATT, ...	Waterford, N. Y.
E. V. d'INVILLIERS, ...	Philadelphia, Pa.

MANAGERS.

(To serve for three years.)

T. A. Rickard,	Denver, Colo.
H. O. Hofman,	Boston, Mass.
John A. Church,	New York City.

TREASURER.

Theodore D. Rand,	Philadelphia, Pa.

SECRETARY.

Rossiter W. Raymond,	New York City.

The following paper was read and discussed:

Aluminum-Bronze, by Dr. Leonard Waldo.

The following paper was presented by President Howe, in the absence of the author:

Notes on Tests by Repeated Stresses, made at the Watertown Arsenal, by James E. Howard, Watertown, Mass. (Included as part of the discussion on the Physics of Steel, p. 776.)

The following papers were read by title:

Benjamin Huntsman, of Sheffield, the Inventor of Crucible-Steel, by R. A. Hadfield, Sheffield, England.

The Silver-Mines of Lake Valley, New Mexico, by Ellis Clark, Phœnix, Arizona.

The Zinc-Ore Deposits of Southwestern New Mexico, by W. P. Blake, New Haven, Conn.

Gold-Milling at the North Star Mine, Grass Valley, California, by Emile R. Abadie, Grass Valley, Cal.

The Allotropism of Gold, by Henry Louis, London, England.

The Manufacture of Open-Hearth Steel in Sweden, by Erik G. Odelstjerna, Filipstadt, Sweden.

A Convenient Still for the Laboratory, by Prof. C. E. Wait, Knoxville, Tenn.

Coal-Sections Developed by Recent Operations in Wise County, Virginia, by Franklin Bache, Abingdon, Va.

A Twelve-Mile Transmission of Power by Electricity, by Thomas H. Leggett, Bodie, Cal.

The Elk Garden and Upper Potomac Coal-Fields of West Virginia, by Jos. D. Weeks, Pittsburgh, Pa.

The annual report of the Council was presented, as follows:

Annual Report of the Council.

In accordance with the rules, the Council makes the following report to the Institute:

The financial statement of the Secretary and Treasurer shows receipts from all sources for the year ending February 1st (including $3463.88 on hand at the beginning of the year), of $26,293.86, and expenditures of $24,593.90, leaving a surplus of $1699.96, being a reduction in the surplus of February 1, 1893, of $1763.92. In addition to this, the Treasurer holds U. S. bonds of the par value of $2900 and a special deposit of $4598, proceeds of U. S. bonds called in and paid by the government, which fund has not been permanently re-invested. The detailed statement of receipts and expenditures is as follows:

Receipts.

Balance from statement, February 1, 1893,	$3,463 88
Annual dues,	20,010 47
Life-membership,	900 00
Sale of publications,	745 52
Binding of *Transactions*,	186 00
Authors' pamphlets,	117 80
Electrotypes,	59 75
Iron and Steel Institute (refund of expenses incurred for that Society),	220 61
Interest on U. S. bonds and deposits,	589 83
	$26,293 86

Disbursements.

Printing volume xxi. of *Transactions*,	$2,787 39
" pamphlet editions of papers,	3,845 01
" authors' editions,	152 75
" mailing list,	42 50
" envelopes, circulars, and ballots,	454 25
Binding volume xxi. of *Transactions*,	2,032 20
" exchanges and odd volumes,	102 09
Engraving and electrotyping,	1,902 29
Postage, including P. O. box-rent,	702 42
Stationery,	106 23
Rent,	800 00
Express and freight charges,	1,410 99
Janitor's fees,	104 00
Telegrams, cablegrams, and car fares,	22 06
Coal, ice, and porters,	78 96
Salaries, including clerks and stenographers,	9,266 37
Storage of *Transactions*,	154 88
Special stenographers and expenses at meeting,	359 25
Telephone,	151 00
Gas,	12 62
Miscellaneous expense, mailing pamphlets,	33 38
Office supplies and repairs,	73 26
	$24,593 90
Balance,	1,699 96
	$26,293 86

It will be seen that the actual expenditures of the year have exceeded the current receipts by $1763.92. The receipts of 1892 exceeded the expenditures $1744.99. The statement this year is, therefore, some $3500 less favorable than that of the year preceding. This has been the result of two general causes—a diminution in receipts, through delays and failures in the payment of dues, by reason of the severe conditions of the past year, and an increase of expenses necessitated by the participation of the Institute in the International Engineering Congress at Chicago. The accounts thus increased are principally those of the printing of pamphlet editions, engraving and electrotyping, salaries, and special stenographers, the aggregate of which is some $3400 larger than in 1892; but economies effected in other directions reduced the difference between the expenses of the two years to $2362. The extra expenses, due to the International Congress, will involve some $4000 for the preparation and distribution of the extra volume of *Transactions* (Volumes XXII. and XXIII., as has been already announced, are both to be issued during 1894). To meet this further expenditure, the Council has decided to withdraw for the present the offer of free binding to members paying their dues promptly. This will remove an item of expense which amounted last year to $2000; and if the members of the Institute will heartily support it by the prompt payment of their dues, it will be possible to finish the work undertaken for the credit of the country and the Institute, in connection with the Columbian Exposition, without drawing upon any other resources than the current receipts.

Upon this result the Council heartily congratulates the Institute. Coming so soon after the heavy outlay of 1890, which had well nigh exhausted the treasury, the request that the Institute should undertake the double task of managing two divisions of the International Engineering Congress was received by the Council with some anxiety; yet it was felt that, in view of the standing, representative character and history of the Institute, this honorable distinction, however burdensome, must be accepted.

As announced in the last annual report, this measure was accordingly decided upon, and the preparations for the important event were undertaken with vigor. The annual meeting, held in Montreal, February, 1893, was, however, not in the least diminished in attendance or interest by the prospect of the special assemblage at Chicago in August. The proceedings and papers of that very delightful Montreal meeting were published in Vol. XXI. of the

Transactions, in order to leave the following volume (or two volumes, if necessary) to be occupied wholly with the papers and discussions of the Chicago sessions. It is not necessary to inform the members of the Institute that the International Engineering Congress was highly important and interesting. More than a thousand engineers of all branches and from all countries were gathered in connection with it. The two volumes of our *Transactions*, containing papers and discussions of Divisions C and D, which constituted the double meeting of the Institute, will tell their own story concerning the professional value of their proceedings.

The circular of the Executive Committee of Engineering Societies, which will be found in the appendix to this report, gives a full account of the activity of that committee in connection with the International Engineering Headquarters and the Congress.

In addition to the participation of the Institute in these joint enterprises, a special Columbian Committee was appointed by the Council, consisting of Mr. John Birkinbine, Chairman; Dr. David T. Day, Secretary, and Messrs. Jos. D. Weeks, W. B. Potter, James A. Lewis, R. W. Hunt, Theodore D. Rand, J. C. Smock, Charles Kirchhoff and R. W. Raymond.

Before the appointment of this Committee, the following special circular was issued by order of the Council, to secure funds for the purposes named:

AMERICAN INSTITUTE OF MINING ENGINEERS.

SPECIAL CIRCULAR.

COLUMBIAN EXPOSITION FUND.

NEW YORK, March, 1892.

To Members and Associates.—An International Engineering Headquarters will be maintained in the City of Chicago during the Columbian Exposition of 1893. The estimated expense will be $15,000, which must be met by contributions from American engineers. The plan (based on that of the very successful Centennial Headquarters of this Institute in 1876) is admirable; the Committee in charge comprises two express representatives of the Institute and many of our members besides, and a hearty support is deserved.

The Council has also undertaken, at the request of the General Committee of Engineering Societies, to organize and conduct the Mining and Metallurgical Departments of the World's Engineering Congress of 1893 in Chicago.

For the above purposes, and for other contingencies which will undoubtedly arise in connection therewith, a considerable amount of money will be required. The Council desires to contribute for our members and associates $4000 towards the International Headquarters, and an equal sum, in addition, would not be too much for the proper maintenance of the credit of the Institute in many ways. But the fund of the Institute, derived from annual dues, is not available for any of these

purposes. Neither in connection with the Centennial of 1876, nor in the extensive hospitalities of the International meetings and excursions of 1890, nor at any other time has money been paid from that fund for entertainments, excursions or any expenses not belonging to the normal work of the Institute, conducted for the benefit of all its membership.

The Council, therefore, now earnestly appeals to members and associates for liberal subscriptions to a special Columbian Exposition fund, to be disbursed in the name of the Institute by the Council, or a committee of its creation, which shall make a suitable contribution for the International Headquarters, as above explained, and also, so far as the amount collected will permit, carry out such other measures as may be deemed advisable for the credit of the Institute, and the convenience and profit of its members and associates during the Exposition.

The sum of $5 is suggested as a minimum subscription, but it is hoped that all who can afford to give more largely will do so. Subscriptions of over $100 have been numerous on former occasions of this kind, and the present appeal is not less worthy of a liberal response.

Please send your subscription as early as possible, accompanied either with payment or with a statement of the time when payment will be made, to Mr. James F. Lewis, 23 Park Place, New York City, who has consented to act for the present as treasurer of this fund.

By order of the Council,

JOHN BIRKINBINE,
President.

R. W. RAYMOND,
Secretary.

In response to this circular, the amount of $4522.70 was subscribed and paid by members of the Institute, and of this sum $4000 was paid over to the Joint Committee of Associated Engineering Societies for the maintenance of the General Headquarters in Chicago, and $249.36 has thus far been expended by the special Columbian Committee of the Institute, mainly in connection with the Engineering Headquarters in the Mining Building at the Exposition, where Dr. Day, the Secretary of that Committee, was in constant attendance, and extended to members and guests of the Institute, as well as to members of affiliated American and foreign societies, hearty courtesy and valuable aid. These special headquarters, set apart by Hon. F. J. v. Skiff, Chief of the Department of Mines and Mining for the American Institute of Mining Engineers, were placed with Mr. Skiff's assent, and with the most cordial good-will on the part of the Council of the Institute, at the disposal of all members and guests of the Associated Engineering Societies, and were highly appreciated as a rendezvous for engineers of all countries. In addition to the pleasant balcony connected with them, they comprised a conversation-room, a reading-room and a valuable professional reference library of several thousand volumes.

To this library the Council willingly contributed by the loan of a complete set of the *Transactions*.

Changes in membership have taken place during the year as follows: 163 members and 18 associates have been elected; 4 associates have become members; 4 members have been reinstated on payment of arrears of dues; the deaths of 17 members and one associate have been reported; 26 members and 15 associates have resigned, and 60 members and 11 associates have been dropped for continued default in the payment of dues. These changes are tabulated as follows, showing a net gain of 55 in total membership:

	Hm.	Fm.	M.	A.	Totals.
At date of last report	15	38	2136	187	2376
Gains: By Election	.....	.....	163	18	181
Reinstatement	.....	.....	4	.....	4
Change of Status	.....	.....	4	.....	4
Losses: By Resignation	.....	.....	26	15	41
Dropping	.....	.....	60	11	71
Change of Status	.....	.....	.....	4	4
Death	.....	.....	17	1	18
Total gains	.....	.....	171	18	189
Total losses	.....	.....	103	31	134
Present membership	15	38	2204	174	2431

The list of deaths comprises the names of the following members and associates: George H. Babcock, H. M. Becher, G. B. Bell, A. F. Brainerd, J. H. Brunnings, George H. Ely, A. W. Humphreys, E. S. Moffat, F. O. Norton, J. D. Potts, E. A. Price, Frank Ringeling, G. Goodman Stone, W. J. B. Walker, J. H. E. Waters, Bard Wells, Arthur F. Wendt, Stephen Wilcox. Among these names are some of the most highly-esteemed contributors to the *Transactions* of the Institute, and some of the most eminent representatives, commercial as well as scientific, of the professions to which our members belong.

APPENDIX.

OFFICE OF THE
EXECUTIVE COMMITTEE OF ENGINEERING SOCIETIES,
COLUMBIAN EXPOSITION.

CHICAGO, ILL., January 20, 1894.

EXECUTIVE COMMITTEE—CIRCULAR No. 4.

To the President and Secretary of the American Institute of Mining Engineers:

The Executive Committee, appointed by the delegates of the fifteen Engineering Societies of the United States and Canada, which temporarily associated themselves

together for the purpose of maintaining Joint Engineering headquarters, and of promoting an Engineering Congress, in Chicago, during the Columbian Exposition of 1893, submit this their final report:

In accordance with the authority conferred by the General Committee at its last meeting, two other Societies, *i.e.*, "The American Water-Works Association" and the "Scandinavian Society of Civil Engineers," were admitted to this association on the basis of a contribution of $5 for each of their members.

In March, 1893, Mr. Max E. Schmidt was appointed Secretary, to take general charge of the headquarters. He gave part of his time to this work during April, his whole time during the period of the Exposition, and again part of his time afterwards until the final winding up of the affairs of the committee.

In April, identification cards were issued by the committee to each of the societies composing the association, in proportion to their respective quota of contribution, in order that by means of these cards each Secretary might accredit to the headquarters the members of his society, or such foreign visiting Engineers as he might desire to honor.

A general invitation was also issued in the name of all the associated societies, separately enumerated, through the president of the general committee, and sent to some 30 foreign Engineering Societies, inviting their members to avail themselves fully and freely of the facilities provided in Chicago, and also to make themselves known to the officers of the respective societies should they visit the cities in which these have their headquarters.

A large suite of rooms, comprising an entire floor, was secured for headquarters in the business portion of Chicago, at No. 10 Van Buren Street. These were leased from May 1 to November 1, 1893, at a rental of $900 for the six months.

This suite consisted of the following rooms:

1 on the 2d floor, a registry room, size 20 feet by 10 feet.
1 on the 3d floor, a reception room, size 50 feet by 25 feet.
1 on the 3d floor, a reading room, size 50 feet by 25 feet.
1 on the 3d floor, a committee room, size 26 feet by 24 feet.
1 on the 3d floor, a secretary's room, size 20 feet by 15 feet.
1 on the 3d floor, a cloak room, size 20 feet by 10 feet.
1 on the 3d floor, a writing room, size 24 feet by 24 feet.

These rooms proved to be exceedingly well located, being in close proximity to many of the prominent hotels, to the Art Palace building, in which the meetings of the Engineering Congress were held, and to the cable, elevated, and surface railroads, which furnished transportation to the Exposition, as well as to the dock of the Exposition steamboats.

Through the courtesy of the American Institute of Mining Engineers, a room 40 feet by 20 feet was also placed at the disposal of the committee in the gallery of the Mines and Mining building at the Exposition, by Mr. F. J. v. Skiff, Chief of the Mines and Mining Department.

Possession of both these headquarters was obtained on the 1st of May, 1893, but those at No. 10 Van Buren Street were in need of extensive remodelling, fitting up, and repairing, as well as tinting the walls and ceiling, so that they were not finally ready for occupancy until May 20, 1893.

Both headquarters were neatly furnished with reading and writing tables, desks, chairs, and lounges, at a total cost of $1352.41. This furniture was sold, in sundry lots, for $557.95 after the close of the Exposition.

In addition to Engineering reference books, albums, and circulars, these headquarters were provided with all the leading Engineering periodicals of the United

States and Canada, and most of those in Europe, through the voluntary contribution of their respective publishers, there being a total of some 89 journals and periodicals. On the walls were displayed photographs and engravings of sundry Engineering works and machines, kindly loaned for that purpose by various Engineering firms and establishments.

The staff employed was as follows:

1 Secretary in general charge.
1 General Assistant at 10 Van Buren Street.
1 General Assistant at Exposition.
3 Registry and Mail Clerks.
1 Stenographer.
2 Errand Boys.
1 Janitor.

The headquarters at No. 10 Van Buren Street were open on week-days from 8 A.M. to 10 P.M., and on Sundays from noon to 2 P.M.

The room in the Mines and Mining building at the Exposition was open from 8 A.M. to 6 P.M., except on Saturday, when it was open from 8 A.M. to 9.30 P.M. This room was closed on Sunday.

In engaging the staff, preference was given to Civil Engineers who could converse in other languages besides English.

As the rooms at No. 10 Van Buren Street were open 14 hours a day, the members of the staff relieved each other at stated intervals and on alternate days. Their general duties were to make accredited visitors welcome, and to register their Chicago address; to take up their identification card and to issue in its stead a card entitling the holder to the use of both Engineering headquarters and to attendance at the Engineering Congress; to care for and deliver the mail to visitors; to supply local information, and, upon request, to give cards of introduction to such Engineering works and establishments of the United States and Canada as either American or foreign visiting Engineers might desire to see.

For the latter purpose, a classified index of such works and establishments was compiled by the staff, and the Secretary was in correspondence with 878 different officers, firms, or establishments, and issued some 600 cards of introduction to the visiting Engineers.

The total number of Engineers who registered at the headquarters was 3132, divided as follows:

United States and Canada,	2408
Australia,	5
Austria-Hungary,	52
Belgium,	4
Chili,	1
Colombia,	1
East India,	3
France,	100
Germany,	382
Great Britain,	56
Holland,	3
Japan,	1
Italy,	28
Mexico,	5
Portugal,	1

Roumania,	1
Russia,	38
Spain,	2
Sweden,	41
Total,	3132

The total number of visiting Engineers was much in excess of this record, many coming in for a day or two and failing to register, more particularly during the week of the Engineering Congress, when the attendance was very large, and there was, necessarily, a good deal of haste among the visitors.

Allowing for Engineers who belonged to more than one society, the aggregate membership of the associated societies is estimated at about 6000, so that 40 per cent. of the members visited Chicago and were registered. The number of foreign visitors was rather less than one-third the number of Americans.

When the Exposition began fairly to draw visitors, informal gatherings took place at No. 10 Van Buren Street every Monday evening, from 8 to 11 P.M., at which light refreshments were served. The first took place June 29th, and the last on October 30th; making a total of 19 Monday night receptions. These were much appreciated, especially by the foreign Engineers, who thus had an opportunity to meet their American professional brethren, and of supplementing through personal intercourse the information which they had gathered at the Exposition and otherwise. Since their return home, a number of letters of thanks have been received, and more may be expected, copies of which will be duly communicated to the American societies.

At three of these receptions lectures were delivered.

August 28th.—"On Electric Light Plants in London," by Prof. Sylvanus B. Thompson (Stereopticon Views).

October 16th.—"On the Manufacture of Guns," by Captain W. H. Jaques, Ordnance Engineer (Stereopticon).

October 23d.—"On Photography in Colors," by Hugo Benedix, of France (Stereopticon Views).

During the week of the Engineering Congress a formal reception, to the visiting Engineers and their ladies, was given at No. 10 Van Buren Street on Tuesday, August 1st, followed at the close of the Congress by a general reception on Saturday, August 5th, both of which were largely attended, and at which refreshments were served.

A similar entertainment took place on Friday, September 15th, when a reception was tendered to the visiting delegation of the French Society of Civil Engineers.

The committee is informed that the Engineers were the only class who maintained headquarters in Chicago during the Exposition, and if it is to believe the assertions of visitors, foreign and native, it has the satisfaction of thinking that its efforts are well thought of by them.

Engineering Congress.

The International Engineering Congress was held in the Art Palace on the Lake Front, during the week beginning on Monday, July 31st. The aggregate attendance was estimated at about 800 members. This Congress was divided into seven sections, in which 221 Engineering papers were presented and discussed. In order to make this possible, almost all these papers had been printed and distributed in

advance by the organizations in charge of the respective divisions. The final publications, including the discussions, are now appearing issued by the "American Society of Civil Engineers," the "American Society of Mechanical Engineers," the "American Institute of Mining Engineers," the "Society for the Promotion of Engineering Education," the "Public Printing Office in Washington," and by the firm of "John Wiley & Sons," of New York.

This Engineering Congress took the place of the summer meeting of the three societies first above named.

The Executive Committee prepared a detailed programme of the papers, which was printed for the Congress by the "World's Congress Auxiliary," of the Columbian Exposition; it provided ushers and attendants, and furnished Stenographers for the Divisions of "Engineering Education," "Military Engineering," and "Marine and Naval Engineering," as these could not be provided by the "Auxiliary." It also furnished committee rooms at No. 10 Van Buren Street to the various divisions, and for that purpose rented the whole of the second floor of that building for the week of the Congress, thus nearly doubling the amount of available space.

The expenses charged direct to the Engineering Congress will be seen to be $571.22 upon the annexed financial statement, but if the planning, the correspondence and the incidental expenses be included, it may be estimated to have involved an expenditure of about $1000.

This International Engineering Congress has generally been considered a success. After its close a list in detail of the papers was compiled by the Secretary, in an "Announcement Relative to Publication," and the Executive Committee caused 10,500 copies to be printed at their expense and distributed pro rata among the various societies forming this association, for issue to their members; so that they might be advised where and at what cost the papers and discussions could be obtained.

The following financial statement is submitted by the Treasurer:

To Amounts Received From:

American Society of Civil Engineers,	$3,000	
American Institute of Mining Engineers,	4,000	
American Society of Mechanical Engineers,	2,000	
Canadian Society of Civil Engineers,	800	
Engineers' Club of Philadelphia,	850	
Boston Society of Civil Engineers,	550	
Engineers' Club of St. Louis,	350	
Civil Engineers' Club of Cleveland,	320	
Engineers' Club of Kansas City,	120	
Montana Society of Civil Engineers,	120	
Civil Engineers' Society of St. Paul,	60	
Engineers' Club of Minneapolis,	50	
Engineers' Association of the South,	200	
Engineers' Society of Western Pennsylvania,	650	
Western Society of Engineers,	672	
American Water-Works Association,	300	
Scandinavian Society of Engineers,	275	
M. N. Forney (gratuity),	25	
Total,		$14,342

By Amounts Paid:

Insurance on Furniture,	$8 60	
Express and Postage,	350 25	
Stationery and Printing,	476 35	
Expense Account Engineering Congress,	571 22	
Rent Account No. 10 Van Buren Street,	900 00	
Furniture and Fixtures,	794 46	
Fitting and Maintaining Headquarters,	1,612 76	
Receptions and Entertainments,	1,246 97	
General Expenses,	974 99	
Salary Account,	5,971 20	
Total Expenditure,		$12,906 80
Balance on hand,		1,435 20
		$14,342 00

Audited and found Correct.

C. L. STROBEL,
JOHN W. CLOUD,
Auditing Committee.

It will be noticed from this statement that there is an unexpended balance of $1435.20, and all bills having been paid, save the small amount required to print this circular, the Executive Committee has decided to return the surplus in its hands to the societies in the Association, in proportion to the amount contributed by each.

A dividend of 10 per cent. has therefore been declared, and a draft representing the share to which your society is entitled will accompany this report.

Respectfully submitted,

O. CHANUTE,
President General Committee.
ELMER L. CORTHELL,
Chairman.
CHARLES L. STROBEL,
ROBERT W. HUNT,
JOHN W. CLOUD,
DON J. WHITTEMORE,
WILLIAM FORSYTH,
Executive Committee.
MAX E. SCHMIDT,
Secretary General Committee.

After the passage of a resolution instructing the Secretary to make by letter suitable acknowledgment of the labors of the Local Committee and the abundant courtesies and hospitalities of the citizens and corporations of Norfolk and vicinity, the meeting was adjourned.

Members and Associates Elected.

The following persons were made members or associates by election during the sessions of the meeting:

Members.

R. B. Allen,	Harrisonburg, Va.
Theophilus Allen,	Harqua Hala, Arizona.
Charles Fitzhugh Andrews,	Isaacs Harbor, N. S.
Louis W. Atkinson,	Thurmond, West Virginia.
H. M. Barksdale,	Wilmington, Delaware.
James Brakes,	Crown Point, N. Y.
Ross E. Browne,	San Francisco, Cal.
Celso Capacci,	Florence, Italy.
Joseph E. Carne,	Sydney, N. S. W.
Edmund Clark,	Braddock, Pa.
W. E. Corey,	Munhall, Pa.
Kendall F. Crocker,	Whiteburn, N. S.
N. H. Darton,	Washington, D. C.
Harry J. Davis,	Munhall, Pa.
Henry G. Davis,	Baltimore, Md.
Charles D. Demond,	Great Falls, Montana.
J. M. Dodge,	Philadelphia, Pa.
George E. Drummond,	Montreal, Canada.
Thomas J. Drummond,	Montreal, Canada.
Howard Weidener DuBois,	Ishpeming, Mich.
William D. Ewart,	Chicago, Ill.
Aaron French,	Pittsburgh, Pa.
Harry F. Forbes,	Rockford, Ill.
H. G. Haskell,	Wilmington, Del.
George W. Hayes,	Lebanon, Pa.
Eric Hedburg,	Joplin, Mo.
George A. Hopkins,	Swissvale, Pa.
Edward L. Kurtz,	Newcastle, Pa.
William Lamb,	Norfolk, Va.
Harry K. Landis,	Rolla, Mo.
Nevin McConnell,	Munhall, Pa.
Vance C. McCormick,	Harrisburg, Pa.
William McKell,	Glen Jean, West Virginia.
Reinhard Mannesmann,	New York City.
James Campbell Macdonald,	Country Harbor, N. S.
Francis Herbert Mason,	Truro, N. S.
Galloway C. Morris,	Philadelphia, Pa.
Frank Moynau,	So. Chicago, Ill.
Frank Owen,	London, England.
Amedee N. Reyburn,	St. Louis, Mo.
Francis Arthur Rich,	Denver, Colorado.
Maurice Riché,	Ocala, Florida.
James Riddle,	Norfolk, Va.
Edward T. Sawyer,	Columbus, Ohio.
Charles C. Sharp,	Corning, Ohio.
Homer Reed Stanford,	Buffalo, N. Y.
Julius H. Striedinger,	San Francisco, Cal.

Benjamin Talbot,	Pencoyd, Pa.
W. H. Thomas,	Algoma, W. Virginia.
Joseph R. Wilson,	Philadelphia, Pa.

Associates.

Edmund P. Allison,	Halifax, N. S.
N. L. Burwell,	Chicago, Ill.
Frederick Willis Davis,	New York City.
Pemberton Smith,	Albany, N. Y.
George A. Waller,	Freiberg, Germany.
Peter White,	Marquette, Michigan.

Associates made Members.

N. H. Emmons 2d,	Charcas, Mexico.
Arthur L. Tuttle,	St. Louis, Mo.

Excursions and Entertainments.

On Tuesday, as already noticed, the visiting members and guests were entertained at breakfast in Norfolk by the Chamber of Commerce, and subsequently were conveyed in carriages through the city to view both its new and its old features of interest and beauty.

Wednesday was occupied in a highly delightful excursion to Lambert's Point, Newport News, Hampton and Fort Monroe. The party left Norfolk in the steamer "Luray," placed at their disposal by the Old Dominion Steamship Company. At Lambert's Point, the extensive terminal facilities of the Norfolk and Western Railroad were inspected. At Newport News, after visiting the magnificent yard of the Newport News Ship-building Company, and viewing the great Atlantic liner "New York" in the dry-dock, the party, some two hundred in number, was entertained at luncheon at the Hotel Warwick by the Company. An hour of great interest was subsequently spent at Hampton Institute, where the singing of the vast assembly of students produced upon the visitors a thrilling effect, never to be forgotten. The excursion closed with a visit to Fort Monroe, where a special parade of troops was held in honor of the Institute, and the party returned by steamer to Norfolk and by special train to Virginia Beach.

On Friday afternoon an excursion was made to the Portsmouth Navy Yard, where the party was courteously entertained by Rear-Admiral Brown, commanding.

On Saturday, a small party, including half a dozen ladies, braved the threatening weather, and made an excursion to the Dismal Swamp, as the guests of Mr. Walter B. Brooks, Jr., President of the Dismal Swamp Canal Company.

Free transportation between Norfolk and Virginia Beach was furnished by the Norfolk, Albemarle and Atlantic Railroad.

Special rates and excellent accommodations were given by the Bay-Line steamers from Baltimore, and the Washington and Norfolk line from Washington, and by the management of the Princess Anne Hotel, the beautiful situation and pleasant interior of which added greatly to the social enjoyment of the meeting.

On Thursday evening a banquet was held at the Princess Anne, with the usual accompaniments of music and oratory, and graced by the presence of numerous guests from Norfolk, who returned to their homes by special train in the small hours of the morning.

The attendance of ladies accompanying members was unusually large at this meeting, and, as a consequence of this fact, and of the constant association of all under one roof, the social enjoyment of the meeting was a special feature throughout. Indeed, no one of the formal excursions or entertainments was more pleasurable than the friendly gatherings at all hours in the superb "sea-parlor" or before the great fire-places of the Princess Anne.

Registry.

The following names were registered during the meeting. It is probable that the list is not complete. The sessions and excursions were numerously attended by ladies accompanying members and by citizens of Norfolk and vicinity.

W. H. Adams.
Robert Allison.
Charles Bailey.
G. F. Becker.
W. E. Bibbs.
C. P. Bonner.
C. R. Boyd.
D. F. Brooke.
W. T. Brooke.
J. W. Cabot.
Charles Carroll.
W. B. Coggswell.
H. M. Cornish.
D. Jones Crain.
N. H. Darton.
G. M. Davidson.
David T. Day.
George Dean.
C. W. Ellicott.
S. F. Emmons.
W. E. C. Eustis.
W. D. Ewart.
James Finlay.
Lewis W. Francis.
W. S. Gaylor.
John C. George.
M. Goldsmith.
John Graham.
A. Hardt.
Joseph Hartshorne.
H. G. Haskell.
J. A. Haskell.
C. W. Hayes.
T. W. Henderson.
E. M. Henry.
G. C. Hewett.
H. D. Hibbard.
L. Holbrook.
Jed. Hotchkiss.
Henry M. Howe.

W. C. Hungerford.
A. E. Hunt.
Thomas G. Janvier.
W. H. Jaques.
J. E. Johnson.
Charles Kirchhoff
I. W. Kirchhoff.
Clarence Kirwan.
H. C. Knox.
E. K. Landis.
R. G. Leckie.
J. Henry Lee.
G. R. Lendeman.
H. McCormick, Jr.
Graham Macfarlane
B. M. Moore.
H. B. Nason.
George Ormrod.
E. W. Parker.
James Platt.
J. C. Platt.
T. H. Phillips.
S. M. Pitman.
R. W. Raymond
John Reed.
R. H. Richards.
James Riddle.
W. S. Russell.
W. T. Ryle.
Axel Sahlin.
Walter Sharpe.
A. N. Sheafer.
W. L. Sheafer.
Pemberton Smith.
R. T. Smith.
H. R. Stanford.
W. J. Taylor.
Gray Torrey.
H. G. Torrey.
E. Russell Tratman.
J. A. Turner.
J. W. Tyson.
L. Waldo.
C. D. Walcott.
A. L. Walker.
Russell Walker.
W. R. Webster.
G. D. Whitaker.
T. Whitaker.
John Wilkes.
W. G. Wilkins.
B. P. Willis.

PROCEEDINGS OF THE SIXTY-SEVENTH MEETING, BRIDGEPORT, CONN., OCTOBER, 1894.

Local Committees.

Bridgeport Reception Committee.—Dr. I. DeVer Warner, *Chairman;* Morris B. Beardsley, *Vice-Chairman;* James Staples, *Treasurer;* Dr. F. M. Wilson, *Secretary;* Henry Sanford, Dr. George L. Porter, Dr. C. S. Ward, Goodwin Stoddard, A. J. Hobbs, Rev. William H. Lewis, W. E. Baillie, Sydney Bishop, Floyd Tucker, John C. Matthews, Col. N. H. Heft, Joel Farist, Benj. Fletcher, J. H. Jenkins, E. G. Burnham, Henry Setzer, Jr., J. B. Hubbell, O. G. Jennings, Martin Luscomb, Hon. William D. Bishop, Henry Atwater, Charles Sherwood, Isaac Holden, E. G. Burnham, Thomas B. DeForest, David Trubee, Charles A. Hamilton, Capt. John McNeil, W. F. Hobbs, Dr. Leonard Waldo, Geo. W. Beach, Wm. G. Lineburgh, Nelson M. Beach, Charles L. Hill, F. A. Bartlett, David F. Read, O. M. Shepard, Mayor W. B. Bostwick, Hon. Robert E. DeForest, Dr. C. C. Godfrey, S. T. Rogers, Rev. Dr. C. R. Palmer, Charles H. Sanford, Tracy B. Warren, R. B. Lacey, Gen. Thomas L. Watson, Erwin S. Strickland, Geo. C. Edwards, Geo. W. Longstaff, Wm. Somers, Col. L. N. VanKeuren, Frank M. Wilson, F. W. Bolande, Chas. E. Brown, Rev. Louis N. Booth, and the Governors of the Seaside Club.

Waterbury Reception Committee.—Hon. E. C. Lewis, *Chairman;* Geo. H. Clowes, C. P. Goss, H. L. Wade, Jas. S. Elton, D. S. Plume, A. M. Dickinson, H. S. Chase, M. L. Sperry, Geo. H. Benham, Irving H. Chase, Leroy Upson, Geo. Hart, E. M. Burrall, Chas. A. Hamilton, Geo. L. White, Chas. S. Morse, A. S. Chase, E. D. Steele, E. L. Frisbie, Jr., J. Richard Smith, Thos. B. Kent, Gordon W. Burnham, E. O. Goss, W. S. Atwood, Chas. Cotter, Geo. B. Lamb, Archer J. Smith, O. H. Stevens, T. R. Hyde, R. F. Griggs, Hon. F. J. Kingsbury, L. J. Atwood, H. H. Peck, E. A. Smith, John P. Elton, F. L. Curtiss, W. E. Fulton, F. L. Adams, F. H. Hoffmann, Geo. A. Driggs, A. O. Jennings, J. Hobart Bronson, Chas. M. Mitchell, D. N. Plume, Irving L. Atwood.

New Haven Reception Committee.—Hon. H. B. Sargent, Mayor of New Haven; Charles P. Clark; C. H. Platt; Geo. J. Brush, Director of the Sheffield Scientific School.

Ansonia Reception Committee.—William H. Matthews, *Chairman;* Russel A. Cowles, Charles A. Cowles, Charles F. Bliss, William A. Cowles, Frank E. Hoadley.

Hotel Headquarters.—The George Hotel, Black Rock Harbor. All sessions were held at the hotel.

The opening session was held on Tuesday evening, October 2d.

Dr. I. DeVer Warner, Chairman of the Bridgeport Reception Committee, called the meeting to order, and, after a few words of cordial welcome, introduced Dr. George L. Porter, of Bridgeport, who delivered an address (see p. 609).

President John Fritz, after acknowledging the welcome extended to the Institute, proceeded to deliver the Presidential address.

The Secretary mentioned the recent death of J. H. Bramwell, one of the original members of the Institute, and Mr. E. C. Pechin, of Cleveland, O., being called upon, paid an earnest and eloquent tribute to the memory of Mr. Bramwell.

The session was then adjourned.

The second session was held on Wednesday morning, October 3d, at 10 o'clock.

The following papers were presented by the Secretary in the absence of the authors:

The Inaccuracy of the Commercial Assay for Silver and of Metallurgical Statistics in Silver Mills, with Special Reference to the Treatment of Roasted Ores by Amalgamation and by the Russell Process, by C. A. Stetefeldt, Oakland, Cal.

Losses of Gold and Silver in the Fire-Assay, by Howard V. Furman, Denver, Colo.

Further Experiments on Amorphous Gold, by Henry Louis, London, Eng.

A paper on A Uniform Method for the Assay of Copper-Material for Gold and Silver, by Albert R. Ledoux, New York City, was presented by the author, and followed by general discussion on the subject.

The Secretary presented samples of wire-silver with calcite deposited upon it from Gothic, Colo., and of monazite sand from North Carolina.

After discussion on Coal-Dust as an Explosive Agent, in connection with the paper of Mr. Glenn, read at the Virginia Beach meeting, and later communications thereon, the session was adjourned.

The third session was held on Wednesday afternoon, October 3d, at 3 o'clock.

The following paper was read and discussed:

The New Mining Law of New York, by R. W. Raymond, New York City.

The subject of The Effect of Vibration upon the Stems of Stamps was brought up by request and discussed at considerable length.

The following papers were presented in print by the Secretary, in the absence of their authors:

The Geological Structure of the Ringwood Iron-Mines, New Jersey, by Frank L. Nason, New Brunswick, N. J.

Notes on the Structure of the Franklinite and Zinc-Ore Beds of Sussex County, New Jersey, by W. P. Blake, Shullsburg, Wis.

The Ore-Deposits of Butte City, by Robert G. Brown, Butte, Montana.

The Structure of the Richmond Coal-Basin, by E. J. Schmitz, New York City.

The Phosphates of Tennessee, by Thomas C. Meadows and Lytle Brown, Nashville, Tenn.

Alunogen and Bauxite of New Mexico, by W. P. Blake, Shullsburg, Wis.

The Manganese-Slags of Tombstone, Arizona, by John A. Church, New York City.

Ore-Dressing and Concentration in Sweden, by P. G. Lidner, New York City.

Solids Falling in a Medium.—II., by F. M. F. Cazin, Hoboken, N. J.

A Note on the Taylor Gas-Producer Plant, at the Ontario Mill, by C. A. Stetefeldt, Oakland, Cal.

Magnesia and Sulphur in Blast-Furnace Cinder, by Frank Firmstone, Easton, Pa.

Also, contributions in additional discussion of former papers, as follows:

On the Genesis of Ore-Deposits.

On the Taylor Gas-Producer.

On the Gold Stamp-Mill.

The fourth and concluding session was held on Thursday evening, October 4th.

Mr. John Birkinbine presented for inspection a piece of Carpenter steel, a piece of brown hematite-ore containing apparently native iron, and an Aztec mirror, said by the former owner to be meteoric iron, but now believed to be marcasite.

The following paper was read by the author and discussed at considerable length:

The Nickel-Mine at Lancaster Gap, Pennsylvania, and the Pyrrhotite Deposits at Anthony's Nose, on the Hudson, by J. F. Kemp, New York City.

The following papers were read by title:

The Lead- and Zinc-Deposits of Missouri, by Arthur Winslow, St. Louis, Mo.

The Mines of the Chalanches, France, by T. A. Rickard, Rico, Colo.

After the unanimous adoption of a resolution instructing the Secretary to make by letter suitable acknowledgment of the labors of the Local Committee and the abundant courtesy and hospitality of the citizens and corporations of Bridgeport, Waterbury, Ansonia and New Haven, the meeting was adjourned.

Members and Associates Elected.

The following persons were elected by mail, May, 1894:

MEMBERS.

Audley H. Ackerman,	Marysville, Mont.
Thomas Andrew,	Zoutpansberg, Transvaal.
William Braden,	Helena, Mont.
A. I. Boucher,	Johannesburg, South Africa.
Prof. Walker Bowman,	Athens, O.
William J. Chalmers,	Chicago, Ill.
Herbert A. Evans,	Honda, Colombia.
Donald G. Forbes,	Shillingstone, Dorset, Eng.
L. D. Godshall,	Rico, Colo.
Fred. E. Harnden,	Pueblo, Colo.
John Herron,	Marysville, Mont.
Edward R. Hewitt,	New York City.
Arthur E. Ingram,	Zoutpansberg, Transvaal.
Edward Knight,	South Chicago, Ill.
Reuben W. Leonard,	Kingston, Ontario.
J. C. McDowell,	Pittsburgh, Pa.
P. D. Mackay,	Wilkinsburg, Pa.
Alex. Montgomery,	Launceston, Tasmania.
Philip A. Mosman,	Durango, Colo.
Francis C. Phillips,	Allegheny, Pa.
J. C. Roberts,	Rico, Colo.
Henry W. Sabin,	San Francisco, Cal.
William A. Sherrill,	Auraria, Ga.
John Harcourt Smith,	Zeehan, Tasmania.
Audley H. Stow,	Bramwell, W. Va.
E. J. Wilson,	Rico, Colo.
Horace G. Williams,	Lykens, Pa.
J. W. Young,	Helena, Mont.

ASSOCIATES.

Edward Burd Grubb,	Sheridan, Pa.
J. Elwood Jones,	New York City.
Edmund Mozier King,	Launceston, Tasmania.
John Musson,	Sydney, New South Wales.
Percy Williams,	Cleveland, O.

ASSOCIATE MADE MEMBER.

Charles A. Bohn,	Calchihuites, Zacatecas, Mexico.

The following persons were made members or associates upon the recommendation of the Council during the sessions of the meeting.

MEMBERS.

James W. Abbott, ... Ouray, Colo.
H. H. Ashley, ... Wilkes-Barre, Pa.
L. S. Austin, ... Durango, Colo.
Frederic Reeve Baker, ... Athens, O.
Eugene Bavington, ... Pierce City, Mo.
Arthur K. Bohn, ... Marysville, Mont.
Charles P. Bower, ... Philadelphia, Pa.
William Gaston Caperton, ... Fire Creek, W. Va.
John J. Case, ... Great Falls, Mont.
Howard F. Chappell, ... Chicago, Ill.
E. H. A. Cohen, ... Transvaal, South Africa.
William Ashton Doble, ... San Francisco, Cal.
Joseph D. Fraser, ... Ferrona, Nova Scotia.
James W. Fuller, Jr., ... Catasauqua, Pa.
William Kennedy Gibson, . . . Dayton, Tenn.
William Shafer Hall, ... Easton, Pa.
William P. Hardenbergh, ... Newark, N. J.
Gustaf A. Hellberg, ... Norway, Mich.
George R. Hurlburt, ... Ouray, Colo.
Cary T. Hutchinson, ... New York City.
J. Moore Jackson, ... Clarksburg, W. Va.
L. C. Jakway, ... Durango, Colo.
Alfred James, ... Glasgow, Scotland.
Charles James, ... Philadelphia, Pa.
J. Elmer Jones, ... Hazleton, Pa.
Kwong Yung Kwang, ... Kaiping Coal-Mine, No. China.
Carlos F. de Landero, ... Hidalgo, Mexico.
George Hamilton Lloyd, ... Rhayader, Radnorshire, Wales.
John G. Luckraft, ... Cripple Creek, Colo.
Charles F. McKenna, ... New York City.
John H. McLure, ... Thurber, Texas.
A. C. Mauney, ... Georgeville, N. C.
J. E. Ostrander, ... Moscow, Idaho.
Louis Pelatan, ... Paris, France.
William H. Peirce, ... Baltimore, Md.
Lucius Pitkin, ... New York City.
Myron Knox Rogers, ... Butte, Mont.
W. J. Scott, ... Wilkes-Barre, Pa.
Edward M. Shepard, ... Springfield, Mo.
Francis H. Sistermans, ... Rosario, Sinaloa, Mexico.
John Law Smith, ... West Hartlepool, Eng.
Erwin S. Sperry, ... Bridgeport, Conn.
James Spiers, Jr., ... San Francisco, Cal.
H. Starkweather, ... Silverton, Colo.
Andrew Stevens, ... Rico, Colo.
William H. Storms, ... San Francisco, Cal.
Ralph Hayes Sweetser, ... Boston, Mass.
William Ulenberg, ... Lead, South Dakota.
Hamilton Walker, ... Butte, Mont.
J. N. Walker, ... Silverton, Colo.
E. A. Weinberg, ... Alderslot, Marysborough, Queensland.
Arthur W. B. Whiteley, ... Biwabic, Minn.

ASSOCIATES.

H. P. McIntosh,	Cleveland, O.
Rea E. Maynard,	Stamford University, Cal.
William Arthur Metcalf,	Steelville, Mo.
Thomas Moore,	Elizabeth, N. J.
Lena Allen Stoiber,	Silverton, Colo.
Bertrand S. Summers,	Chicago, Ill.
W. Wittle,	Rico, Colo.

ASSOCIATES MADE MEMBERS.

Frank C. Hamilton,	Brooklyn, N. Y.
Percy Williams,	Rico, Colo.

Excursions and Entertainments.

On Wednesday evening, October 3d, at the beautiful home of the Seaside Club, in Bridgeport, a brilliant social reception was given to the Institute and its guests by the Governors of the Seaside Club in co-operation with the Bridgeport Reception Committee.

Thursday, October 4th, was devoted to an excursion through the Naugatuck valley in a special train provided by the courtesy of the New York, New Haven and Hartford Railroad Company. Leaving Bridgeport at 8.45 A.M., the train stopped an hour and a half at Ansonia, where the rolling-mills of the Ansonia Brass and Copper Company were visited. At 11.15 A.M., the party reached Waterbury, and was conveyed to the armory of the Ninth Connecticut Regiment, where luncheon was served, with accompanying music and oratory, after which separate smaller parties were organized for visiting respectively the mills of Benedict & Burnham, Booth & Hayden, Randolph & Clowes, the Furrill foundry, the Scovil Manufacturing Company, the Waterbury Brass Company, the Waterbury Watch Company and other establishments. At 4 P.M., the special train left Waterbury, and the party arrived at Bridgeport an hour later, after a delightful day.

Friday, October 5th, was spent in visiting, under the guidance of the Local Reception Committee, numerous points of interest in Bridgeport, including the Seaside Park, the Wheeler & Wilson Sewing Machine Company's works, the Holmes & Edwards Silver Company, the Barnum Institute, the Bridgeport Chain Company, the Bridgeport Brass Company's works, the Seaside Institute and the Warner Brothers corset factory, the Farist steel works, Eaton, Cole & Burnham's works and the Sella Collection of Mountain Photographs on view at the Public Library. Luncheon was served

at the beautiful Seaside Institute, established in connection with the factory of Warner Brothers for the benefit of their employees.

In the afternoon, a water excursion took place in the harbor of Bridgeport, and in the evening a "shore dinner" was given to the Institute and other invited guests by the Bridgeport Reception Committee at the George Hotel. This dinner, consisting almost exclusively of sea-food, and followed by toasts and speeches, was highly enjoyed by all.

On Saturday, October 6th, a special train conveyed the members and guests of the Institute to New Haven, where they were received by the President and Faculty of Yale University and conducted through various buildings of the university and of the Sheffield Scientific School, after which they enjoyed, at the invitation of the Mayor and Council of New Haven, a drive through the city and to the magnificent East Rock, which constitutes the unsurpassed attraction of the suburbs. The perfect autumn weather gave to this memorable experience the last touch of perfection and made it a fit climax for the pleasures of the week.

Registry.

The following persons registered their names at the hotel-headquarters. There were also many ladies accompanying members:

Robert Allison.
M. Barnett.
F. J. Batchelder.
John Birkinbine.
Theodore Blake.
J. Parke Channing.
F. L. Clerc.
Edgar S. Cook.
W. P. Darling.
David T. Day.
W. F. Durfee.
B. F. Fackenthal, Jr.
John Fritz.
A. P. Gaines.
William Glenn.
E. S. Godfrey.
M. L. Griffin.
A. L. Haasis.
Frank C. Hamilton.
Anton Hardt.
E A. Hardt.
H. G. Haskell.
Edward R. Hewitt.
George H. Hewitt.
James Hillhouse.
Thomas Hobson.
L. Holbrook.
J. F. Holloway.
H. M. Howe.
W. S. Hungerford.
E. V. d'Invilliers.
Arthur W. Jenks.
J. F. Kemp.
William Kent.
C. Kirchhoff.
G. F. Knapp.
H. H. Knox.
W. B. Kunhardt.
E. D. Leavitt.
Albert R. Ledoux.
Arthur Lee.
J. F. Lewis.
John Macdonough.
Charles Morrison.
H. B. Nason.
E. E. Olcott.
E. W. Parker.
Edmund C. Pechin.

William H. Peirce.
E. D. Peters, Jr.
S. M. Pitman.
J. C. Platt.
William Ramsay.
R. W. Raymond.
Ellen H. Richards.
T. Robins, Jr.
R. P. Rothwell.
N. W. Shed.
W. H. Shockley.
L. Bennett Smith.
Oberlin Smith.
T. Guilford Smith.
E. G. Spilsbury.
E. R. Toedt.
H. G. Torrey.
J. Gray Torrey.
E. E. R. Tratman.
James W. Tyson.
Leonard Waldo.
John R. Walker.
W. R. Webster.
W. H. Wiley.
John Wilkes.
Oliver Williams.

PAPERS.

THE STETEFELDT FURNACE.

BY C. A. STETEFELDT, OAKLAND, CAL.

(Virginia Beach Meeting, February, 1894.)

It may be considered somewhat strange that I should present to the Institute, at this date, a paper on the Stetefeldt furnace, which was first introduced for actual mill-work at Reno, Nevada, in 1869. What induces me to take this step is the fact that many misconceptions still exist about this furnace. Comparatively few professional metallurgists have had experience with it; and it has been mostly under the management of the "muscular amalgamator." Besides, the plans and a description of its latest construction have never been published. The literature on the Stetefeldt furnace in the *Transactions* is incomplete and rather limited, being confined to the following papers: In *Trans.*, viii., 554, there is a description of the construction and operation of the old Stetefeldt furnace at the Ontario mill, Utah, by R. P. Rothwell. In *Trans.*, xiv., 341, the present writer discusses the comparative loss of silver in roasting Ontario ore in the Howell and Stetefeldt furnaces; also the comparative loss of silver and gold in roasting Lexington ore in a Stetefeldt furnace at Butte, Montana. R. H. Terhune records in *Trans.*, xvi., 21, the results of experiments in oxidizing-roasting of pyritiferous galena, and of iron matte containing lead, in a Stetefeldt furnace. In the same volume, p. 473, Ellsworth Daggett compares favorably the work done by the Stetefeldt furnace with that of the other roasting-furnaces. The last reference to the Stetefeldt furnace in the *Transactions* is that of James Douglas in his paper, "Summary of American Improvements and Inventions, etc." (*Trans.*, xxii., 328), read at the Chicago meeting, August, 1893, to the discussion of which I have contributed some observations (*Trans.*, xxii., 659)

In the papers of W. S. Morse (*Trans.*, xxi., 919) and myself (*Trans.*, xxiii., 134) on the use of producer-gas for drying and roasting ore, the former read at Montreal, February, 1893, and the latter at Chicago, August, 1893, the latest statistics on the consumption of fuel for roasting in the Stetefeldt furnace are found. Many articles in

reference to it have appeared in the *Eng. and Min. Journal*, and some independent pamphlets on the subject are now out of print.

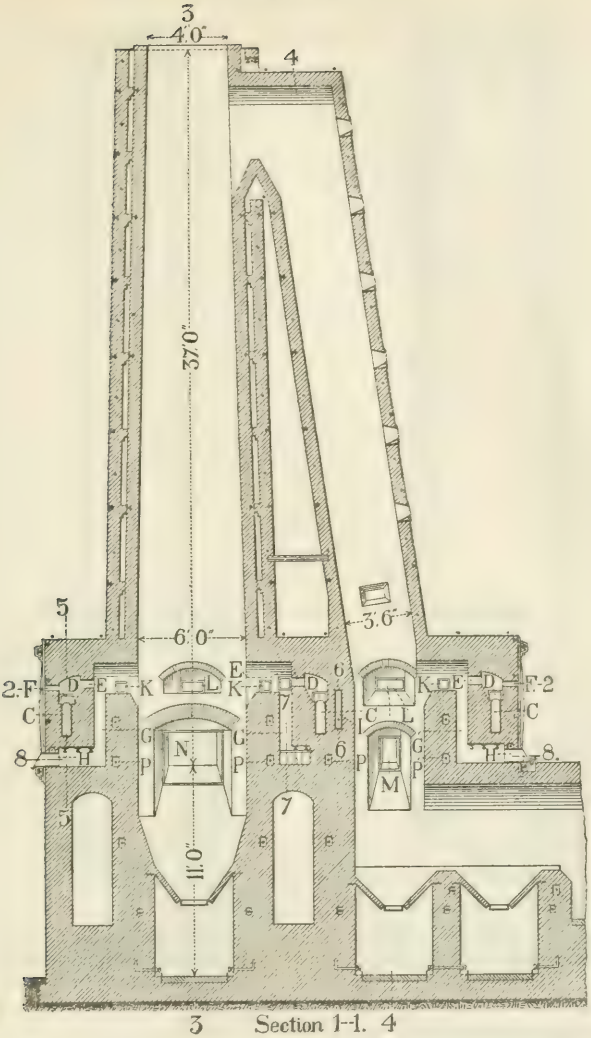
In a paper entitled, "A Review of the Russell Process," read before the Colorado Scientific Society, in May, 1893, L. D. Godshall draws some questionable general conclusions from a limited experience with the Stetefeldt furnace at the Holden mill, Aspen, Colorado; and this paper will be made the starting-point for combating several false notions about the furnace.

Construction of the Stetefeldt Furnace.—The latest improvement in construction consists in the application of producer-gas, generated in a Taylor gas-producer. So far as the use of gas-fuel, generally speaking, is concerned, it had already been applied in the Manhattan furnace, Austin, Nevada, in 1870. But in that case each of the three fire-places had an independent gas-generator, built close to the furnace and forming a part of its structure, in which charcoal or a mixture of charcoal and wood was used. In consequence of the excellent results obtained with this system at Austin, furnaces of the same construction were put up at Mineral Hill, Belmont, and Secret Cañon, all in Nevada. The author, however, soon discovered that such complicated apparatus was not safe in the hands of the average "muscular amalgamator," and returned to building furnaces with ordinary fire-places for wood. At Belmont an explosion destroyed the arches of the dust-chambers, the foreman of the mill having neglected to light the gas after starting the generator. At Secret Cañon a quicksilver-bottle, filled with water, and fastened to a hemp-rope, was used as a counter-weight for the cover of one of the generators. In lifting the cover the rope broke, and the quicksilver-bottle dropped into the generator-shaft. The fireman did not consider that this might have serious consequences. After a while a terrific explosion shattered the walls of the generator, but fortunately did no other damage

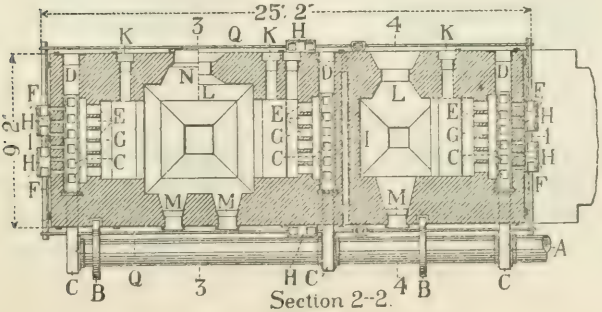
It is evident that in a furnace receiving fresh supplies of ore continuously, and performing the roasting in a few seconds, a uniform temperature must lead to better results, economically as well as from a metallurgical standpoint.

Plates I. and II., in which the principal dimensions are specifically given, represent the latest design of the Stetefeldt furnace, as built at the Holden mill, Aspen, Colorado.

The spiral-weld steel-tube, A, provided with expansion-joints, B, supplies gas from a Taylor gas producer to the cast-iron burners, C, which have openings with covers in front for cleaning out soot and

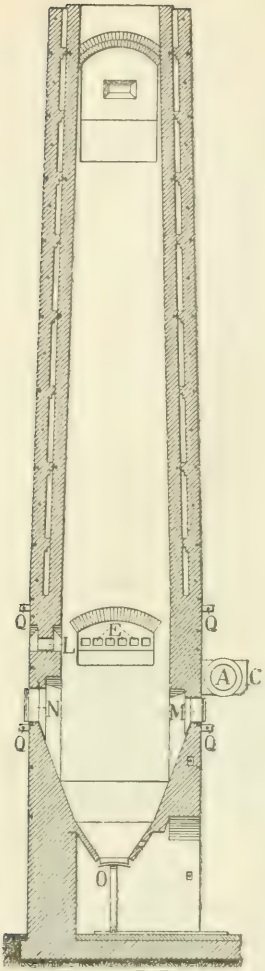


Section 1-1.

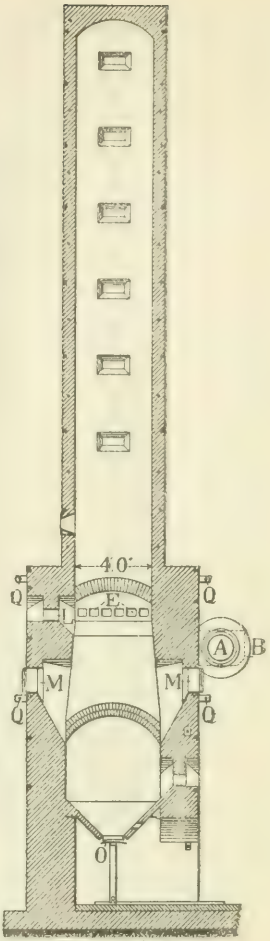


Section 2-2.

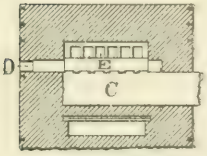
Sections of the Stetefeldt Furnace.



Section 3-3.

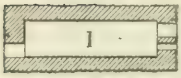


Section 4-4.



Section 5-5.

Section 7-7.



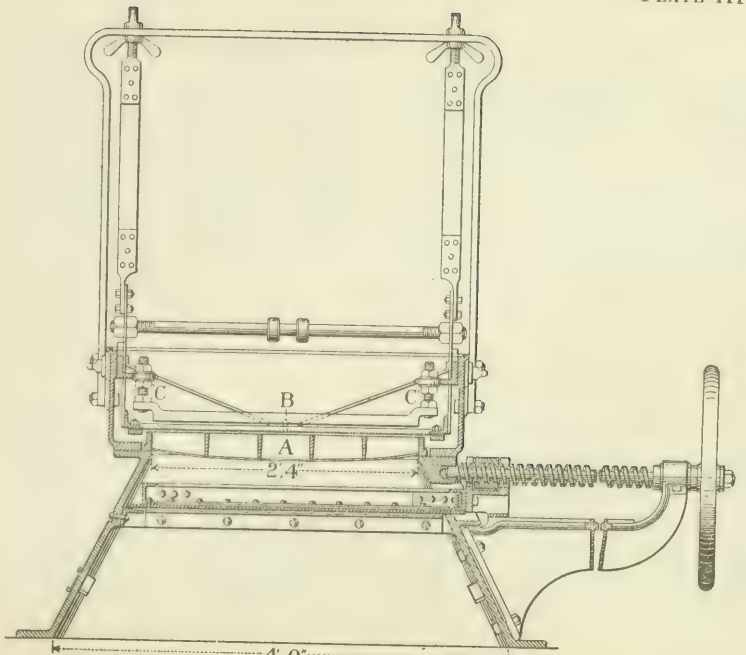
Section 6-6.

Section 8-8.

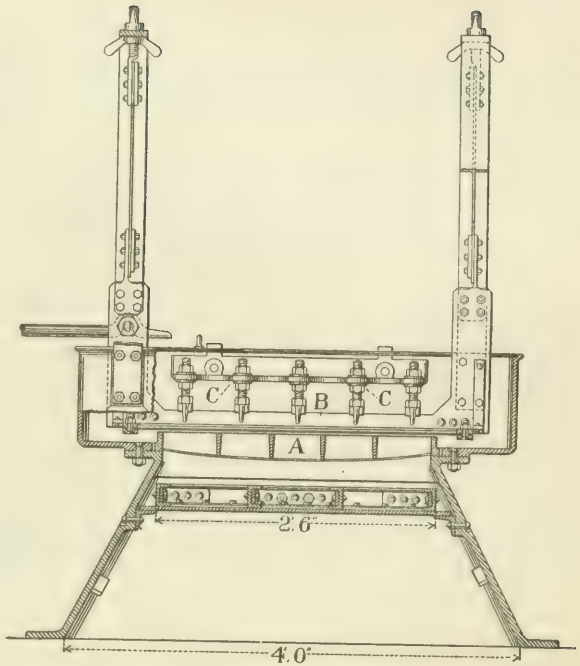
Sections of the Stetefeldt Furnace.

tar. The gas escapes from the burners through square holes on the top. These can be more or less closed by cast-iron slides (not shown in the drawing), the handles of which move through the covers of the frames, D, thus regulating the supply of gas. The latter escapes into the combustion-chambers through the openings, E. Should soot accumulate here it can be removed through the pipes, F, ordinarily closed by iron plugs. Air for combustion ascends through the channels, G, and the air-supply is regulated by sheet-iron slides at the air-doors, H. The air-channel, I, in the wall between shaft and flue, is provided to prevent over-heating of the gas-burner. The fire-bridges can be cleaned through the openings, K, and also through the doors, L. Air can be supplied below the fire-bridges through the doors, M, and the big door, N, on the shaft is used for cleaning the hopper. For discharging the roasted ore from the hoppers into cars underneath, the swinging dampers, O, are pushed back by handles running in guides, which are not shown in the drawing. It will be noticed that many rails (30 pounds per yard) are inserted in the walls of the furnace. Anchor-holes, P, running through hot walls, are made two courses of brick high, 3 inches wide inside and wider at the faces of the walls, so that they are not closed by the perpendicular buck-straps. The long walls of the lower part of the furnace are anchored outside by the rods, Q. All walls are braced by wrought-iron buck-straps between anchor-rods. Where ordinary bricks of good quality can be obtained, a lining of fire-brick is only necessary in the lower part of the shaft and flue, commencing about 4 feet above the fire-bridges in the shaft and at the fire-place in the flue. Other details of the furnace do not require description.

The Stetefeldt feeder, shown in Plate III., consists of a cast-iron grate, A, covered by a punched steel-sheet with holes of one-eighth of an inch or less diameter, through the openings of which the ore-pulp, mixed with salt, is fed by the oscillating motion of the wrought-iron rocker, B. A coarse wire-screen, No. 4, is stretched over the frame of the rocker. The necessary friction is created by the stationary blades, C. The distance between the punched-screen and the wire-screen, and between the latter and the lower edges of the blades, can be regulated, as also the number of strokes the rocker makes per minute, whereby it becomes possible to feed into the furnace, with the same machine, almost any desired quantity of ore. The coarser and warmer the pulp, the easier works the feeder. A damper, moved by a screw, is inserted below the grate, A, whenever



Longitudinal Section.



Transverse Section.

The Stetefeldt Feeder.

it becomes necessary to exchange worn-out screens. This is also done when the feeding of ore is stopped for any length of time.

The dust-chambers, of which drawings and a description were published in the *Eng. and Min. Journal* of July 28, 1883, have not been changed in construction. The Stetefeldt feeder has also remained the same, except that the rocker with coarse wire-screen, which was formerly supported by friction-rollers, is now suspended by adjustable, flexible steel-bands. A change in the frame of the feeder was made many years ago. Originally it was in the form of a water-jacket. This caused a serious accident at Mineral Hill, Nev. The wise chief engineer of the mill had provided the influx as well as the discharge water-pipe of the jacket with cocks. Some thoughtless fellow had shut both cocks, and when the furnace was fired up, after a clean-up of the mill, the fireman noticed some steam issuing from the water-jacket. As he stepped near, an explosion took place, and, blinded by dust, the man staggered forward and dropped into the hot shaft. Although taken out alive, he died shortly afterwards. From that time on the water-jacket was discarded, and it proved to have been superfluous.

Application of the Stetefeldt Furnace.—The Stetefeldt furnace was especially invented and designed for the chloridizing-roasting of silver-ores, necessary for subsequent beneficiation by amalgamation or lixiviation; and in this field it has been, and is now, largely, successfully and exclusively used. Whether it can roast to advantage pyritic ores preparatory to smelting in reverberatory or blast-furnaces has never been proved or disproved, the smelters persistently refusing to try the experiment on account of the expense, because such a furnace must be built on a large scale. That silver-ores carrying a very large percentage of pyritic mineral can be well chloridized in a Stetefeldt furnace has never been claimed, because this cannot be done directly in a reverberatory-furnace either. In the latter furnace an oxidizing-roasting of ores high in sulphur always precedes the admixture of salt. Hence, if such ores are to be chloridized in a Stetefeldt furnace, they should be first partly desulphurized either by heap-roasting or by roasting in a Gerstenhöfer or Hasenclever furnace. The ore from the Sombrerete mine, Mexico, was only successfully chloridized in a Stetefeldt furnace after it had been subjected to a somewhat crude and hasty heap-roasting.

Mr. Godshall, in his paper cited above, "based on the actual experience of nearly a year as superintendent of the Holden S. & M. Co. at Aspen, Colorado," says:

"The use of this furnace has certain limits, beyond which it is unwise to go. Siliceous ores carrying 6 per cent. or more sulphur can be chloridized better in some other furnace. Ores carrying a large percentage of lime—say, from 15 to 20 per cent. of CaO—are also very difficult to chloridize properly in the Stetefeldt furnace, unless sufficient sulphur is present to combine with the CaO and form $CaSO_4$; even then the chloridization in the furnace is frequently very low and rarely exceeds 60 per cent.

"The writer has seen the furnace deliver roasted ore from certain mixtures high in lime and sulphur where not more than 15 to 20 per cent. of the silver had been chloridized in the shaft of the furnace, where about 65 per cent. of the ore is roasted. However, after the above ore had been lying on the cooling-floor for three days, fully 90 per cent. of the silver was found to be chloridized. The extremely low percentage of the silver converted into chloride in the furnace was due partly to its having been crowded beyond its capacity, but chiefly to the large percentage of sulphur present causing such a strong reducing atmosphere of sulphur dioxide that the effect of the chlorine liberated by the acid gases was neutralized. Frequently the odor of sulphurous acid escaping from the roasted ore as it was discharged from the shaft of the furnace was sufficiently strong to overcome all smell of chloride fumes. The sulphur in the raw charge of the ore just considered ranged from 8 to 10 per cent., being about half that of the lime and magnesia present."

And a little further on:

"Can such ores be roasted better with any other furnace? Metallurgically, the answer is, Yes. The proof of the above assertion with regard to Aspen ores has been demonstrated by the writer by roasting in a reverberatory-furnace ten lots of ore containing 25 per cent. of CaO (MgO not determined, but probably amounting to 10 or 12 per cent.) with less than 2 per cent. of sulphur, and using, practically, the same amount of salt as in the Stetefeldt furnace. The chloridization was all that could be desired."

And finally, in his conclusions, he says:

"For ores containing from 3 to 8 per cent. of sulphur, the Brueckner, Pearce or Howell-White are to be recommended."

It does not seem out of place to combat such unwarranted limitations of the use of the Stetefeldt furnace.

Mr. Godshall, however, is rather generous in admitting ores with less than 6 per cent. of sulphur, which limit, however, he reduces finally to 3 per cent. Some metallurgical writers have gone so far as to contend that only oxidized or so-called "chloride-ores" were fit for treatment in this furnace, because they happened to see the furnace in operation on such material.

I should say here that the Aspen ore to which Mr. Godshall refers is exceptional and peculiar in its character. The gangue is composed of quartz, heavy-spar and dolomite; pyritic minerals are either en-

tirely absent or else occur only in small quantities. Which of the two statements is correct, I do not know, having never seen specimens of the ore. The analysis given by Mr. Morse, and also by Mr. Godshall, refers not to the Aspen ore pure and simple, but to the ore-pulp sent to the furnace, *i.e.*, to Aspen ore plus the addition of pyritic ores. An average pulp-sample from over 30,000 tons of ore contained as follows: SiO_2, 21.7; $BaSO_4$, 20.9; CaO, 11.0; MgO, 4.2; Fe, 10.0; Zn, 2.9; Pb, 2.3; Cu, 0.16; S, 8.1 per cent. Whether all the CaO and MgO are combined with carbonic acid, or partly with silica, is not stated.

Now, in the first place, it must be acknowledged that the Aspen ore is a difficult ore to chloridize in the Stetefeldt as well as in any other furnace; and while it would be injudicious to draw general conclusions, as Mr. Godshall does, from a single and short experience, under any circumstances, it is pre-eminently so in this instance. From the fact that the sulphur in the Aspen ore exceeds Godshall's limit by 2 to 4 per cent., according to his first statement, and from 5 to 7, according to his second one, one would be forced to the conclusion that the Stetefeldt furnace at the Holden mill was a complete failure. Mr. Morse's statistics contradict this. For instance, during four months, from August to November, 1892, the silver left in the tailings, after extraction by the Russell process, was 10 per cent. of the silver in the ore, which ranged in value from 21 to 27 ounces per ton, and the sulphur in undecomposed sulphurets had been reduced to 0.2 per cent. Would any other furnace have done better "metallurgically?" Mr. Godshall gives no definite reply to this question; he only says, that the reverberatory-furnace, with only $\frac{1}{4}$ of pyritic ores added, and the same amount of salt, gave a chlorination that "was all that could be desired." But even granting that, "metallurgically," any other furnace would produce better results than the Stetefeldt, would this be true "economically," especially with ores averaging only 24 ounces of silver per ton? Metallurgically, we can accomplish anything; but the metallurgist who leaves the question of economy out of sight is a dead failure. What would be the cost of a reverberatory-furnace plant to treat 90 tons of Aspen ore per day? How much labor would be needed? Could a ton of ore be roasted in a reverberatory-furnace at an expenditure of 18 cents (Mr. Morse's official figure) for fuel?

I could quote many examples of roasting in the Stetefeldt furnace, with results of high chlorination, of ores containing far more sulphur than Mr. Godshall's limit of less than 6 per cent., or less than 3 per

cent. (which of the two limits is the correct one his readers must decide for themselves); but, unfortunately, no exact sulphur determinations were made. I can give, however, exact analytical figures on base ore from the 500-foot level of the Ontario mine. The analyses, to which further reference will be made later, were executed by Mr. Falkenau, of San Francisco. From Falkenau's analysis the following mineralogical composition of the ore was calculated:

15.00	per cent.	zinc-blende,	containing	6.0	per cent. of the silver in the ore.
7.60	"	galena,	"	5.8	
4.55	"	fahlore,	"	88.2	
3.50	"	pyrites of iron,	"	Trace.	
69.35	"	gangue, composed of quartz and clay.			

The contents in sulphur, which alone interest us here, were 7.68 per cent. In samples of roasted ore from the shaft, taken immediately after discharging, 81.3 per cent. of the silver was chloridized, and the sulphur in undecomposed sulphurets was 0.18 per cent.

In samples of roasted ore from the flue, taken immediately after discharging, 82.2 per cent. of the silver was chloridized, and the sulphur in undecomposed sulphurets was only 0.064 per cent. It is hardly necessary to add that the chlorinations rose high into the nineties after the ore had remained in heaps for 24 hours on the cooling-floor.

From this it appears that the Stetefeldt furnace is by no means such a poor desulphurizer as the majority of metallurgists are inclined to assume, *i.e.*, when proper conditions are maintained and the capacity of the furnace is not overtaxed, as was the case in the experiment. About 35 tons of ore were roasted in twenty-four hours. I call especial attention to the fact that the larger part of the sulphur in this ore was combined with zinc and lead.

Mr. A. F. Wendt brought, in 1890, a large quantity of silver-ore from the Huanchaca mine, Bolivia, to the United States for experimental purposes. An analysis of the ore is not at my disposal, especially not a determination of its percentage in sulphur; but judging from appearances it must have contained not less than 30 and up to 40 per cent. of sulphur. It consisted of a solid mass of pyritic minerals, principally pyrites, zinc-blende, and galena. My services having been engaged to roast this ore chloridizing in the Stetefeldt furnace at the Ontario mill, I pronounced it at once unsuited for such treatment without previous heap-roasting, to which it was well adapted. The ore was, however, roasted directly in the

Stetefeldt furnace, and some interesting results will be given later on. Huanchaca ore was also roasted in Brückner furnaces in Colorado, and at the Boston and Montana works in Butte, and in the Stetefeldt furnace at the Lexington mill. Upon samples of roasted ore from the latter places, analytical investigations were made by Mr. Falkenau.

A charge of $5\frac{1}{2}$ tons of Huanchaca ore, roasted oxidizing in a large Brückner furnace, still contained after twenty hours 16.4 per cent. of sulphur in sulphurets. In another charge, 9.1 per cent. of sulphur in sulphurets was found after sixty hours of roasting. A charge roasted first oxidizing, and then with salt (time of roasting not given), contained 10.7 per cent. of sulphur in undecomposed sulphurets.

A lot of Huanchaca ore was first partially desulphurized in a Brückner furnace (how much sulphur was left not stated or investigated, but evidently a large amount. The sample is also described as being coarsely crushed but without giving size of screen), and then roasted chloridizing in the Stetefeldt furnace at the Lexington mill. In the roasted ore the following percentages of sulphur were found in undecomposed sulphurets: Coarse ore from shaft, 10.4 per cent.; ore from flue below auxiliary fire-place, 3.2 per cent.; ore from dust-chambers, 1.3 per cent. Chlorination-tests of these samples were not made by Mr. Falkenau, and I have no information about them.

These imperfect figures show that the Stetefeldt furnace, even in roasting such ores, is a fair desulphurizer as compared with the slow work of the Brückner furnace. Results of roasting pyritic ores oxidizing in the Stetefeldt furnace would, no doubt, be materially improved if the furnace were constructed especially for this purpose, and care were taken not to overtax its capacity.

The Chlorination of Base Ores on the Cooling-Floor.—It is well known that high chlorinations of silver-ores containing a large percentage of pyritic minerals can only be obtained after roasting in a Stetefeldt furnace by leaving the discharged ore for twenty-four hours, or longer, in heaps on the cooling-floor. The same applies to the Howell furnace. But why should this be made an objection? This chloridizing in heaps costs nothing except interest on the capital invested in a larger cooling-floor. The incomplete desulphurization and chlorination of a base ore, immediately after discharging from a Stetefeldt furnace, shows itself by the pungent smell of sulphurous and sulphuric acid, and by the presence of a large percentage of magnetic oxide of iron (which can be extracted by a magnet),

provided the ore was rich in pyrites of iron. After sufficient exposure on the cooling-floor the magnetic oxide disappears either almost entirely or is at least greatly reduced in quantity. Thus, for instance, base ore roasted in the shaft of the Lexington furnace contained 17.5 per cent. of magnetic oxide immediately after discharging, and only 2.3 per cent. after having remained for twelve hours on the cooling-floor. The chlorination of the silver increased during this time from 47.7 to 90.8 per cent.

The effect of heap-roasting on the cooling-floor can be best studied by quoting observations made on roasting Huanchaca ore in the Ontario mill.

In one experiment three tons of this ore were crushed in a battery of ten stamps through a No. 40 screen at the rate of 1 ton per hour, and fed into the furnace with 18.5 per cent. of salt. The ore, after discharging, emitted such a strong smell that it was scarcely possible to approach the pile for thorough sampling. A quickly cooled sample from the shaft looked perfectly black, and 30 per cent. of magnetic oxide of iron was extracted from it. Ore from the flue contained only 10.5 per cent. of magnetic oxide. The discharged ore was piled up about 2 feet high, keeping the products from the shaft and flue separate. After twenty hours a porous crust, from 5 to 8 inches thick, had formed on the heaps, and this was removed. The crusts and the insides of the heaps were now sampled. The changes which had taken place are recorded in the following tables:

Roasted Ore from Shaft.

	Soluble Salts. Per cent.	Chlorinations of the Silver. Per cent.
Immediately after discharging	22.2	18.1
Inside of heap	19.7	21.8
Crust	39.8	70.9

Roasted Ore from Flue.

	Soluble Salts. Per cent.	Chlorinations of the Silver. Per cent.
Immediately after discharging 1st flue-hopper	15.9	14.3
" " " 2d and 3d "	26.6	59.2
Inside of heap	22.5	53.2
Crust	29.2	89.2

The distribution of ore in the furnace was about as follows: 68 per cent. in shaft; 20 per cent. in first flue-hopper; 2.5 per cent. in second and third flue-hoppers; and the rest in dust-chambers. The collection in the second and third flue-hoppers is always slight.

As already observed, the samples taken, immediately after discharging, were not exact averages.

If the ore had been left on the cooling-floor in large heaps for three days, instead of twenty hours, undoubtedly much higher chlorinations would have been obtained. It is to be regretted that sulphur-determinations were not made. The increase in the percentage of soluble salts in the crusts is very notable.

In conclusion, I give the remarkable results obtained in amalgamating the roasted Huanchaca ore in iron pans:

Amalgamation of Huanchaca Ore.

Charge.	Chlorination of car-sample. Per cent.	Silver extracted by Amalgamation. Per cent.
1.....	38.8	75.9
2.....	36.7	80.0
3.....	33.7	81.3
4.....	33.7	89.0

The value of the roasted ore, after leaching out soluble salts, was from 118 to 123 ounces of silver per ton.

The last and highest result was obtained by adding 4 pounds of blue-stone to the pan-charge of 3000 pounds.

The retorted bullion was .814 fine in silver.

From this, it would appear as if a high chlorination was more detrimental than beneficial to the amalgamation of Huanchaca ore; the highest results having been obtained with ore of the lowest chlorination. I have no doubt that the addition of more blue-stone than 4 pounds to the pan-charge would have increased the extraction materially; but the roasted ore had all been amalgamated before the beneficial effect of the blue-stone was discovered.

The question is pertinent. How is the chlorination of the silver effected in heap-roasting? Mr. Godshall discusses this question as follows:

"While on the cooling-floor, the ore undergoes a heap chloridizing-roasting. Inasmuch as the supply of free oxygen is extremely limited, and repeated tests failed

to show any presence of sulphate of iron, it would seem to indicate that sulphurous acid might, under certain conditions, have the power of liberating the chlorine from the salt, with formation of sulphite of soda. The writer freely admits that he is ignorant of the exact chemical reaction which occurs, under conditions of heat and exclusion of air, between sulphurous acid and sodium chloride."

It is somewhat surprising that such an able chemist as Mr. Godshall should advance such a poor theory, which, as he admits, he cannot himself explain.

In the first place, it is not true that the supply of air to the ore is excluded, or extremely limited. The crust on the surface of the heap is extremely porous, and so is the ore inside of the heap. This can be easily ascertained by filling a cupel-mould with the roasted ore and pressing it down with the piston. While the access of the air is slow, it is, practically, unlimited if sufficient time is allowed. That Mr. Godshall failed to find any sulphate of iron counts for nothing, the temperature being too high inside of the heap for the formation and existence of this salt.

It seems to me, that the theory of this heap-roasting is very simple. The lower oxides (and their salts) of copper, manganese, and iron, easily combine, and their higher oxides easily part with oxygen under certain conditions, thus acting as transferrers of oxygen to other compounds which do not combine with it directly. This is beautifully illustrated by the conversion of sulphurous into sulphuric acid in the Roessler converter. In heap-roasting a similar reaction takes place. Here, the oxygen of the air, at first, slowly attacks the sulphur in undecomposed sulphurets and forms sulphurous acid. The latter, remaining in contact with the higher oxides of copper, manganese, and iron, is converted into sulphuric acid, which decomposes the salt and sets chlorine free. That the lower oxides again combine with oxygen to higher ones is proved by the conversion of the magnetic oxide to ferric oxide inside of the heap after sufficient time has elapsed. As in the Roessler converter, the reducing and oxidizing reactions, producing sulphuric acid, go on simultaneously; but they cease when all the undecomposed sulphurets have disappeared, or the temperature has become too low. If the access of air were excluded, none of these reactions could take place in heap-roasting. With the reactions effecting the chlorination of the silver we need not concern ourselves here. I have observed that, other conditions being the same, silver-ores containing a fair percentage of copper can always be more highly chloridized than those in which copper is absent, or occurs only in very slight quantity.

This is due to the fact that copper is the most energetic transferrer of oxygen, of the three metals named, in the formation of sulphuric acid.

The Constitution of the Ore After Roasting in the Stetefeldt Furnace.—Samples of roasted Ontario ore (the mineralogical composition of the raw ore has been given above) taken immediately after discharging from the shaft and flue, were subjected to a partial chemical analysis with the following results:

Roasted Ore from the Shaft.

	Per cent.
Cupric and cuprous chloride,	0.25
Zinc chloride,	1.38
Aluminum chloride,	1.51
Sodium chloride,	3.68
Traces of other chlorides.	
Lead sulphate,	3.26
Aluminum sulphate,	0.56
Sodium sulphate,	4.62
Traces of other sulphates.	
Remainder: Metallic oxides and gangue.	

Roasted Ore from the Flue.

	Per cent.
Aluminum chloride,	1.07
Sodium chloride,	3.08
Traces of other chlorides.	
Copper sulphate,	0.74
Zinc sulphate,	1.48
Aluminum sulphate,	2.88
Lead sulphate,	5.18
Sodium sulphate,	10.01
Traces of other sulphates.	
Remainder: Metallic oxide and gangue.	

The separation of the soluble metallic chlorides and sulphates was effected by alcohol, in which the chlorides are soluble, and the sulphates are not. After treatment with alcohol the soluble sulphates were extracted by water.

It appears from this that in the shaft copper and zinc are only present as chlorides, and in the flue only as sulphates. The sulphates of lead and aluminum predominate in the flue, and aluminum chloride predominates in the shaft. Sodium sulphate also predominates in the flue. Whether this holds good generally with ores roasted chloridizing in the Stetefeldt furnace I do not know, such investigation not having been carried out elsewhere.

Fine Crushing.—Those who have had no experience with the Stetefeldt furnace are still under the impression that for chloridizing-roasting the ore must be pulverized exceedingly fine, finer than for any other furnace. This is absolutely erroneous. If silver-ores are to be roasted in a Stetefeldt furnace, to prepare them for extraction by amalgamation or lixiviation, they do not require finer crushing than for roasting in Brückner, Howell, or reverberatory-furnaces.

In all cases alike the required fineness of crushing (I refer here to crushing by stamps exclusively) depends on the character of the ore, *i.e.*, whether the silver-bearing minerals are finely impregnated in the gangue or not. In amalgamation the coarseness of crushing is limited by the capacity of the settlers to work off coarse sand; and this limit is a 26-mesh wire-screen. For lixiviation there is no limit of coarseness, and the most advantageous size of the screen is entirely dependent on the character of the ore. The Ontario ore is very interesting in this respect. Its principal silver-bearing mineral is fahlore (or its products by decomposition), and this never impregnates the quartz, but occurs in little bunches, and frequently on the cleavages of zinc-blende and galena. The latter minerals, if pure, are comparatively low in silver. How little coarse crushing affects the roasting of Ontario ore in the Stetefeldt furnace is shown by the following table:

Mesh of Screen on Battery.	Tons crushed in 24 hours. Per stamp.	Rate at which the St. fur. was fed in 24 hours. Tons.	Salt used. Per cent.	Silver extracted by Russell Process. Per cent.
20	2¾	55	12.5	97.0
16	3½	70	12	97.1
10	4½	94	14	93.4
6	6⅓	126	8	91.9

I do not know whether the extraction by the Russell process refers to actual clean-up of sulphides on ordinary mill-assays (which are too low), or to "apparent" extraction, *i.e.*, extraction calculated from the value of the roasted ore, after leaving out soluble salts, and the tailings. This, however, does not influence the main point at issue, the experiments having all been conducted alike.

The lower extraction in the last two experiments may have been due more to overtaxing the capacity of the furnace than to coarse crushing. In the last experiment the lower percentage of salt may

have influenced the result. Ontario ore, however, is very exceptional in this respect, and there may not be many ores that could be roasted successfully for lixiviation after crushing through a No. 6 battery-screen.

The Volatilization of Silver by Chloridizing-Roasting in the Stetefeldt Furnace.—Mr. Godshall discusses, in his paper quoted above, the causes for the loss of silver by volatilization in roasting, but does not say anything new on the subject. The evaporation or volatilization of all substances is governed by the same general laws. The effective elements are: Time, temperature, surface exposed, character of the atmosphere in which evaporation or volatilization takes place, density or pressure of the latter, and its motion or exchange in relation to the substance evaporated or volatilized. Thus, for instance, more silver is volatilized in roasting a small ore-sample in a muffle than in actual reverberatory-furnace work, because more surface is exposed, and the particles have more contact with air in the former case.

In regard to the Stetefeldt furnace, Mr. Godshall says:

"Mr. Stetefeldt claims that the volatilization of silver is principally a function of time. If the above statement is true, it is one of the strongest arguments in favor of the Stetefeldt furnace. If it can be shown that with only momentary exposure of the ore, as is the case in the Stetefeldt furnace, the percentage of silver volatilized is as high or nearly as high as when the ore is roasted for eight hours with the same quantity of salt in a reverberatory-furnace, it must be concluded that there are other influences more important than time governing such loss. This has been found to be the case with Aspen ores. The average loss by volatilization determined in roasting some twenty-ton lots of ore in a reverberatory-furnace was found to be less than that experienced at Aspen, where the Stetefeldt furnace is used."

I do not find fault with Mr. Godshall's general argument, but with his method of stating the facts. He does not even state the difference in the loss of silver. What he should have given is the loss in each furnace, with full explanations how the results were obtained. This is not like giving the results of a chemical analysis which is based upon well known and acknowledged methods of investigation. Mr. Godshall's dogmatic statement fails to convince.

It is easy enough to determine the silver in some twenty-ton lots of ore before and after roasting in a reverberatory-furnace; but when it comes to an exact and reliable investigation of this kind concerning results in the Stetefeldt furnace, the difficulties are many, and the sources of error are not easily eliminated. Or if Mr. Godshall made use of an indirect method, as I did in ascertaining the relative

loss of silver by volatilization in the Howell and Stetefeldt furnaces at the Ontario mill (see my paper, previously referred to), he should have stated this in detail.

Regarding the Stetefeldt furnace, a loss of silver caused by dust-chambers of insufficient capacity, should not be confounded with loss by volatilization.

At the Ontario mill the dust-chambers are connected with the chimneys by long flues ascending the side of a steep hill. The flues are about 150 feet long, 4 feet wide, and 6 feet high, with several perpendicular steps where large quantities of dust collect. These flues are cleaned once a year, and the dust taken out represents in weight about 1.2 per cent. of the total ore sent to the furnaces, and contains about 1.26 per cent. of its silver. If these flues were absent, and the dust now gathered in them had escaped into the air through the chimneys, could this be charged to "loss by volatilization?"

At Aspen the chimney of the Stetefeldt furnace stands close to the last dust-chamber. The furnace roasts, in twenty-four hours, more than double the quantity of ore going through the Ontario furnace; the Aspen ore contains between three and four times as much sulphur as the average Ontario ore going to the mill. (The richest and the basest Ontario ores are sold to smelters.) Hence, the supply of air to the Aspen furnace must be at least six times as great as that to the Ontario furnace. Finally, the ore is crushed at Aspen through a 30-mesh screen; at the Ontario mill, through a 26-mesh screen. Mr. Godshall says that the ore at Aspen is crushed so fine that from 85 to 90 per cent. of the battery-pulp passes through a 150-mesh screen. This, I suppose, is due to the peculiar nature of the gangue of the Aspen ore. Would it be strange then if, under the circumstances, 6 per cent. or more dust left the last dust-chamber of the Aspen furnace and was lost by passing out through the chimney? But could the silver lost in this way be properly charged to "loss by volatilization?"

Recently Mr. Morse has put up a plant for collecting this dust, or the "volatilized silver," as he calls it. Of its construction I am ignorant, as well as of the results obtained. Mr. Godshall's experiments refer to the time when this dust-plant was not in operation.

In conclusion, I must remark that the actual loss of silver by volatilization in the Stetefeldt furnaces that were running formerly and are in operation now has never been accurately ascertained. Ordinary mill-statistics are utterly worthless for this purpose. None of the mills possess accurate weights of the ore taken to the furnace

and of the ore after roasting. Besides, the ordinary mill-assays are not correct, being altogether too low.

The fact that the loss of silver by volatilization during chloridizing-roasting in the Stetefeldt furnace is a minimum, *under all circumstances*, as compared with roasting in any other furnace, lacks absolute proof. On the other hand, Mr. Godshall has not made out a clear case that even with Aspen ores this loss is less in the reverberatory-furnace than in the Stetefeldt.

I refrain from republishing the chlorinations obtained in roasting silver-ores in the Stetefeldt furnace at numerous mills.

NOTES ON THE UNWATERING OF A FLOODED MINE, AND ON THE PERMEABILITY OF NATURAL STRATA TO AIR.

BY BEVERLEY S. RANDOLPH, FROSTBURG, MD.

(Virginia Beach Meeting, February, 1894.)

THE coal-measures of George's Creek coal-region, in Maryland, lie in a comparatively flat synclinal basin, about 4 miles wide. The dip of the measures is, in places near the edge of the basin, as much as 20 feet in 100. The Pittsburgh seam, known locally as the "Big Vein," is the only seam worked at the present time.

Ocean No. 3, one of the large mines of the Consolidation Coal Company, has its opening on the outcrop of the Pittsburgh seam, in a ravine which cuts into this seam to such an extent as to bring this opening to within a mile of the axis of the basin.

After working all the coal to the rise of this point, the mine was further developed by extending a slope down the dip of the seam, making such an angle with the line of greatest dip as to give a grade varying from 1 to 10 per cent., down which the empty cars carry a rope from a single-drum winding-engine. The coal was worked from each side of this slope in successive lifts during its progress.

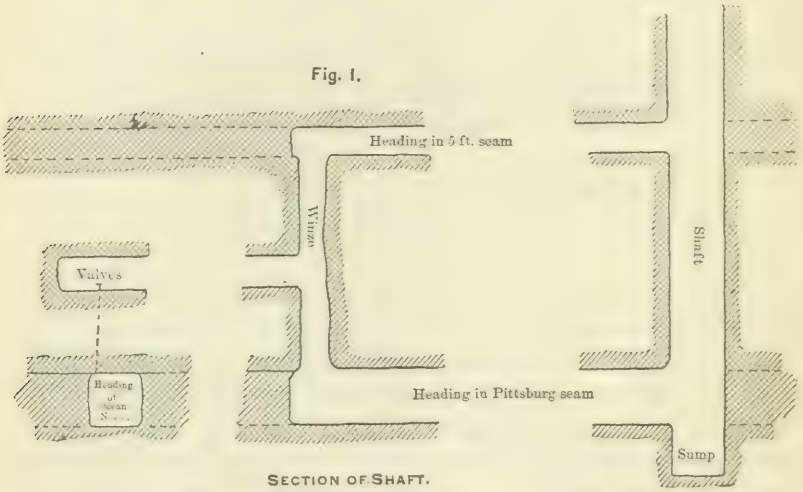
From the top of the slope, for a distance of 4000 feet to the fourth lift, the "make" of water is drained by gravity through a heading into the adjoining mine. Below this point the water was pumped into this same heading, at the fourth lift, from which point, as before mentioned, it flowed by gravity.

During the month of March, 1891, the pumps used for unwatering this portion of the mine were overpowered by an unexpected rush of water, and these lower workings were temporarily abandoned and allowed to fill.

It was then determined to sink a shaft to reach the seam near the foot of the slope, in order to install a large pumping-plant, with boilers, at the top of the shaft.

Owing to the hilly nature of the country, the most available point for this shaft proved to be near George's Creek, at a distance of about 800 feet from the nearest heading of Ocean No. 3. The depth of shaft required was about 250 feet.

Fig. 1.



SECTION OF SHAFT.

An area of unworked coal, lying north of the slope and below the fourth lift, was attacked in such a way that the water from the portions of the mine already open could not flow into it, and the output of the mine was thus maintained, while the lower levels were allowed to fill and the shaft was being sunk. During this time these lower levels filled with water up to the before-mentioned drainage-heading at the fourth lift, giving a head of about 80 feet when the water was eventually drawn off.

The coal of the Pittsburgh seam in this district is very soft and friable, and has frequent "slips" or planes of fracture, lying at every imaginable angle, along which the cohesion is practically nothing.

To approach this body of water, with its head of 80 feet, through such material, was a hazardous undertaking. Not only must the

men be safely withdrawn after the water was tapped, but the subsequent flow must be rigidly controlled, since it flowed into the small sump of the shaft, and from there was lifted to the surface by the pumps. Any water in excess of the capacity of the pumps would, therefore, flood them and stop operations. To meet these conditions, the following method was adopted. (See Fig. 1.)

From the foot of the shaft a heading was driven in the Pittsburgh seam to within about 100 feet of the heading of Ocean No. 3, from which it was proposed to draw the water. At the same time a heading was also driven, parallel to the first, in a seam of coal about 5 feet thick, lying some 40 feet above the Pittsburgh seam. These headings were connected at their faces by a short vertical winze.

From a point about 12 feet above the top of the Pittsburgh seam a drift was driven from the side of this winze to a point immediately over the heading of Ocean No. 3. This drift was located at this height because the first 6 or 7 feet over the Pittsburgh seam are very friable and liable to fall, and it was desirable to have several feet of solid rock under the drift, through which to bore into the water.

Several pieces of cast iron pipe, 4 inches internal diameter, with flange on one end, were sunk in the floor of this drift, immediately over the heading of Ocean No. 3, and securely cemented and fastened down with straps and bolts. Common gate-valves were then bolted on the flange ends of these pipes which were uppermost. A drill measuring scant 4 inches was then inserted through the open gate-valves, and holes were drilled through into the water. The drill being withdrawn and the valve closed, the water could then be admitted to the shaft as desired. One hole supplied the capacity of the pumps for several days, and others were opened as needed.

When the water had run down to the level of the valves, the intervening rock and coal were gradually broken down.

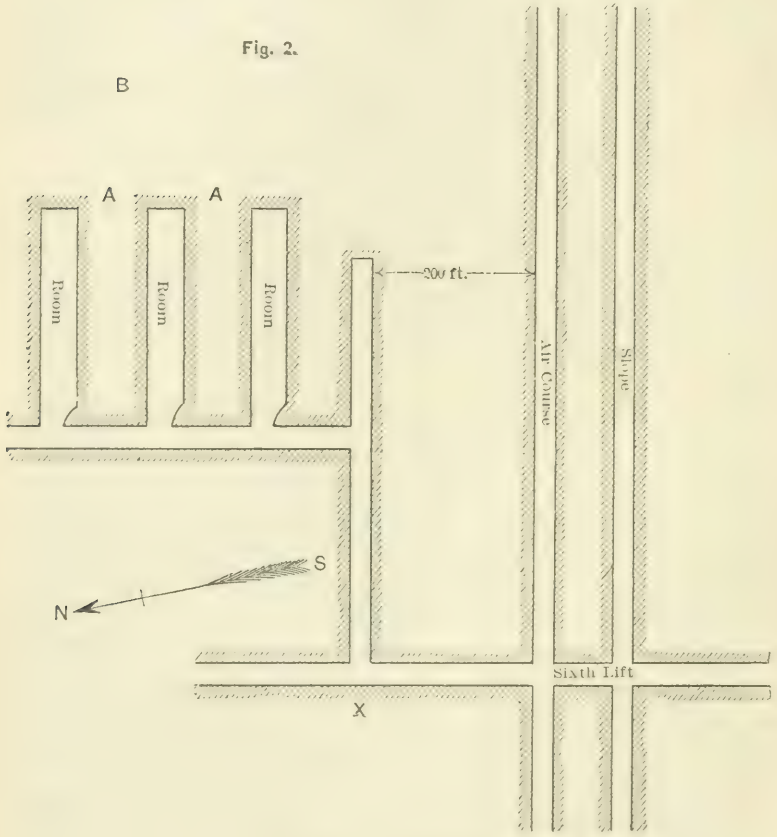
An instructive incident, bearing upon the permeability of natural strata to air, occurred during the unwatering of this mine.

Previous to the flooding of the mine, the slope had been driven some 2500 feet below the fourth lift above mentioned, without opening any workings on the north side. At this point, in the sixth lift headings were driven north and south. At a point, X, shown in Fig. 2, about 200 feet north of the slope, a heading was driven to the rise, parallel to the slope. From this, other workings were started, the rooms, A A, Fig. 2, being those of present interest. The face of these rooms is about 40 feet higher than the point, X, at the sixth

lift. When the water was rising in the workings, a considerable quantity of air was therefore entrapped into the rooms, A A.

This portion of the mine was abandoned to the water about eighteen months, and during probably half that time the water was overflowing at the fourth lift, giving a head of 40 feet at the face of the rooms, A A. Subsequent examination revealed the fact that

Fig. 2.



SKETCH OF WORKINGS.

the rooms had been thoroughly filled with water to the roof, and the air must therefore have been forced out through not less than 200 feet of solid coal.

During the time these workings were abandoned, the output was maintained, as before mentioned, from the area, B. While the water was being pumped out, the workings in B were extended and holed through into the rooms, A A. When the first opening was made

into the latter, the surface of the water on the slope was about 10 feet lower than the face of these rooms. A violent rush of air into rooms A A occurred, and the water rose suddenly on the slope about 7 feet vertically. The air had not been able to follow in through the coal as fast as the water was taken out, the vacuum representing a water-column 7 feet high. The time during which the water was lower on the slope than the face of rooms A A, and the air was therefore being drawn into these rooms, was about two weeks.

It is to be regretted that more accurate data were not obtained in regard to this phenomenon, but its importance was not realized until too late.

Some years ago, in the same seam, the writer had occasion to drive into a heading which had been driven over an anticlinal dome, that is, a point from which the seam dipped in every direction. The heading was filled with water, so as to cut off the portion passing over this anticlinal dome from the remainder of the workings; and in order to escape from it the air would have to pass through the roof at right angles to the stratification. The head of water supported by the imprisoned air when reached was about 3 feet, and the marks on the side of the heading indicated that the water had never reached the roof, although at one time the imprisoned air had supported a head of upwards of 80 feet.

It is therefore to be inferred that air will pass slowly through the soft semi-bituminous coal, but will not pass, to a perceptible degree, through slate rock at right angles to the stratification.

SURVEY OF UNDERGROUND CONNECTION AT LEAVENWORTH, KANSAS.

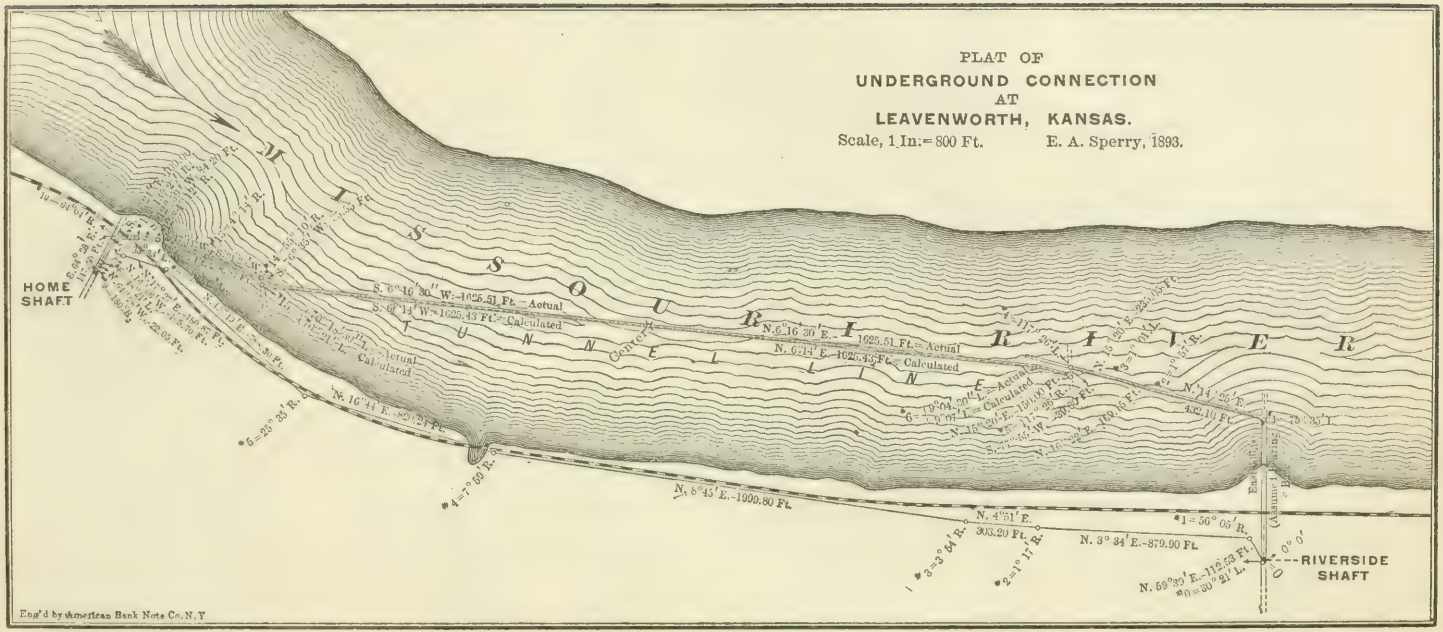
BY EDWIN A. SPERRY, GOTHIC, COLO.

(Virginia Beach Meeting, February, 1894.)

AT the request of some of my engineering acquaintances, to whom the results became known, I submit the following description of the survey made by me for the underground tunnel-connection between the Riverside and Home coal-shafts, at Leavenworth, Kansas.

The State law requires an escape-shaft; and as neither of the shafts had one, the two companies decided to make this connection in order to comply with the law and still escape the great expense of sinking

FIG. 1.


PLAT OF
UNDERGROUND CONNECTION
AT
LEAVENWORTH, KANSAS.
Scale, 1 In.= 800 Ft. E. A. Sperry, 1893.
MISSOURI RIVER
HOME SHAFT
RIVERSIDE SHAFT
TUNNEL
Center Line
S. 6° 16' 30" W.-1025.51 Ft.-Actual
S. 6° 13' W.-1025.43 Ft.-Calculated
N. 6° 16' 30" E.-1025.51 Ft.-Actual
N. 6° 13' E.-1025.35 Ft.-Calculated
N. 16° 44' E.-851.21 Ft.
N. 8° 45' E.-1909.80 Ft.
N. 4° 51' E.
303.20 Ft.
N. 3° 34' E.-379.90 Ft.
N. 14° 56' E.
432.10 Ft.
N. 59° 30' E.-115.73 Ft.
Engr'd by American Bank Note Co. N. Y

two additional shafts. Being the engineer of the Kansas and Texas Coal Company of St. Louis, Mo., owners of the Riverside mine, I was detailed to make the surveys. The shafts were about 5020 feet apart, and I was instructed to start work at both ends, that it might be driven more rapidly. Fig. 1 is a plat of the connection.

I started the surveys December 1, 1891, and on December 31, 1892, the headings were broken through, with an error of less than three feet in lateral measurement, and about two inches in length. These results, I think, are very good, when the circumstances are taken into consideration. In the first place, I was obliged to work with green men, such as I could pick up around a coal-mine, who had had but a trifling experience in work of this kind. Then I had to drop my base-lines 720 feet down each of the two shafts, which were but 10 feet in width, giving me a base of less than 10 feet in either case. Finally, the line of the tunnel was so located as to run down the middle of the Missouri river, which made it impossible to check on the surface further than by running down the river-bank, along the bluffs. These special difficulties required exceptional care. My plan of operation was as follows:

1. To tie the two shafts by carefully checked lines, three in number, down the Missouri Pacific railroad track, using only parts of any one line for either of the others, and thus checking the traverse on the various points.

2. To drop my bases down the two shafts.

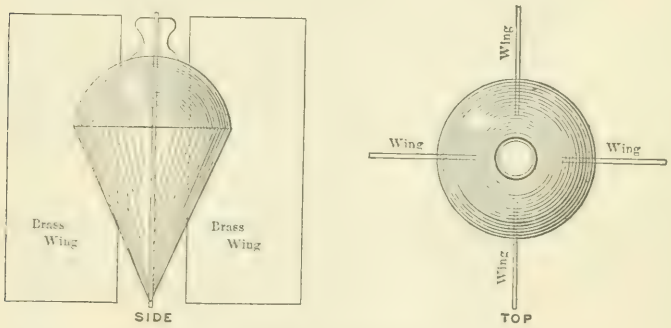
3. To run my underground lines to the point of starting, in each mine. I had a fair opportunity to close in on some parts of my underground lines, and I checked them as much as possible in this way.

In reading angles I repeated four times, taking the mean as my value. The instrument I used on the work was a "light mountain-transit," made by W. & L. E. Gurley, of Troy, N. Y., and I am more than pleased with its operation. It was of the old pattern, and I have had it in almost constant use since 1884 without finding any repairs necessary. My measurements were made on the surface with a 500-foot steel tape, made for me by the same firm. The underground measurements were made with a 100-foot steel tape (Chesterman).

The first problem, that of tying the two shafts, was one of comparative simplicity, and consisted of checking and rechecking until the tie was absolute, or as nearly so as possible. After this preliminary operation, what might fairly be called the real work was begun.

The second problem, that of dropping the bases in the shafts, was one of great importance, and demanded, of course, the utmost care and accuracy of instrumental work. I first tried plumb-lines, with 5-pound plumb-bobs. I used braided linen line at first, thinking that the lightness of the line would be an advantage, but found that this would not do. Next I tried fine piano-wire; and although it was an improvement, it was not much better. I then sawed slits in the sides of the plumb-bobs, into which I inserted square pieces of sheet-brass, in order to offer more resistance to the oil or water in which I hung them. I found this a great improvement, but even then they were not steady. Water was dropping considerably in the shafts, more especially in the Home shaft, and notwithstanding the fans were stopped, there was a constant whirl of air, which threw the drops of water from side to side, so that when they struck the line it was with considerable force, enough, it seems, to prevent its settling perfectly.

FIG. 2.



Winged Plumb-Bob.

Fig. 2 represents my "winged" plumb-bob. I think it would be highly serviceable in shafts not more than 200 or 300 feet in depth. As a final measure, I obtained from Messrs. Gurley a side telescope, which A. S. Aloe & Co., of St. Louis, attached to my transit. In order to adjust the instrument thoroughly, I suspended one of my "winged" plumb-bobs in oil, with fine piano-wire, from a high trestle, on which I adjusted the axis on the standards after having adjusted the line of collimation in the main telescope. I then stretched two wires horizontally from the roofs of two buildings, making them as nearly parallel as possible by very careful measurement, and at a distance from each other equal to the distance between the centers of the two telescopes. I then set up my instru-

ment exactly under the two wires, and with them checked the adjustments of both the line of collimation and the axes on the standards. Having these in perfect adjustment, I then adjusted the side telescope to travel on the side-wire, or in a line parallel to it. I also, with this wire, adjusted the hair as to its being vertical, both in the main as well as in the side-telescope. In this way I brought both telescopes to work in planes as nearly parallel as possible.

There were two landings at each shaft, one about 20 feet above the other. I brought my tie line to each shaft at the lower landing, placing small tacks (on which I had filed an "x") on each side of the shaft, at a short distance from the edge, so that they could be easily seen from the upper landing.

I then took a 4 by 4-inch timber, long enough to reach across the shaft safely, through which I bored, at the middle, a $\frac{1}{2}$-inch hole, and nailed a short board across one end. To this timber, through the hole, I bolted my tripod head, and placed it across the shaft at the upper landing, so as to bring the tripod head at the center of the shaft. The short cross board at the end gave three points of bearing, thus insuring firmness. Having placed planks across the shaft, I screwed my transit to the tripod head, which gave me a command of the entire shaft both above and below. I then carefully "whipped in" my side telescope between the two tacks on the lower landing, and threw my sight to the bottom of the shaft. In this way I could eliminate almost any error in the adjustment. If the standard-axes had been out, the bearing of the line at the bottom would have been the same as above; if the hair had not been exactly vertical, the cross-hair intersection would have travelled on the same line; if the projection of the object-glass had not been perfectly true, it would have made the same error on each side of the shaft at the bottom. Of course, it would have been desirable to have the line at the bottom absolutely coincident with the line at the top, and I endeavored to have it so; but if it was not exactly under, but was on the same bearing as the line above, the result would have been but slightly affected. I tried to have my instrument in perfect adjustment, and I am sure that it was quite so, but I did not want to take any chances in this matter. It was rather too far to call easily; so I arranged a code of signals with the bell.

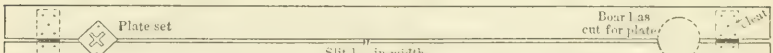
I found that it was impossible to use a direct light; so I had two straight-edges cleated together, leaving a slit between them $\frac{1}{16}$-inch wide. They are shown in Fig. 3.

At first I used the boards as they were, but found that the chang-

ing temperature and the moisture in the shaft flared the light, even with as narrow a slit as this. I then tried sheet-iron plates in which I had cross-slits cut, placing the cross-slits, at 45° with the main slit, between the straight-edges.* Even this, with the naked light, was not very satisfactory, and I finally tried a "bulls-eye" lantern, using a common reflector to deflect the light up through the plates. I cut away the boards so as to allow the light to go up through the full length of the slits in the iron sheets, and of course placed the intersections of the slits in line with the slit in the boards. In this way I had only to set the cross-slits on the line, and the center of the slit between the boards indicated the line at any point in their entire length.

I checked these lines several times, and I found almost no perceptible variation, especially in the Riverside shaft. In the Home shaft the conditions were much less favorable, as I have before indicated.

FIG. 3.



Arrangement of Slits for Illumination of Line.

In fact, I found that I had an error of about three minutes in this shaft, or rather that the results seemed to locate it at this point, at which I was not at all surprised, as I looked for it here more than at any other point. In checking my line as plumbed, with the line as projected with the instrument, I found in one case a variation of thirty minutes in bearing, which would have been a very serious matter in the results.

The matter of levels did not enter into the problem, as the coal-seam governed that entirely. The tunnel was timbered and lagged throughout. The dimensions of the tunnel were 7 feet at the bottom, 6 feet at the top, and 5 feet 5 inches in the clear after timbering. The air was carried by a brattice the entire distance with good results. No trouble was experienced from warping and leaking of the board brattice.

Both the mines are worked on the long-wall system, and of course it was necessary to remove the timbers as fast as the face reached them.

The connection is closed by a system of "traps," so as to prevent

* In Fig. 2 the engraver has made the opening at the right-hand end, as cut for the plate, a little too large.

any interference with the ventilation-system of either mine, each being carried on perfectly independent of the other.

I append the plat and calculations, as probably interesting to those members of the Institute who have to deal with work of this kind.

Calculated Traverse.

From Station 0. at Riverside Shaft to face of Riverside Tunnel, B. S. West.

No.	Angle. Deg. Min.	Bearing, Deg. Min.	Distance. Feet.	North. Feet.	South. Feet.	East. Feet.	West. Feet.
0	.....	.....	606.90	.....	.....	606.90	.....
1	75 35 L	N.14 25E.	432.10	418.48	.....	107.58	.....
2	1 57 R.	N.16 22E.	169.15	162.30	.....	47.66	.....
3	1 01 L.	N.15 21E.	235.05	226.66	.....	62.22	.....
4	117 26 L.	S.77 55W.	36.80	.....	7.70	.....	35.98
5	117 26 R.	N.15 21E.	150.00	144.65	.....	39.70	.....
6	9° 07′ 24″ L.	N.6° 13′ 36″ E	1625.43	1615.84	.....	176.30	.....
			3255.43	2567.93	7.70	1040.36	35.98
				7.70		35.98	
				2560.23		1004.38	

From Station 0. at Riverside Shaft to face of Home Tunnel, B. S. West.

0	30 21 L.	N.59 39E.	112.53	56.85	.....	97.10	.....
1	56 05 L.	N. 3 34E.	879.90	878.20	.....	54.74	.....
2	1 17 R.	N. 4 51E.	303.20	302.11	.....	25.63	.....
3	3 54 R.	N. 8 45E.	1999.80	1976.52	.....	304.22	.....
4	7 59 R.	N.16 44E.	820.50	785.76	.....	236.23	.....
5	25 35 R.	N.42 19E.	788.30	582.89	.....	530.71	.....
6	24 52 L	N.17 27E.	180.87	172.54	.....	54.23	.....
7	33 33 L.	N.16 06W.	105.70	101.55	.....	.....	29.31
8	48 22 L.	N.64 28W.	22.05	9.50	.....	.....	19.89
9	180 00 R.	S.64 28E.	147.50	.....	63.57	133.09	.....
10	64 04 R.	S. 0 24E.	100.00	.....	100.00	0.70	.....
11	13 23 R.	S.12 59W.	84.20	.....	82.04	.....	18.91
12	4 12 R.	S.17 11W.	223.55	.....	213.57	.....	66.04
13	4 14 R.	S.21 25W.	238.93	.....	222.43	.....	87.24
14	55 10 R.	S.76 35W.	35.55	.....	8.24	.....	34.58
15	70° 21′ 24″ L.	S.6° 13′ 36″ W	1625.43	.....	1615.84	.....	176.30
			7668.01	4865.92	2305.69	1436.65	432.27
			3255.43	2305.69		432.27	
			10,923.44	2560.23		1004.38	

Calculated connecting line, N. 6° 13′ 36″ E.—3250.86 feet.

"Closing-In" Line.

No.	Angle. Deg. Min.	Bearing. Deg. Min.	Distance. Feet.	North. Feet.	South. Feet.	East. Feet.	West. Feet.
.....	.....	N.15 21E	(Line 5-6 Riverside line.)				
0	8 55L.	N. 6 26E.	400.00	397.48	.....	44.82	.....
1	0 08L.	N. 6 18E.	400.00	397.58	.....	43.89	.....
2	0 13R.	N. 6 31E.	400.00	397.42	.....	45.39	.....
3	0 25R	N. 6 56E.	330.00	327.59	.....	39.83	.....
4	2 31L.	N. 4 25E.	100.00	99.70	.....	7.70	.....
5	0 13R.	N. 4 38E.	200.00	199.34	.....	16.15	.....
6	1 34R.	N. 6 12E.	400.00	397.66	.....	43.20	.....
7	0 12R.	N. 6 24E.	500.00	496.88	.....	55.73	.....
8	0 03R.	N. 6 27E.	300.00	298.10	.....	33.70	.....
9	0 05R.	N. 6 32E.	221.35	219.91	.....	25.18	.....
10	70 00R	N.76 32E.	(Equals line 14-15 Home line S. 76°35′ W.)				
				3231.66		355.59	

$$\frac{355.59}{3231.66} = \text{Tang. } 6° 16′ 45″.$$

$$2.5509495 = \text{Log. } 355.59$$
$$3.5094257 = \text{“ } 3231.66$$
$$9.0415238 = \text{“ Tang. } 6° 16′ 45″$$

$$\frac{3231.66}{\text{Cos. } 6° 16′ 45″} = 3251.16$$

$$3.5094257 = \text{Log. } 3231.66$$
$$9.9973268 = \text{“ Cos. } 6° 16′ 45″$$
$$3.5120385 = \text{“ } 3251.16$$

Actual "closing in" line = N. 6° 16′ 45″ E. — 3251.16
Calculated " " " = N. 6° 13′ 36″ E. — 3250.86
Error . . . = 3′ 09″ .30

Actual Traverse.

From Station 0. at Riverside Shaft to face of Riverside Tunnel.—B. S. West.

No.	Angle. Deg.	Min.	Bearing. Deg.	Min.	Distance. Feet.	North. Feet.	South. Feet.	East. Feet.	West. Feet.
0	0	00	East.		606.90			606.90	
1	75	35L.	N.14	25E.	432.10	418.48	.....	107.58	.....
2	1	57R.	N.16	22E.	169.15	162.30	.....	47.66	.....
3	1	01L.	N.15	21E.	235.05	226.66	.....	62.22	.....
4	117	26L.	S.77	55W.	36.80	.....	7.70	.....	35.98
5	117	26R.	N.15	21E.	150.00	144.65	.....	39.70	.....
6	9° 4′	15″L.	N.6	16′45″E.	1625.58	1615.83	.....	177.79	.....
					3255.58	2567.92	7.70	1041.85	35.98
						7.70		35.98	
						2560.22		1005.87	

From Station 0. at Riverside Shaft to face of Home Tunnel.—B. S. West.

0	30	21L.	N.59	39E.	112.53	56.85	.....	97.10	.....
1	56	05L.	N. 3	34E.	879.90	878.20	.....	54.74	.....
2	1	17R.	N. 4	51E.	303.10	302.11	.....	25.63	.....
3	3	54R.	N. 8	45E.	1999.80	1976.52	.....	304.22	.....
4	7	59R.	N.16	44E.	820.50	785.76	.....	236.23	.....
5	25	35R.	N.42	19E.	788.30	582.89	.....	530.71	.....
6	24	52L.	N.17	27E.	180.87	172.54	.....	54.23	.....
7	33	33L.	N.16	06W	105.70	101.55	.....	.....	29.31
8	48	22L.	N.64	28W	22.05	9.50	.....	.....	19.89
9	180	00R.	S.64	28E.	147.50	.....	63.57	133.09	.....
10	64	04R	S. 0	24E.	100.00	.....	100.00	0.70	.....
11	13	23R.	S.12	59W.	84.20	.....	82.04	.....	18.91
12	4	12R.	S.17	11W.	223.55	.....	213.57	.....	66.04
13	4	14R.	S.21	25W.	238.93	.....	222.43	.....	87.24
14	55	10R.	S.76	35W.	35.55	.....	8.24	.....	34.58
15	70°18′50″L.		S.6	16′45″W	1625.58	.....	1615.83	.....	177.79
					7668.16	4865.92	2305.68	1436.65	433.76
					3255.58	2305.68		433.76	
Actual line measured, .					10923.74	2560.24		1002.89	
Calculated line, . .					10923.44	2560.22		1005.87	
Excess of measured line,					.30	.02		2.98	

$$\frac{2.98}{\text{Cos. } 6°16′45″} = 2.96 = \text{lateral error between faces.}$$

$$1.48 = \text{lateral error for each face.}$$

Résumé.

Errors.

Excess of "closing angles,"	0° 03′
Actual line to right of calculated line,	0° 03′ 09″
Actual length in excess of calculated line,	0.30 feet.
Lateral distance of each face off true line at meeting-point,	1.48 feet.
Lateral distance between face-centers,	2.96 feet.

Memoranda.

Whole length of line measured,	10,923.74 ft
Whole length of line calculated,	10,923.44 ft.
Number of angles turned (to starting points),	20
Number of angles turned (on closing-in line)	11
Total number of angles turned,	31
Total amount of angle to the right,	715° 03′
Total amount of angle to the left,	355° 00′
Excess to the right,	360° 03′
Connecting line calculated N.6°13′36″E.,	3250.86 ft.
Connecting line actual N.6°16′45″E.,	3251.16 ft.

ORE-WASHER AT LONGDALE, VIRGINIA.

BY GUY R. JOHNSON, LONGDALE, VA.

(Virginia Beach Meeting, February, 1894.)

Like many other similar plants the ore-washer of the Longdale Iron Company has been a growth of years. In its principal features there is probably nothing new, as the type is that of the well-known "log-washer." Some of its minor details may, however, be of interest to those members who have to deal with the washing of large quantities of brown iron-ores, the principal gangue of which is clay.

Until 1883, the ore at Longdale, being of unusual purity, needed no washing, and was charged to the furnaces as it came from the mines—both lump and fine—the only attempt at cleaning being the picking over a screen of $1\frac{1}{2}$-inch iron bars, $3\frac{1}{2}$ inches from center to center, set in the top of the bins through which the cars for the furnaces were loaded.*

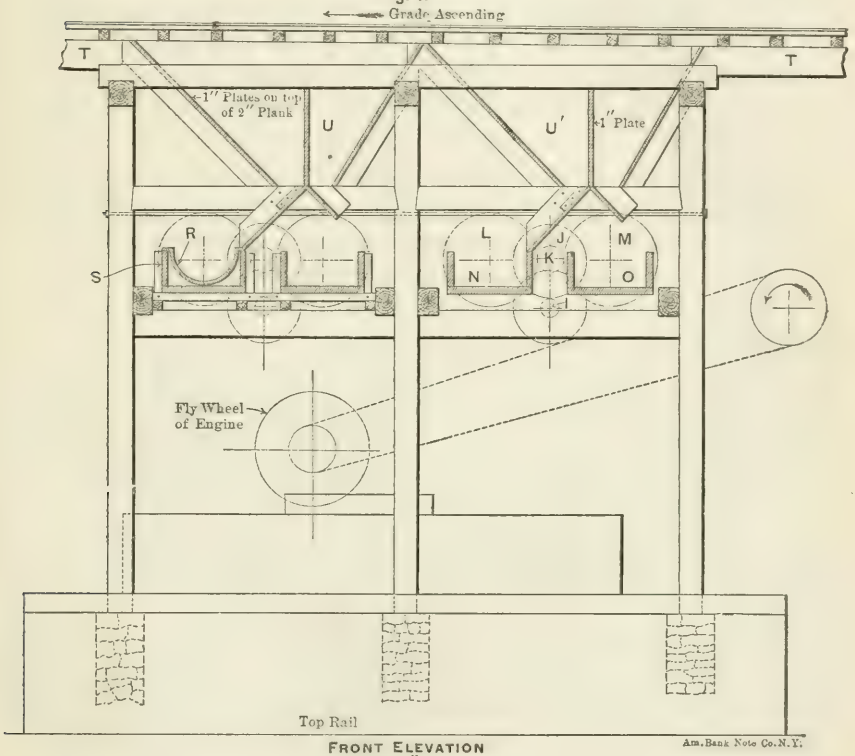
In 1883 it was no longer possible to win the ore by open cuts, and regular underground work became necessary. It was very

* See *Trans.*, xx., p. 102.

soon found that the ore was not so clean as near the surface; and at the same time that careful sorting by the feeble light of miners' lamps was impracticable. It was consequently evident that the ore must be washed; and a small quantity thus treated in a log-washer using horse-power, made a great improvement in the work of the furnaces.

In 1884 a complete steam-driven plant, consisting of two Carter

Fig. I.



Longdale Ore-Washer.

washers, was erected and put in operation. The result from these washers was as disappointing as that from the log-washers had been gratifying; and after a struggle, during which they were thoroughly tested, the Carter washers were thrown out and a set of four log-washers, driven by the same engine and gear, was put in. As soon as these began to wash, the effect was visible. The ore was properly cleaned, and the furnaces worked correspondingly well.

For several years this plant washed the ore satisfactorily; but

there were certain features of the plant which it was desired to eliminate. Chief among these were the engine—an old-fashioned machine, very wasteful of steam—and the driving-gear, which consisted of a set of belt-driven friction-wheels, so arranged that each pair of logs could be started or stopped at will.

Early in 1891, as the old plant showed unmistakable signs of giving out, the writer began to prepare plans for a new plant, to be situated alongside of the old one.

Fig. 1 is a front- and Fig. 2 a side-elevation of the new washer. The new engine (not shown) is an automatic Buckeye, giving 25 horse-power with 60 pounds pressure, and 285 revolutions per minute. From the 3-foot belt-pulley of the engine, the main shaft is driven by a 12-inch belt. On this main shaft are the pulleys for driving the washers, which is done with 6-inch rubber belts, running over the pulleys, D and E (Fig. 2). F and G, in the same figure, are the loose pulleys to which the belts are shifted when it is desired to stop the washers without stopping the engine.

As the four logs are arranged in pairs, the pairs being alike, a description of one pair only will be given.

To the end of the shaft, H (Fig. 2), to which is fastened the pulley, D, is keyed the small pinion, I, which meshes into the spur-wheel, J. This drives another pinion at K, and this in turn gears into the spur-wheels, L and M, thus driving the logs in the two washers, N and O, at the rate of 12 revolutions per minute.

The driving-gear is connected to the logs, which are on a slope of $\frac{3}{4}$ inch per foot, by cast-iron clutches, one of which is shown at P (Fig. 2).

The rear bearing is $5\frac{1}{2}$ inches in diameter, and is of cast-iron. It is cast solid with a flange, on the face of which is turned a shoulder. This shoulder fits into a corresponding recess turned in the similarly flanged end of the log. The two flanges are bolted together, and make a very stiff joint, as the shoulder prevents any lateral motion.

The logs are simply pieces of cast-iron pipe, 17 feet $5\frac{1}{2}$ inches long, $11\frac{1}{2}$ inches diameter, with metal $\frac{3}{4}$ inch thick, and flanged at each end. This makes a splendid log, one that is stiff and wears well.

The method of attaching the spoons is shown in Fig. 3. They are put on in two spiral threads, 180° apart, and with a 5-foot pitch. They are set 45° apart on the circumference, thus making 8 spoons to each revolution, as shown in Fig. 3. By this method of laying out, there are, at every $\frac{1}{8}$ of a revolution, two spoons opposite each other and 180° apart. If now, holes be bored through the pipe,

under the two holes with which the foot of each spoon is provided, two through-bolts will fasten on two spoons. These bolts are $\frac{3}{4}$ inch in diameter, and are made with nuts at each end as shown.

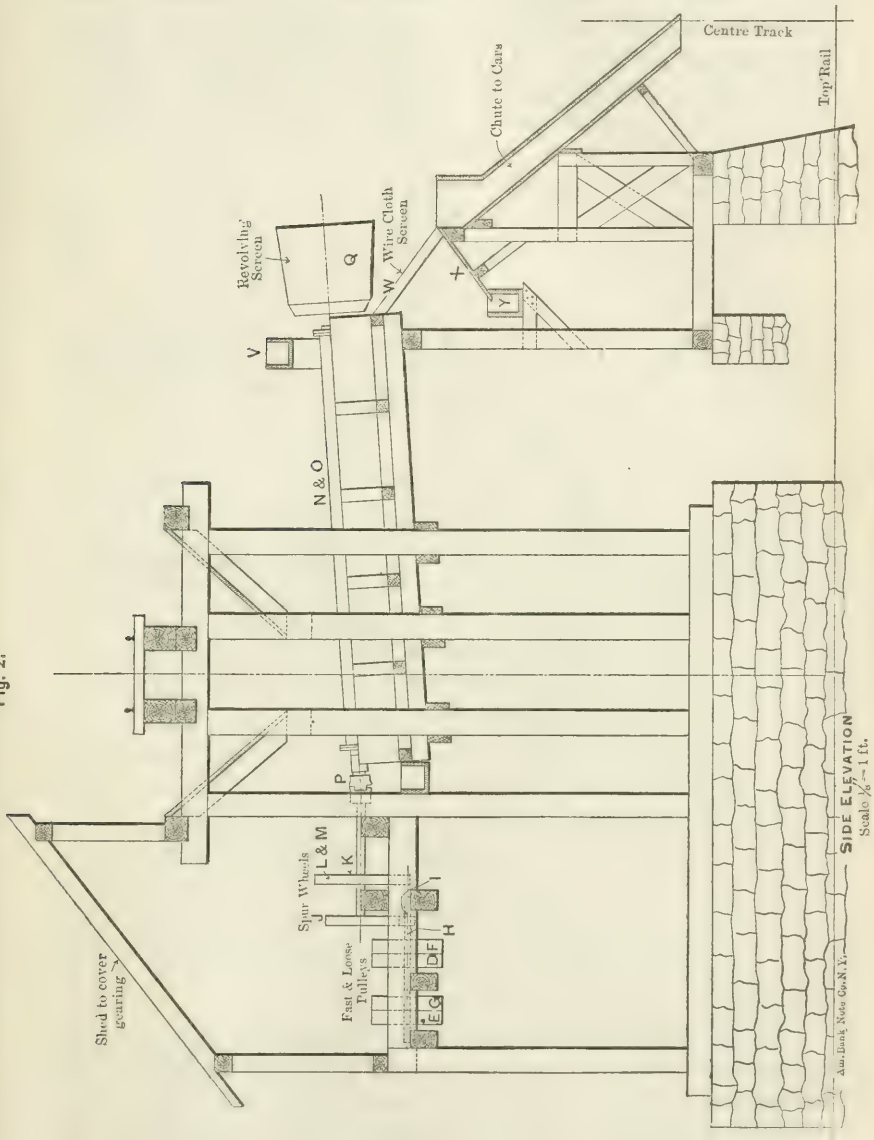


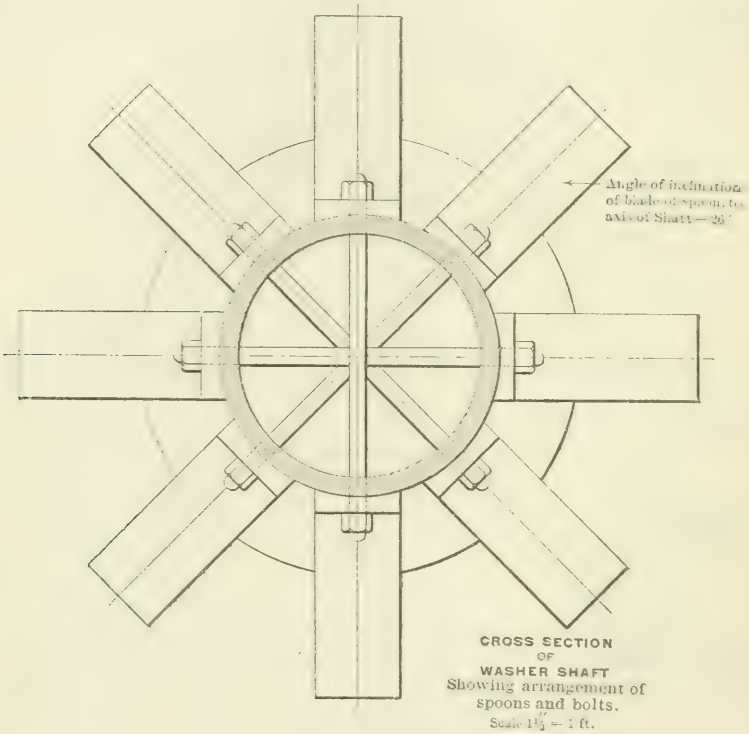
Fig. 2. Longdale Ore-Washer.

At the upper end of the log there is a gudgeon, similar to the one at the lower end, except that the bearing is only $4\frac{1}{2}$ inches in diameter

and extends 2 feet beyond the box. To this end the revolving screen, Q (Fig. 2), is attached. The screens are made of $\frac{3}{16}$-inch steel-plates, perforated with $\frac{3}{16}$-inch holes, $\frac{1}{2}$ inch from center to center.

The troughs are made in the usual way, of a wooden frame in which are fastened the iron plates constituting the trough proper. The bottoms and sides of the frames are of 3-inch pine, thoroughly braced by the yokes shown at S, Fig. 1. Both bottom and sides

Fig. 3.


Angle of inclination of blade of spoon, to axis of Shaft = 26°
CROSS SECTION OF WASHER SHAFT
Showing arrangement of spoons and bolts.
Scale 1½" = 1 ft.

Longdale Ore-Washer.

are bolted to iron end-pieces, in which are cast seats for the chilled iron gearing-boxes.

The iron plates constituting the trough proper rest upon the sides of the frames, to which they are attached by $\frac{5}{8}$-inch lag-screws. As indicated at R, Fig. 1, they are of the usual semi-circular pattern, the only modification being that they are cast in sections only 15 inches long. This permits them to be made as open-sand castings.

In the operation of the plant, the ore is brought from the mines

in side-dump cars, holding about $5\frac{1}{2}$ tons each. The cars are pushed out past the washer on the trestle, T, Fig. 1, which is built with a grade ascending in the direction of the arrow shown in the drawing. The cars are then allowed to drop down, two at a time, until they come over the chutes, U and U′, which are lined with 1-inch iron plates, as indicated in Fig. 1.

The ore, falling through the chutes to the logs, is caught by the spoons, which force it up against a descending current of water from the trough, V (Fig. 2), until it reaches the revolving screens, Q, into which a stream of water from the same trough, V, is flowing. There the ore is further washed and at the same time separated. All over $\frac{3}{16}$-inch diameter passes along the screen and falls into the "chute to cars" (Fig. 2).

The fines, which drop through the perforations, fall on the 14-mesh wire-cloth screen, W (Fig. 2), where they are further washed and screened, all over 14-mesh going to the cars, while the sludge falls on the apron X, and is thence carried away in the trough Y, which also conveys away the water from the rear end of the washers.

As all who have used log-washers know, the current of water descending in the troughs is apt to carry off more or less ore through the rear end. To prevent this loss, two perforated screen-plates (not shown in the drawings) are used. The muddy water from the trough passes through a gate upon these screens, through which it falls and is carried away into Y, while the ore remains upon the screen. Only one screen is used at a time, and as soon as ore enough has accumulated upon it to stop the perforations, the water is shut off and turned into the other. The ore is shovelled back into the washers. This device saves a great deal of ore at a very low cost, as it requires the attention of one man for only part of his time, thus leaving him free to help at other points.

The following table, taken over a week in March, 1893, gives a good idea of the capacity of the plant and its performance:

Average of Six Days' Work.

Pounds of coal burned per diem,	1479.16
Tons of ore washed,	196.2
Tons of washed ore,	138.9
Percentage of washed ore to ore washed,	70.8
Number of hours run,	$5.37\frac{1}{2}$
Number of men (including engineer),	6
Cost per ton of washed ore for labor,	$0.045
Pounds of coal burned per ton of ore washed,	10.6

It may be asked: "Why not use finer wire screens, and thus raise the percentage of yield?" The reply is, that the attempt has been made, and, though temporarily dropped, has not yet been abandoned.

A rough examination had induced us to believe that the silica particles of the sludge were finer than those of the ore. To test this, screens of varying fineness of mesh, all finer than the 14-mesh, were placed under three of the revolving screens, the fourth being allowed to remain as it was, *i.e.*, 14-mesh. Samples were taken frequently throughout each day's run, for a week, from all four screens.

An analysis showed that, for all practical purposes, the iron and silica contents were the same in each case.

The percentage-yield was increased tremendously by the fine screens, but the furnaces did not seem to work the increased fines very well. It was therefore decided to drop the experiment for the time being. It seems very probable, however, from knowledge since acquired, that the trouble at the furnaces may have been due to a different cause, now overcome.

Since the foregoing was written (in 1893), we have put in 20-mesh screens, with an additional saving of 4 per cent. of ore as the result. We have also added two Merry picking-tables (a modification of the German bumping-table).

NOTE ON CERTAIN MAGNETIC PHENOMENA IN GOLD-BEARING SLATES.

BY C. A. MEZGER, M.E., CHARLOTTE, N. C.

(Virginia Beach Meeting, February, 1894.)

During my stay at Raposos, State of Minas Geraes, Brazil, I had the opportunity of making some observations, which, in the shape in which I am able to lay them before the Institute, are only to be considered as an experience which may possibly have important consequences.

The ore-bodies worked there for gold are exclusively chimneys, mostly cylindrical, following the dip of the argillaceous slates (about 45° E.), without any visible connection with each other, without veins, and only exceptionally with some small stringers along the stratification, pinching out at short distances from the chimneys.

They are composed of quartz with about 8 to 10 per cent. of pyrite and pyrrhotite, and different iron oxides.

The slates contain everywhere some 2 per cent. of magnetite, which causes local declinations of the compass.

Slates with a considerably increased quantity of magnetite (8 to 10 per cent.) may be observed in layers in the general slate, without sharp divisions from it. These concentrations are poor in gold. They act most intensely on the needle, and cause on the top of a hill a declination of 10°, and in the mine up to 26°.

The declination of the place was about 3° E.

A cross-cut adit was driven against a group of chimneys which had been worked formerly from the surface.

It crossed a magnetite concentration, on which drifts were driven. Some experiments with a common English theodolite soon showed the practical impossibility of surveying with a needle, which was influenced to an intolerable degree.

The regular survey was subsequently made with a Freiberg theodolite (without needle) and based upon the astronomical meridian, from which all declinations were obtained.

The declination in the cross-cut was pretty uniformly 6° E.

In consequence of these disturbances we used a hanging-compass only for small surveys without importance. For precaution's sake, the instrument was hung up in each line at different places. We soon found a place where the declination was about 9°, or 3° above the average, for a distance of only some 8 to 10 feet in length, where some good ore had been struck—an impregnation of the slate, or perhaps a rudimentary chimney.

This aroused my curiosity, and I called attention to it, thinking at once of a possible correlation. The following day I was informed that the needle showed the same disturbance at another place in the cross-cut. I ordered some of the rock to be blasted away, and, sure enough, there was some good gold-ore indicated *by no other signs* but the disturbance of the needle. We had no other cross-cut at our disposition, and surveys in the drifts of the magnetite-concentration were without result—some interesting experiments and sketches, but nothing new with regard to the first experience.

I brought out many samples of the slates for examination. No difference whatever was apparent between those which caused the stronger declination and those which came from places with regular declination.

I believe that the magnetic polarization of the small crystals of magnetite is parallel where they occur with gold, and that it is in all directions at other places, so as to form ore-magnets in the first and to compensate each other in the second case.

I leave the question open whether there is a causal correlation between the gold and the parallel polarization.

I frankly confess that the material of these observations is scanty and poor. But I think it is worth while to call attention to them, so that mining engineers may be on the lookout for similar phenomena, and the data necessary for safe induction may be gathered.

SOME EXPERIMENTS FOR DETERMINING THE REFRACTORINESS OF FIRE-CLAYS.

BY H. O. HOFMAN AND C. D. DEMOND, MASS. INSTITUTE OF TECHNOLOGY, BOSTON, MASS.

(Virginia Beach Meeting, February 1894.)

THERE are two methods of determining the fusibility or refractoriness of fire-clays, the theoretical and the experimental. In the former, conclusions are drawn from the chemical composition; in the latter, from the changes which the clay undergoes when subjected to intense heat. It is well to use both methods, beginning with the actual heat-test and explaining the results obtained from this by the help of the chemical analysis. In discussing the subject, however, it is better to reverse this order.

I. THE THEORETICAL METHOD.*

Leaving out the hygroscopic and combined waters and any organic matter, as not directly affecting the fire-resisting property, the refractory constituents of fire-clay are alumina and silica, and the fluxing constituents magnesia, lime, soda, potash and ferric oxide. According to Bischof,† alumina is the least fusible component, and silica follows close upon it. If, however, a mixture of the two be heated, it will be found that its melting-point is much lower than that of alumina, as a silicate will then have been formed. The way in which silica affects the refractoriness of alumina has been strikingly presented by Seger‡ in a diagram reproduced in Fig. 1. The abscissæ represent the melting-points of Seger cones Nos. 26 to 36 (see p. 55); the ordinates, the relative amounts of alumina and

* C. Bischof, *Die feuerfesten Thone*, Leipsic, 1876, p. 71.

R. Keller (Revised by C. Bischof), *Ueber die Fabrication und Anwendung feuerfester Steine*, Aix-la-Chapelle, 1890; a lecture, pp. 1–16.

Bruno Kerl, *Handbuch der gesammten Thonwaarenindustrie*, Brunswick, 1879, pp. 59 and 71.

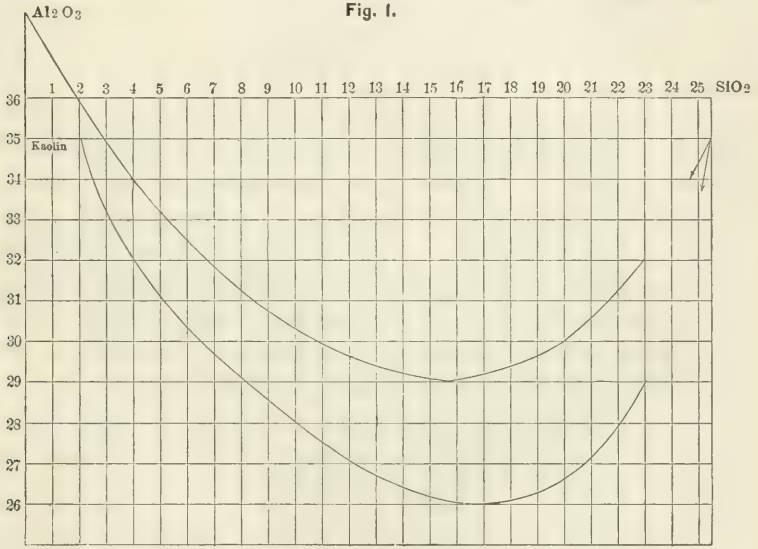
J. Post, *Chemisch-Technische Analyse*, Brunswick, 1890–91, vol. ii., p. 48.

† *Dingler's Polyt. Jour.*, vol. clxix., pp. 353, 455; vol. clxx., p. 43.

‡ *Thonindustrie-Zeitung*, 1893, p. 391.

silica in the mixture. The least fusible consists of one molecule of alumina and two molecules of silica, and melts, as shown by the upper curve, at a temperature indicated by Seger cone No. 36. The fusibility is seen to increase with the amount of silica up to the proportion $1Al_2O_3 : 17SiO_2$, and then to decrease (on account of the preponderance of silica, which cannot combine to form a silicate), until, finally, the effect of the alumina has disappeared and the melting-point of silica, equal to that of Seger cone No. 35, has been reached. The lower curve shows in a similar way the effect of silica on the fusibility of the kaolin from Zettlitz, Bohemia (see Table I., p. 45). The practical deduction is that the refractoriness of a fire-clay increases with the amount of alumina it contains.

Fig. I.



Fusibility of alumina-silica and kaolin-silica mixtures.

The effects of the fluxing constituents have been studied by Bischof* and by Richters.† The latter, in 1868, propounded the law that their fluxing property is inversely proportional to their molecular weights; thus, 40 magnesia would have a slagging effect equivalent to that of 56 lime, 62 soda, 94 potash or 160 ferric oxide. While Bischof‡ supports this view to-day, it is attacked by Seger,§

* *Dingler's Polyt. Jour.*, vol. clxv., p. 378; vol. cxcvi., pp. 438, 525; vol. cxcviii., p. 407. *Thonindustrie-Zeitung*, 1885, p. 13.

† *Dingler's Polyt. Jour.*, vol. cxci., pp. 59, 150, 229; vol. cxcvii., p. 268.

‡ See Keller-Bischof, *op. cit.*, p. 7.

§ *Thonindustrie-Zeitung*, 1893, p. 392.

who maintains that ferric oxide has a stronger slagging effect than any other of the four bases. Whatever may be the precise order in which these bases are to be placed, one thing is settled, namely, that if their total amount exceeds 6 per cent. of the ignited clay, this cannot be classed any longer as refractory.

To sum up, the fire-resisting power of a clay, considered from a chemical point of view, depends first upon the character of the fluxing-constituents and their relation to alumina and silica, and, secondly, on the relation of alumina and silica to one another. Bischof* arrives at the refractory character of a clay by what he calls the refractory quotient, which he obtains by dividing the quotient of the oxygen of the fluxes into that of the alumina by the quotient of the oxygen of the alumina into that of the silica.†

$$\frac{O \text{ in } Al_2O_3}{O \text{ in } RO} \div \frac{O \text{ in } SiO_2}{O \text{ in } Al_2O_3}.$$

In the seven standard fire-clays which he selected as types (see Table I., which has been slightly changed from the original) this refractory quotient is 13.95 with the most refractory clay—No. 1, from Saarau, Silesia—and decreases with the fire-resisting power until it reaches in clay No. 7, from Niederpleis, Nassau, the figure 1.64, and a clay the refractory quotient of which is less than 1 (equal to Seger cone No. 26) is not to be classed any longer as refractory. Taking as an example clay No. 2 of Table I.—the kaolin from Zettlitz, Bohemia—the refractory quotient would be

$$\text{R. Q. B.} = \frac{O \text{ in } Al_2O_3}{O \text{ in } RO} \div \frac{O \text{ in } SiO_2}{O \text{ in } Al_2O_3} = \frac{17.960}{1.401} \div \frac{24.363}{17.960} = \frac{12.82}{1.35} = 9.49.$$

* *Dingler's Polyt. Jour.*, vol. cxciv., p. 420; vol. cxcvi., pp. 438, 447, 525; vol. cc., pp. 110, 289. *Thonindustrie-Zeitung*, 1888, pp. 38, 50.

† When Bischof made his researches silica was written SiO_3. The quotient, expressed as above, would have had the form $\frac{O \text{ in } Al_2O_3}{O \text{ in } RO} \div \frac{O \text{ in } SiO_3}{O \text{ in } Al_2O_3}$, and in carrying out any calculations the oxygen of RO would have to be multiplied by three. For Seger's refractory quotient (see later) the ratio $\frac{O \text{ in } SiO_3}{O \text{ in } RO}$ is required; here also the oxygen of RO has to be multiplied by three. To-day the old mode of expressing the value is retained and the new atomic weights are adopted.

To find the O in the components of clay, each is multiplied by its coefficient, as $Al_2O_3 \times 0.466$, $SiO_2 \times 0.533$, $MgO \times 0.400$, $CaO \times 0.2857$, $K_2O \times 0.1698$, $FeO \times 0.222$.

Iron, present in the clay usually as ferric oxide, is figured as ferrous oxide, $Fe_2O_3 \times 0.9 = 2FeO$. Soda is figured as potassa, $62Na_2O$ being equivalent to $94K_2O$.

TABLE I.—*Bischof's Standard Fire-Clays.*

FIRE CLAY.		Al_2O_3.		SiO_2.				RO.					
Name and Source.	No.	Total pr. ct.	Oxygen, per cent.	Combined per cent.	Non-combined pr. ct.	Total pr. ct.	Oxygen, per cent.	MgO per ct.	CaO per ct.	K_2O per ct.	Fe_2O_3 pr. ct.	Total pr. ct.	Oxygen, per cent.
Saarau, Silesia.....	I.	36.30	16.917	38.94	4.90	43.84	23.366	0.19	0.19	0.42	0.46	1.26	0.293
Zettlitz, Bohemia.....	II.	38.54	17.960	40.53	5.15	45.68	24.363	0.38	0.08	0.66	0.90	2.02	0.467
Andennes, Belgium.....	III.	34.78	16.208	39.69	9.95	49.64	26.457	0.41	0.68	0.41	1.80	3.30	0.778
Coblence, Rhen. Province...	IV.	36.00	16.777	41.00	6.74	47.74	25.445	0.33	0.40	1.05	2.57	4.35	0.938
Grünstädt, Palatinate..... ...	V.	35.05	16.334	39.32	8.01	47.33	25.225	1.11	0.16	3.18	2.30	6.75	1.490
Cassel, Hesse.....	VI.	27.97	13.035	33.59	24.40	57.99	30.908	0.54	0.97	0.53	2.01	4.05	0.985
Niederpleis, Nassau.....	VII.	28.05	13.072	30.71	27.61	58.32	31.084	0.75	0.72	1.39	1.89	4.75	1.120

TABLE I.—CONCLUDED.

Fire Clay.		Loss by ignition, per cent.	O in Al_2O_3 Divided by 3O in RO.	O in SiO_2 Divided by 3O in RO.	O in SiO_2 Divided by O in Al_2O_3.	Pyrometric Equation.	Refractory Quotient.		Relative Refractoriness.	Corresponding Seger Cone.
Name and Source.	No.						Bischof R. Q. B.	Seger R. Q. S.		
Saarau, Silesia	I.	17.78	19.25	26.50	1.38	$19.25(Al_2O_3,1.38SiO_2)+RO$	13.95	33.15	100	36
Zettlitz, Bohemia	II.	13.00	12.82	17.31	1.35	$12.82(Al_2O_3,1.35SiO_2)+RO$	9.49	22.31	60–70	35
Andennes, Belgium	III.	12.00	6.86	11.20	1.63	$6.86(Al_2O_3,1.63SiO_2)+RO$	4.21	11.08	50	33
Coblence, Rhen. Province	IV.	11.81	5.96	9.10	1.51	$5.96(Al_2O_3,1.51SiO_2)+RO$	3.95	9.97	45	32
Grünstädt, Palatinate	V.	10.51	3.65	5.64	1.54	$3.65(Al_2O_3,1.54SiO_2)+RO$	2.37	6.03	30	30
Cassel, Hesse	VI.	9.43	4.41	10.46	2.37	$4.41(Al_2O_3,2.37SiO_2)+RO$	1.86	6.27	20	28
Niederpleis, Nassau	VII.	8.66	3.89	9.25	2.37	$3.89(Al_2O_3,2.37SiO_2)+RO$	1.61	5.54	10	26

These figures embodied in Bischof's pyrometric formula for clay give

$$12.82\ (Al_2O_3,\ 1.35\ SiO_2) + 1\ RO.$$

Bischof's method has found pretty general acceptance within the necessary limitations. It is, however, questioned to some extent by Seger,* who says that while the refractoriness of a clay increases with the alumina and decreases with the silica, the proof is wanting that the decrease in refractoriness takes place in direct proportion to the amount of silica and the increase in proportion to the square of the alumina, and that, furthermore, the refractoriness does not depend solely upon the relation which silica and alumina bear to one another. He therefore recommends adding the ratio of the fluxing constituent to the alumina with that to the silica, and multiplying this sum by the quotient obtained from dividing the latter into the former, thus:

$$\left(\frac{O \text{ in } Al_2O_3}{O \text{ in } R\,O} + \frac{O \text{ in } SiO_2}{O \text{ in } R\,O}\right) \times \left(\frac{O \text{ in } Al_2O_3}{O \text{ in } R\,O} \div \frac{O \text{ in } SiO_2}{O \text{ in } R\,O}\right).$$

Taking the same Zettlitz clay as example, Seger's refractory quotient would be:

$$R.\ Q.\ S. = (12.82 + 17.31)\ \frac{12.82}{17.31} = \frac{30.13}{1.35} = 22.31.$$

Bischof† does not consider Seger's refractory quotient an improvement on his own, and suggests that the older quotient be maintained until something better shall be discovered. Both quotients are used, Bischof's, however, being the more common one. In comparing the two by the standard clays (Table I.), it will be seen that while the experimental results of both Bischof (relative refractoriness) and Seger (number of corresponding cone) agree with Bischof's refractory quotient, there is a discrepancy with Clay VI., which Seger's refractory quotient would make more refractory than Clay V. Something similar is to be noted in Table III., where Seger's refractory quotient makes the kaolin from Blanford, Mass., less refractory than the clay from Mount Savage, Md., while the experiments of Tables IV. and V. prove the contrary to be the case. In the single series of experiments in Tables IV., V., and

* *Thonindustrie-Zeitung*, 1877, pp. 290, 296; 1889, p. 332; 1893, p. 339.

† *Thonindustrie-Zeitung*, 1889, p. 331.

VI., both refractory quotients increase on the whole with the refractory values of the mixtures, but it has not been possible to find the desirable regularity which was thought to exist. Nevertheless, the quotients have been embodied in the tables in order to furnish another means of judging of the refractory character of the clays and their mixtures made for experiment.

No deduction, however, made from the chemical analysis can have the force of a positive determination, because analysis necessarily ignores the physical constitution, whereas a coarse-grained clay is less fusible than a fine-grained, and a compact* than a loose one. Moreover, a clay is not one chemical compound, but an intimate mechanical mixture of several minerals. The more impurities a clay contains (quartz, feldspar, mica, etc.), the more liable will these be to combine upon heating to a fusible compound and attack the practically infusible clay-substance (kaolinite). The result of the chemical analysis will therefore be the more applicable the purer the clay, but in any case will only have an approximate value, and must be supplemented by actual experiment.

II.—The Experimental Method.†

A. *Introductory.*

Experimental methods may be classified as direct and indirect. Until lately, all the direct methods have given only what may be called qualitative results, *i.e.*, small samples of clay have been exposed to an elevated temperature and the effect has been noted, whether as a complete fusion or only as a fritting or whether the clay remains unaltered. Examples of this method are the tests of Knafft, Bischof, and Otto. Knafft‡ exposes the end of a small-pointed cone of clay for a short time to the hottest part of the flame of a blow-pipe with the result that the point remains unaltered or becomes rounded off or fused. Bischof prefers to expose the clay to the flame in the loop of a platinum wire. He obtains it in a suitable form by coating a piece of oiled paper with a thin paste of the clay and water and then drying, when the clay peels off in small thin tablets. Another of his qualitative tests§ is to place a small sample of dry pulverulent clay in a crucible and heat it in a furnace

* Bischof, *Thonindustrie-Zeitung*, 1886, p. 430; Seger, *ibid.*, 1890, p. 201.

† Bischof, *op. cit.*, p. 92; Kerl, *op. cit.*, p. 62; Post, *op. cit.*, p. 50.

‡ *Dingler's Polyt. Jour.*, vol. clxxiv., p. 292.

§ *Op. cit.*, p. 93.

with forced draught for two hours or more. The clay will remain unchanged, or will be fritted or fused according to the degree of refractoriness. His effective tests to distinguish fritting from fusing are to draw a line with pen and ink over the fracture of the sample or touch it with the tongue. If it is fritted it will adhere, and the ink will spread as it would on blotting-paper; if it is fused it will not stick to the tongue, and the pen and ink line will be sharp and clear. The transition from qualitative to quantitative work is made by Otto,* who forms two small test-bricks ($4\frac{3}{4}$ by $2\frac{3}{4}$ by $1\frac{3}{4}$ inches) from a uniform mixture of half raw and half burnt clay, places them alternately with two other bricks of the same size and of known properties to form an oblong on a refractory pedestal in a crucible furnace, and then heats them with charcoal, coke, and forced draught for about two hours. A comparison of the four samples shows if the heat was uniform and how the test-bricks compare with the standards. The only quantitative direct method is that by Seger and Cramer,† who form from the sample of clay to be tested a number of cones of the same size as Seger's small-sized standard cones, Nos. 26 to 36, enclose a test-cone with two different numbers of the standard cones in a magnesia crucible, and heat with gas-carbon in a Deville furnace lined with chromite. This is repeated with fresh cones and different temperatures until the place of the test-cone in the series of standards has been found. It is proposed to make at an early date an extended series of tests by this method, and further discussion of it will therefore be postponed for the present.

The indirect methods all have the common characteristic of mixing the clay that is to be tested with fluxing or refractory substances until it shows the same behavior in the fire as a given standard clay. The amount of ingredient required to accomplish this will be the measure of the fire-resisting value. All the tests so far have been made in clay crucibles, placed in a Sefström or Deville furnace, the fuel used being coke or, preferably, gas-carbon, and the temperatures aimed at varying from 1500° to 1775° C. Bischof, who originated these methods, first used the one known as the quartz method,‡ in which he toned down refractory clays by the use of finely pulverized quartz or chemically pure silica. The manner in which silica acts on alumina and clay (kaolin) has been shown in Fig. 1. Bischof was followed by Richters, who devised the supplementary alu-

* *Dingler's Polyt. Jour.*, vol. clxiii., p. 193; *Thonindustrie-Zeitung*, 1888, p. 163.

† *Thonindustrie-Zeitung*, 1891, p. 195; 1892, pp. 675, 747; 1893, p. 339.

‡ *Dingler's Polyt. Jour.*, vol. clix., p. 54; vol. clxi., pp. 208, 291.

mina method,* which tones up a low-grade clay with pure alumina until it shows the same behavior in the fire as the standard, together with which it is heated. The disadvantages of these methods, namely, the excessive nicety required in regulating the high temperatures with the silica method, and the tendency of the reagent in the alumina method to act too powerfully, and also the fact that the silica method is not always applicable, prompted Bischof to combine the two in his silica-alumina method,† in which he uses as ingredient an intimate mixture of equal parts of chemically pure silica and alumina. This does not soften at the highest temperatures, and as it is more refractory than the best clay known, it will always tone up any clay to which it is added. The mode of procedure is to weigh out ten samples of 0.1 gramme each of clay that has been ground and ignited, to mix them severally with 0.1, 0.2, 0.3, etc., grammes of the ingredient, and form prisms $\frac{1}{4}$ inch high and $\frac{5}{32}$ inch square. These are enclosed in crucibles ($1\frac{3}{4}$ inches high, $1\frac{1}{2}$ inches in diameter at the top and $\frac{3}{4}$ inch at the base, with walls $\frac{3}{16}$ inch in thickness), three or four in each, with a prism of the standard consisting of equal parts of Saarau clay (see Table I.) and the ingredient, and the value of which is assumed as 100. The crucibles are placed in a Deville furnace and heated to the melting-point of wrought-iron (say 1600° C.). The prism, which shows the same behavior in the fire as the standard, is the critical mixture. An arithmetical expression for the refractoriness is obtained by multiplying the amount of ingredient it contains by 100, and deducting the product from the value of the standard. Thus, if 0.1 gramme test-clay required 0.5 gramme of ingredient, its refractory value would be:

$$100 - (0.5 \times 100) = 50.$$

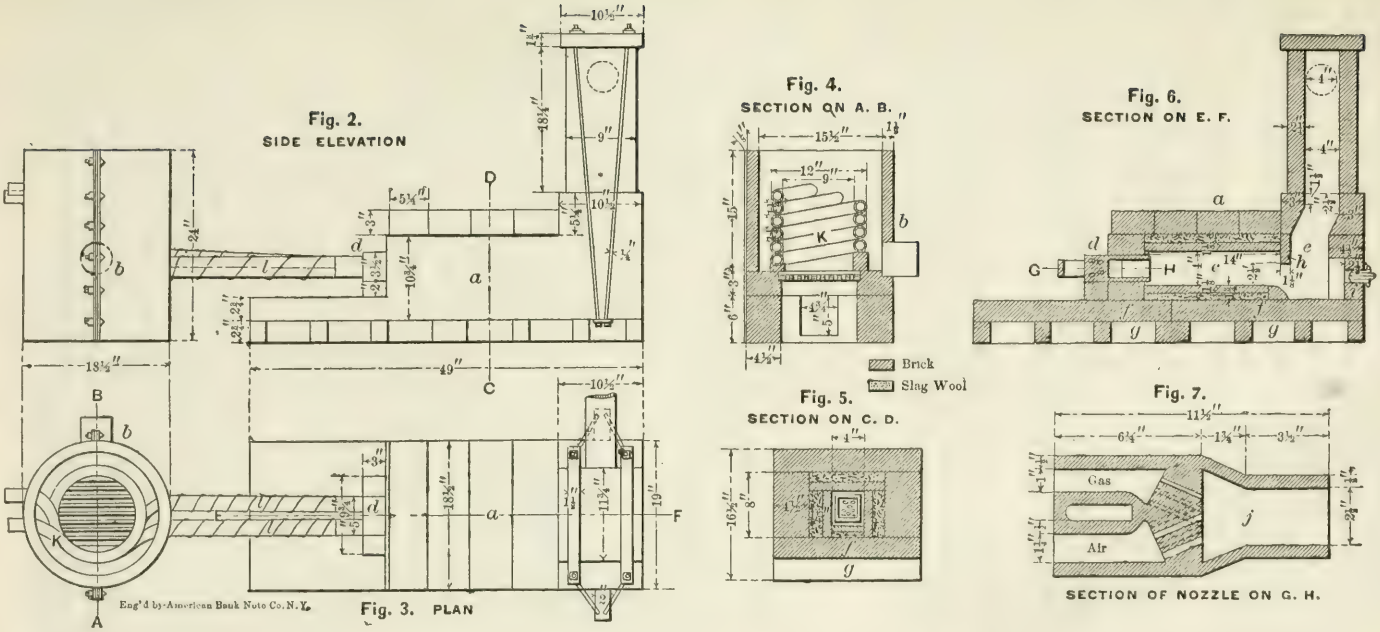
In this manner the relative refractoriness of the seven standard clays in Table I. was determined by Bischof.

B. *New Experiments.*

1. *Introductory*.—Hitherto experiments with fire-clays have always been carried on in crucibles and with extremely high temperatures. The sample has necessarily been excluded from view during the operation, and the changes could only be observed after the crucible had been taken from the fire and sufficiently cooled to

* *Ibid.*, vol. cxci., pp. 59, 150, 229; vol. cxcvii., p. 268.

† *Dingler's Polyt. Jour.*, vol. cxcvi., pp. 438, 525; vol. cxcviii., p. 396.



Hot-blast experimental furnace, fired with illuminating gas.

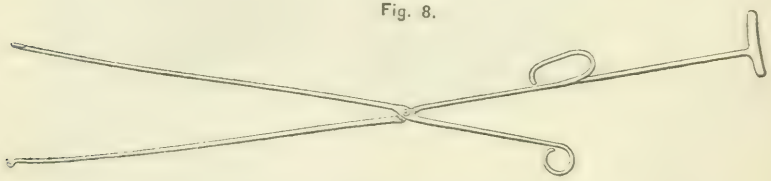
allow an examination of its contents. The temperature of a small crucible-furnace using solid fuel and blast could not be controlled with any degree of nicety, and the measurement of changes of temperature in the crucible, if not impossible, was at least not satisfactory. The writers have therefore attempted:

a. To construct a furnace in which the temperatures could be easily measured and controlled, and the samples watched while the tests were being made, and taken out or exchanged with little or no delay.

b. To devise a method of testing which did not require temperatures approximating to or surpassing the melting-point of platinum, or 1775° C.

2. *The Furnace.*—Figs. 2 to 7 represent the form of furnace finally adopted after a series of preliminary experiments. In it all the work here to be described was carried out. It consists of two parts, the furnace proper, *a*, and the heating-stove, *b*. The furnace

Fig. 8.



Cone-tongs.

is built on the same principle as the one used by Dr. W. W. Jacques, physicist of the Bell Telephone Company for high temperature work. It consists of a rectangular combustion-chamber, *c*, having a nozzle, *d*, at one end for the supply of gas and air, and a well-braced vertical chimney, *e*, at the other to carry off the products of combustion. The chamber is enclosed with thin fire-bricks (Figs. 5 and 6) which are surrounded by slag-wool to reduce the loss of heat by radiation. It rests on two tiles; the side-walls are of ordinary fire-brick laid in clay, and the roof is of four tiles laid dry. At the flue-end of the chamber there is a curtain-brick, *h*, which deflects the flame away from the roof, and thus produces a more uniform temperature than would otherwise be possible.* At the foot of the chimney is the working-door, 4½ by 5 inches, through which the samples of clay to be tested are introduced with a long pair of tongs (Fig. 8, one arm 45 inches, the other 34 inches),

* This might perhaps be done in a simpler way by drawing the products of combustion downward instead of upward.

which have proved very serviceable. The working-door is closed with a fire-brick, *i*, having a peep-hole in the center, through which a pyrometer can be inserted. It is usually closed with a clay plug. The nozzle, *d*, drawn to a larger scale in Fig. 7, is so constructed that gas and air, coming into the chamber, *j*, obliquely to one another in a number of small streams, are intimately mixed before they issue forth into the furnace, and this ensures a very satisfactory combustion. The chamber soon becomes dazzling white. To enable the nozzle to resist this intense heat, it is made of suitable proportions of raw and burnt clay, alumina and asbestos; it is air-dried for several weeks and then placed in the furnace without previous baking. The stove (*b*, Figs. 2, 3, etc.), in which air and gas are heated before they come together in the nozzle, was proposed by Prof. R. H. Richards. It consists of a sheet-iron cylinder lined with fire-clay and closed at the top with a sheet-iron cover ($\frac{1}{8}$ inch thick, not shown in the drawing) having two peepholes. Above the ash-pits is a circular cast-iron grate on which rest the two heating-coils, *k*, made of wrought-iron pipe wound together. The hot gases from the charcoal fire maintained on the grate pass up inside the coils, down on the outside, and then off through the flue, which is at the level of the grate. Thus the coils are heated outside as well as inside. Two pieces of wrought-iron pipe, *l*, enclosed in slag-wool, which is held in place by asbestos-paper and wire, are screwed on to the protruding ends of the heating-coils; they abut against the clay nozzles, the joints being made air-tight with a luting of clay and slag-wool. This furnace, like every other, shows a difference of temperature between the places where the flame enters and those where it leaves the chamber. The first step was to settle the distances from the nozzle within which the temperature was uniform. The observations made with the Le Chatelier pyrometer are given in Table II. They show a uniform temperature in the space between $4\frac{1}{2}$ and 10 inches from the nozzle. These measurements were made along the bottom of the furnace, when it was 4 inches wide and 6 inches high, and the layer of slag-wool on the bottom and the deflecting curtain had not yet been added. Hence the roof of the furnace showed at that time a higher temperature than the floor. The highest temperature attained in the furnace was 1720° C., at about 3 inches from the floor and $4\frac{1}{2}$ inches from the nozzle. A still higher temperature was expected, as in a larger furnace of similar construction, not having any provision for reducing the loss of heat by radiation, platinum had been

TABLE II.—*Measurements of Temperature in Furnace.*

Distance from Nozzle. Inches.	Degrees centigrade.			
1½	1280	1395	.....	.....
4½	.....	1420	.....	.....
5½	.....	.....	1360	1420
7½	1280	1430	.....	.....
8½	.....	.....	1370	1425
10½	1335	1420	.....	.....
11½	.....	.....	1355	1390
13½	1330	1345	.....	.....
14½	.....	.....	1210	1230

melted in two hours after starting, the gas entering the heater through a ⅝-inch pipe connected directly with a 3-inch main. The reason for the failure is the lack of gas, for although a ⅝-inch pipe is used, it passes through long and tortuous passages to reach the main. The temperatures attained, were, however, more than sufficient for the tests.

3. *The Standard of Measurement.*—Before entering upon the experiments proper, it was necessary to settle upon the standard by which any results were to be measured. Bischof's standard clays could not be taken, as the character of the deposits from which he received his material has changed, and samples of the old clays are not in the market. To overcome this difficulty, Seger got up his three-sided pyramids, going by the name of "Seger Cones." They are made with varying proportions of feldspar, kaolin, alumina, marble, quartz and ferric oxide. The first set,* ¾ inch at base and 2⅜ inches in height, contains twenty pieces, the respective numbers, Nos. 1 to 20, being impressed on them. They are intended to measure temperatures varying from 1150° C. to 1700° C., the difference between each cone and its neighbor being calculated as 28.9° C. The temperature indicated by the cone is supposed to be reached when its apex has bent down to the base.

Fig. 9 presents a series of five cones, four of which illustrate the different stages of fusion, while the fifth (the largest, which has not

* *Thonindustrie-Zeitung*, 1886, pp. 135, 145, 168, 229.

been in the fire), shows how considerable is the shrinkage that takes place upon heating. A second set of cones of the same size, Nos. 21 to 25, and a third one of smaller proportions, Nos. 26 to 36,* melting at a gradually increasing temperature, No. 36 having the composition Al_2O_3, 2 SiO_2, complete the row in the ascending temperature scale. Last year a new set of ten cones of the large size was

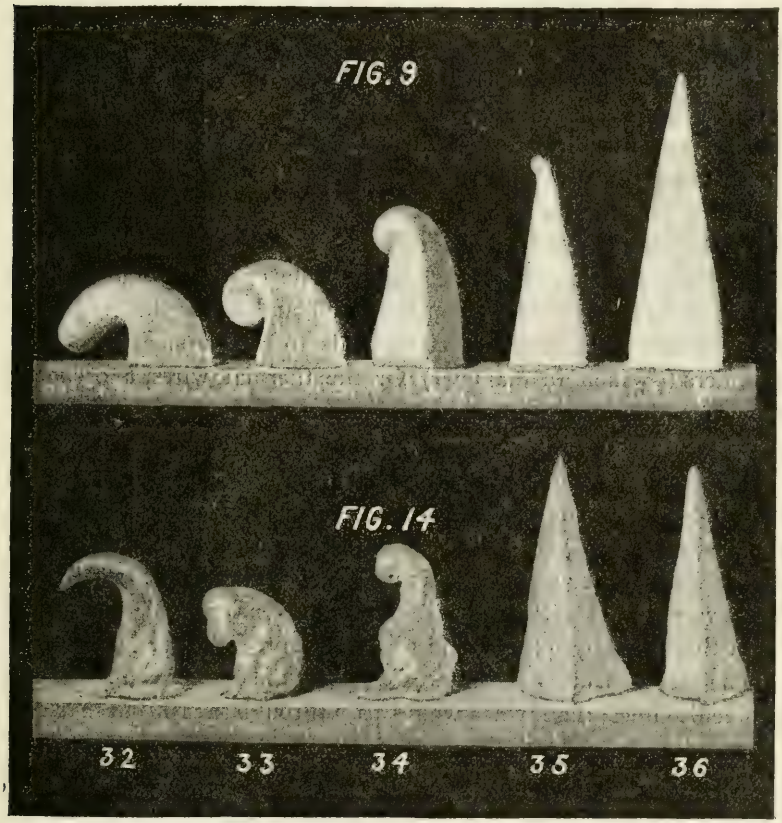


FIG. 9.—Experiments with Seger cones.
FIG. 14.—Experiments with brick-clay from Cambridge, Mass.

brought out by Cramer.† They are marked 010, 09, ... 01, 1, and are intended to measure temperatures ranging from 960° C. to 1150° C., the difference of temperature indicated by each interval being 19° C. All these cones are to be obtained at about one cent apiece from the Royal Prussian Porcelain Manufactory of

* *Thonindustrie-Zeitung*, 1886, pp. 135, 145, 168, 229; 1888, pp. 152, 162.
† *Ibid*, 1892, p. 155.

Berlin, so well known to chemists. It was at first our intention to use the high numbers of the large-sized Seger cones as standards. On melting down several of them, measuring the temperatures at the same time with the Le Chatelier pyrometer, it was found that the fusions were completed at temperatures much lower than called for by the numbers of the cones. It did not lie within the province of the work contemplated to measure accurately the melting-points* of these cones; so it was decided to adopt the Le Chatelier pyrometer, against the results of which no objection has ever been raised. This pyrometer has been repeatedly described.† The small change required to adapt the instrument to the furnace consisted in fastening the clay cylinders ($\frac{3}{8}$ inch in diameter), through which the thermal wires pass, with platinum wires to an electric-light carbon instead of inserting them in the iron pipe commonly used, as this could not be safely subjected to the temperature of the furnace, and, moreover, would have absorbed too much heat. The change was not wholly satisfactory, because the carbon burns off and leaves the binding of the wire loose. A better arrangement is to choose small-sized clay cylinders ($\frac{3}{16}$ inch in diameter) for the thermal wires, and enclose them in a thin clay tube, which can stand sudden changes of temperature.

The form adopted for the test-pieces was that of the large-sized Seger cone. As the cones are not absolutely uniform in size, the dimensions $\frac{3}{4}$ by $\frac{3}{4}$ by $\frac{7}{8}$ inch for base and $2\frac{3}{8}$ inches for the sides were selected, after measuring a number of the originals, as being the best average. It will be noted that the dimensions differ somewhat from those usually quoted, and given on a preceding page.

The clay and other substances used in the experiments were all decidedly finer than what would pass through a 100-mesh sieve. They were first mixed dry, either by hand or mechanically in a Morrell agate mortar, then moistened on a glass plate with just enough water to render them plastic, and then worked with the spatula. To the water, 5 per cent. of dextrine was added, when it was found necessary to give the cone additional firmness. The

* For the discussion, see *pro: Thonindustrie-Zeitung*, 1887, pp. 43, 84, 125, 197, Seger; p. 2, Heintz; pp. 52, 81, Hülsmann; p. 185 Hecht; and *con: Thonindustrie-Zeitung*, 1887, pp. 43, 87, Bischof; 1888, p. 61, Jochum.

† Le Chatelier, *Journal de Physique*, 2d series, vi., January, 1887; *Génie Civil*, March 5, 1887; Howe, *Eng. and Min. Jour.*, Oct. 11, 1890; Struthers, *School of Mines Quarterly*, xii., 143, and xiii., 221; *Berg- und Hüttenm.-Zeitung*, 1892, 277, 301, 310; and Roberts-Austen, "Recent Advances in Pyrometry," *Trans.*, xxiii., 419.

mould used (Figs. 10 and 11) consists of three blocks of oiled wood held together by two 3-inch clamps. In making the cones, two parts, A and B of the mould, are clamped together, the clay-mixture

Fig. 10

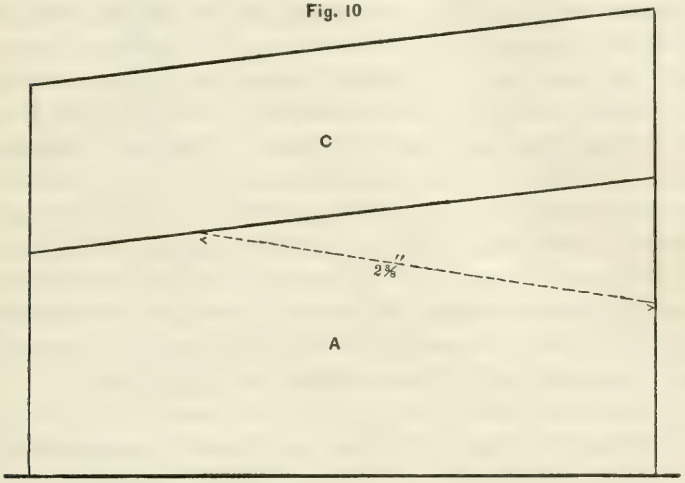
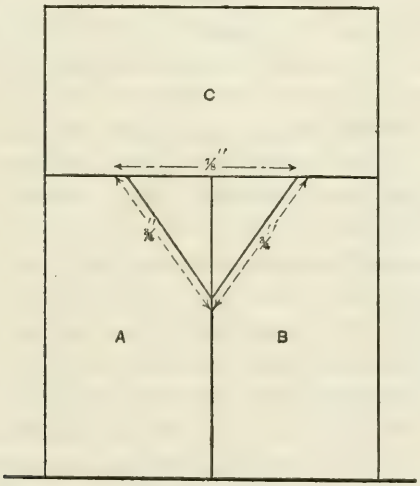


Fig. 11.



Cone-mould.

is pressed in with the spatula, the third part, C, is clamped on; the clay is pressed in firmly from the open base with a suitable piece of wood to insure a good edge and a sharp point, and the mould is then carefully taken apart. The samples were air-dried from fifteen to

twenty hours, then slowly heated on an iron plate over a Bunsen burner and placed in the furnace near the flue, where they remained until red-hot, when they were transferred to their position 4½ inches from the nozzle, where the flame would play squarely upon them. To prevent their sticking to the bottom, it was covered with a thin layer of Mt. Savage clay. The criterion of fusion was the same as that of the Seger cone, but when a sample bent over so that it was wholly below the flame, it was considered to have been fused, though the apex might not quite touch the base. After each experiment and the subsequent measuring of the temperature, the furnace was somewhat cooled down by shutting off the gas. The air was still allowed to pass through for a few minutes, and was then shut off, while the samples were taken out and replaced by new ones, when the air was turned on again and followed by the gas, the amounts of both being quickly increased to the desired degree. The proportions were so regulated that the flame should be slightly smoky.

4. *The Method.*—The method followed is based on the use of calcium carbonate as a flux. The amount of lime it is necessary to add to a weighed quantity of clay to form a fusible compound at a certain elevated temperature will form the criterion of the refractory value of the clay. Åkerman* has shown how the fusibility of silicate of lime is increased by successive additions of alumina. Cramer† got up, in 1887, a series of cones composed of Zettlitz kaolin and increasing proportions of marble, to take the place of Bischof's standard clays. While Seger‡ highly approved of the series as far as the different degrees of fusibility were concerned, he made the objection that the mixtures melted down suddenly at the critical temperatures, while the melting down of clay is slower and more gradual. The writers' experiments show that this sudden collapse of a cone takes place only with very fusible mixtures (*e. g.*, cone No. 5), and that with refractory mixtures the bending over of a cone takes place in a way that is very similar to that of Seger's own cones. As lime is a very powerful flux, and would, therefore, have a strong effect if added, even in small proportions, to a fire-clay, it seemed advisable to try and weaken this effect by diluting with silica and using the mixture instead of the stronger pure calcium carbonate.

* *Stahl und Eisen*, 1886, pp. 281 and 387; abstracted in *Jour. I. and S. Inst.*, 1886, p. 310.

† *Thonindustrie-Zeitung*, 1887, p. 197; see also Bischof, *Thonind.-Ztg.*, 1888, pp. 38 and 50.

‡ *Ibid.*, 1888, p. 153.

TABLE III.—*Analyses of Materials.*

NAME OF MATERIAL.	Al_2O_3.		SiO_2.				RO.							Loss by ignition.	O in Al_2O_3. / 3O in RO	O in SiO_2 / 3O in RO	O in SiO_2 / O in Al_2O_3.	Refract'ry Quotient.	
	Total pr. ct.	Oxygen, per cent.	Combined per cent.	Non-combined pr. ct.	Total pr. ct.	Oxygen, per cent.	MgO pr. ct.	CaO pr. ct.	Na_2O pr. ct.	K_2O pr. ct.	Fe_2O_3 pr. ct.	Total pr. ct.	Oxygen, p. c.					Bischof R. Q. B.	Seger R. Q. S.
Fire clay, Mt. Savage, Md.*	29.10	13.56	28.35	31.84	60.19	32.08	trace.	.....	0.	03	0.89	0.92	0.18	9.90	25.11	59.41	2.36	10.64	35.81
Kaolin, Blanford, Mass.†	31.76	14.80	.....	.....	52.03	27.73	0.54	trace.	trace.	trace.	trace.	0.54	0.22	15.55	22.42	42.01	1.87	11 99	34.45
Brick clay, Cambridge, Mass.‡	28.90	13.46	18.04	30.95	48.99	26.11	3.66	7.10	4.73§	Cl0.13 SO_3 0.20	4.32	19.81	5.57	2.98	0.80	1.56	1.93	0.41	1.22
Quartz, Berkshire, Mass.	.....	.....	.....	.....	99.88	53.24	.....	.....	.....	.....	0.12‖	0.12	0.02	.....	.....	.....	.....	.....	.....
Alumina, Syracuse, N. Y.¶	98.46	45.88	.....	.....	0.25	0.13	.....	.....	0.50**	.....	0.04	0.54	0.13	0.75	.....	.....	.....	.....	.....
Calcium carbonate	.....	.....	.....	.....	.....	.....	.....	56	.....	.....	.....	.....	.....	44	.....	.....	.....	.....	.....

* Kinnicut and Rogers, *Journal of Analytical and Applied Chemistry*, vol. v., page 542 ; *Eng. and Mining Journal*, December 12, 1891.

† Norton, *Technology Quarterly*, vol. iii., page 231.

‡ Card—Thesis in Geol. Department Massachusetts Institute Technology, 1892.

§ Equivalent to 7.17 K_2O.

‖ Assumed ; previous analysis having shown the main impurity to be iron.

¶ Solvay Process Co.

** Equivalent to 0.75 K_2O.

Experiments of this character were carried on with clay from Mount Savage, Md., and with kaolin from Blanford, Mass. A third set of experiments was made with a common brick-clay from Cambridge, Mass. The method here followed was that of Bischof (see page 50) of toning up a clay with alumina, the strength of which has been weakened by silica.

Table III. gives the composition of the materials used in the experiments.

5. *Experiments with Fire-clay from Mount Savage, Md.*—The clay experimented upon belongs to the hard variety;* it has a conchoidal fracture; its color varies from light gray to dark chocolate-brown; it disintegrates very slowly when exposed to the air and is plastic only when ground very fine. It is infusible before the blowpipe.

The results of the experiments with the light variety are brought together in Table IV. and illustrated by Fig. 12.

Experiments Nos. 1 to 4, in which pure calcium carbonate was added to the clay, form a good series; for the fusibility increases regularly with the additions of the flux. Cones Nos. 1 and 2 are quite fused, No. 3 is just fused, while No. 4 is only glazed, and not bent over. The critical mixture contains, however, only between 5 and 10 per cent. of the flux, which is a very small quantity of reagent to produce such a decided result.

Experiments Nos. 5 to 8, where equal parts of silica and calcium carbonate are used, show little improvement over those with calcium carbonate alone. That cone No. 5 should have melted as soon as full gas had been turned on, was to be expected from its low refractory quotient. It is the only example of the sudden fusion of the clay-lime mixture, objected to by Seger, that occurred in the experiments. The critical mixture contains from 10 to 20 per cent. of the flux. The temperatures are lower than might have been expected from the refractory quotient.

Experiments Nos. 9 to 11, with silica to calcium carbonate as 3 to 1, make a decidedly better showing than those with silica and calcium carbonate in equal parts. The critical mixture contains between 20 and 30 per cent. flux, and while the temperatures are higher than before, viz., 1510° C. *vs.* 1440° C., the difference is not great enough to invalidate the comparison.

Experiments Nos. 12 to 15 represent the mixtures of clay with the flux, 5 silica to 1 calcium carbonate, which is a further step in

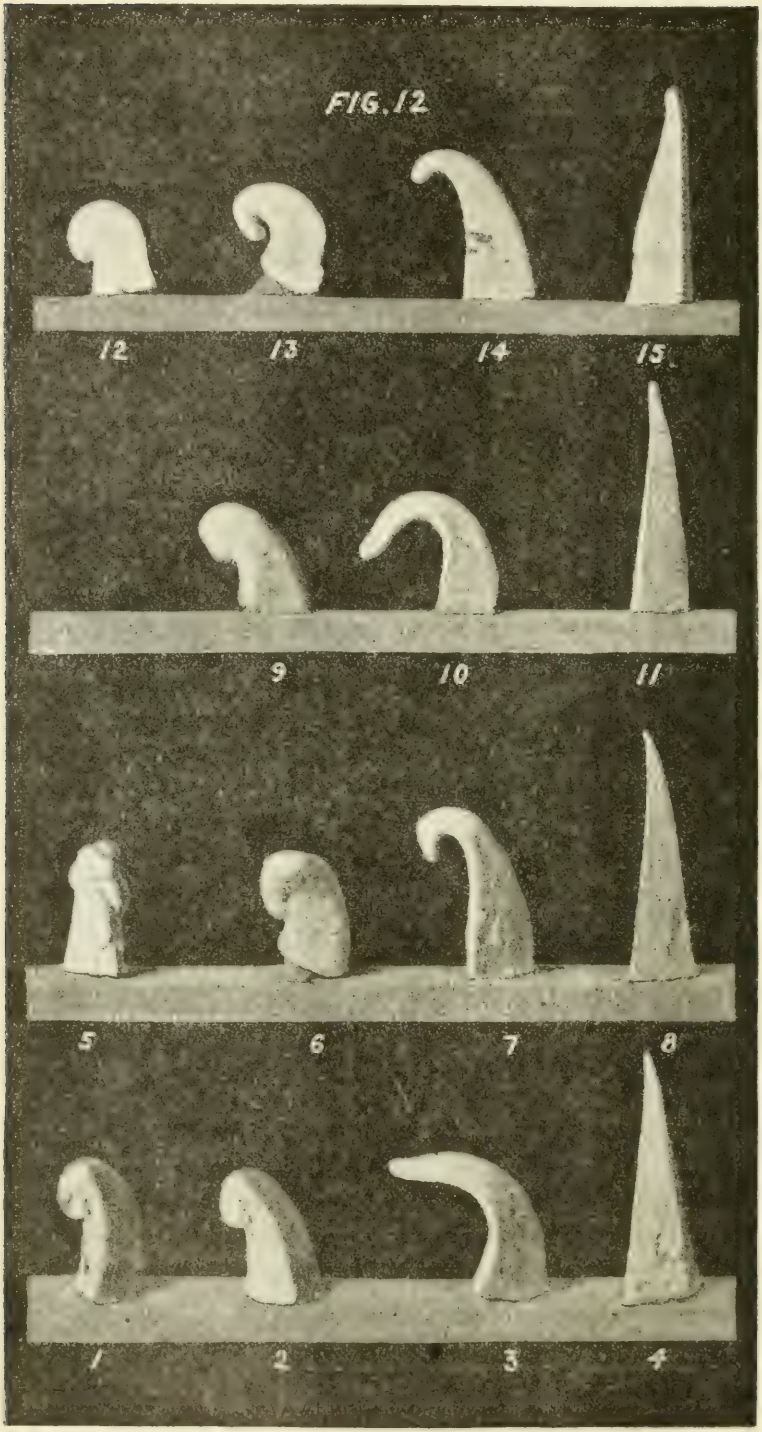
* See R. A. Cook, *Trans.*, xiv., 698.

advance. Cone No. 12 shows good fusion; No. 13 is less completely bent; No. 14 is just a little inclined; and No. 15 is only glazed, and beaded at the apex. The critical mixture contains from 30 to 40 per cent. of flux.

TABLE IV.—*Experiments with Fire-Clay from Mount Savage, Md.*

No. of Experiment and Cone.	Components of Mixture.			Composition of Mixture.						Refractory Quot'nt.		Temperature, ° C.	REMARKS.
	Clay, pr. ct.	Quartz, pr.c.	Calcium carbonate, pr.c.	Al_2O_3 pr. ct.	O per cent.	SiO_2 pr. ct.	O per cent.	RO per ct.	O per cent.	Bischof R. Q. B.	Seger R. Q. B.		
.....	100	.....	.....	29.10	13.56	60.19	32.08	0.92	0.18	10.64	35.81	.....	Added for the sake of comparison.
1	80	.....	20	23.28	10.85	48.15	25.66	11.94	3.34	0.45	1.54	1470	Were together in the furnace, and were removed together.
2	85	.....	15	24.73	11.53	51.16	27.26	9.18	2.55	0.63	2.14	1470	No. 1 fused before No. 2.
3	90	.....	10	26.19	12.20	54.17	28.87	6.43	1.76	0.97	3.28	1580	Were together in the furnace, and were removed together.
4	95	.....	5	27.65	12.88	57.18	30.47	3.67	0.97	1.87	6.30	1580	No. 3 fused, 4 only superfic'ly glazed.
5	40	30	30	11.64	5.42	54.04	28.80	17.20	4.88	0.07	0.43	.....	Melt'd at once when full gas was admit'd.
6	60	20	20	17.46	8.13	56.09	29.89	11.77	3.31	0.22	1.04	1340	Melted quickly.
7	80	10	10	23.28	10.85	58.14	30.99	6.35	1.75	0.72	2.78	1440	Were together in the furnace: No. 7 fused slowly, 8 but slightly glazed.
8	90	5	5	26.19	12.20	59.16	31.53	3.63	0.96	1.63	5.10	1440	
9	60	30	10	17.46	8.13	66.07	35.22	6.19	1.71	0.36	1.95	1420	Fused slowly.
10	70	22.5	7.5	20.37	9.49	64.61	34.43	4.87	1.33	0.65	3.02	1510	Were together in the furnace; No. 10 fused slowly, 11 is glazed and has a rounded point.
11	80	15	5	23.80	10.85	63.13	33.65	2.85	0.81	1.44	5.90	1510	
12	40	50	10	11.64	5.42	74.02	39.45	6.03	1.68	0.14	1.22	1470	Together in fur.; temperatures are av'r'ges of 2 series of exp'ts; both fused slowly: 12 is seen to be more fused than 13.
13	50	41.7	8.3	14.55	6.78	71.74	38.24	4.74	1.34	0.29	1.98	1470	
14	60	33.3	6.7	17.46	8.13	69.37	36.97	4.34	1.18	0.50	2.80	1600	Together in furn.; No. 14 somewhat fused, 15 glazed & beaded at apex.
15	70	25	5	20.37	9.49	67.10	35.76	3.47	0.93	0.90	4.30	1600	

The proportion of 5 silica to 1 calcium carbonate is the highest that was used. A mixture still richer in silica than this might, possibly, be better for fire-clays less refractory than that from Mount Savage; but 5 silica to 1 calcium carbonate is sufficient, so long as the mixture fulfils the necessary condition of being weak enough to be used in considerable quantities without having too great an



Experiments with fire-clay from Mt. Savage, Md.

effect. This is important, as there must be difference enough in the amounts added to the various clays to afford a satisfactory scale for measuring their fusibility.

TABLE V.—*Experiments with Kaolin from Blanford, Mass.*

No. of Experiment and Cone.	Components of Mixture.			Composition of Mixture.						Refractory Quotient.		Temperature, ° C.	REMARKS.
	Kaolin, per cent.	Quartz, p. ct.	Calcium carbonate, p. ct.	Al_2O_3 pr. ct.	O per cent.	SiO_2 per ct.	O per cent.	RO per ct.	O per cent.	Bischof R. Q. B.	Seger R.Q.S.		
.....	100	.....	.....	31.76	14.80	52.03	27.73	0.54	0.22	11.99	34.45	.....	Added for sake of comparison.
16	85	.....	15	26.99	12.58	44.22	23.57	8.86	2.58	0.86	2.49	1490	Were together in furnace. No. 16 well fused; No. 17 less so.
17	90	.....	10	28.58	13.32	46.83	24.96	6.09	1.79	1.32	3.80	1490	
18	95	.....	5	30.17	14.06	49.43	26.34	3.31	1.00	2.50	7.18	1640	Only well bent.
19	70	15	15	22.23	10.36	51.40	27.39	8.79	2.56	0.51	1.85	1370	Fused.
20	80	10	10	25.41	11.84	51.61	27.51	6.04	1.77	0.96	3.18	1490	Fused.
21	90	5	5	28.58	13.32	51.82	27.62	3.29	1.00	2.14	6.58	1610	Only beaded at the apex.
22	50	37.5	12.5	15.88	7.40	63.47	33.83	7.31	2.12	0.25	1.45	1330	Fused.
23	60	30	10	19.06	8.88	61.18	32.61	5.96	1.74	0.46	2.16	1500	Fused.
24	70	22.5	7.5	22.23	10.36	58.89	31.39	4.60	1.36	0.83	3.37	1560	Fused.
25	80	15	5	25.41	11.84	56.60	30.17	3.25	0.98	1.58	5.61	1660	Only well bent.
26	60	33.33	6.67	19.06	8.88	64.51	34.38	4.09	1.20	0.63	3.10	1340	Fused.
27	70	25	5	22.23	10.36	61.39	32.72	3.21	0.96	1.13	4.73	1380	Fused half down.
28	80	16.67	3.33	25.41	11.84	58.53	31.06	2.32	0.71	2.11	7.67	1580	Only slightly beaded at apex.
29	90	8.33	1.67	28.58	13.32	55.14	29.39	1.43	0.46	4.37	14.02	1580	Unaffected.

6. *Experiments with Kaolin from Blanford, Mass.*—The kaolin from Blanford, Mass.,* used in these experiments is soft, pinkish-white, and slightly plastic. It has a high degree of purity as seen by the analysis (Table III.), and is infusible before the blow-pipe. The

* Crosby, *Technology Quarterly*, vol. iii., p. 228.

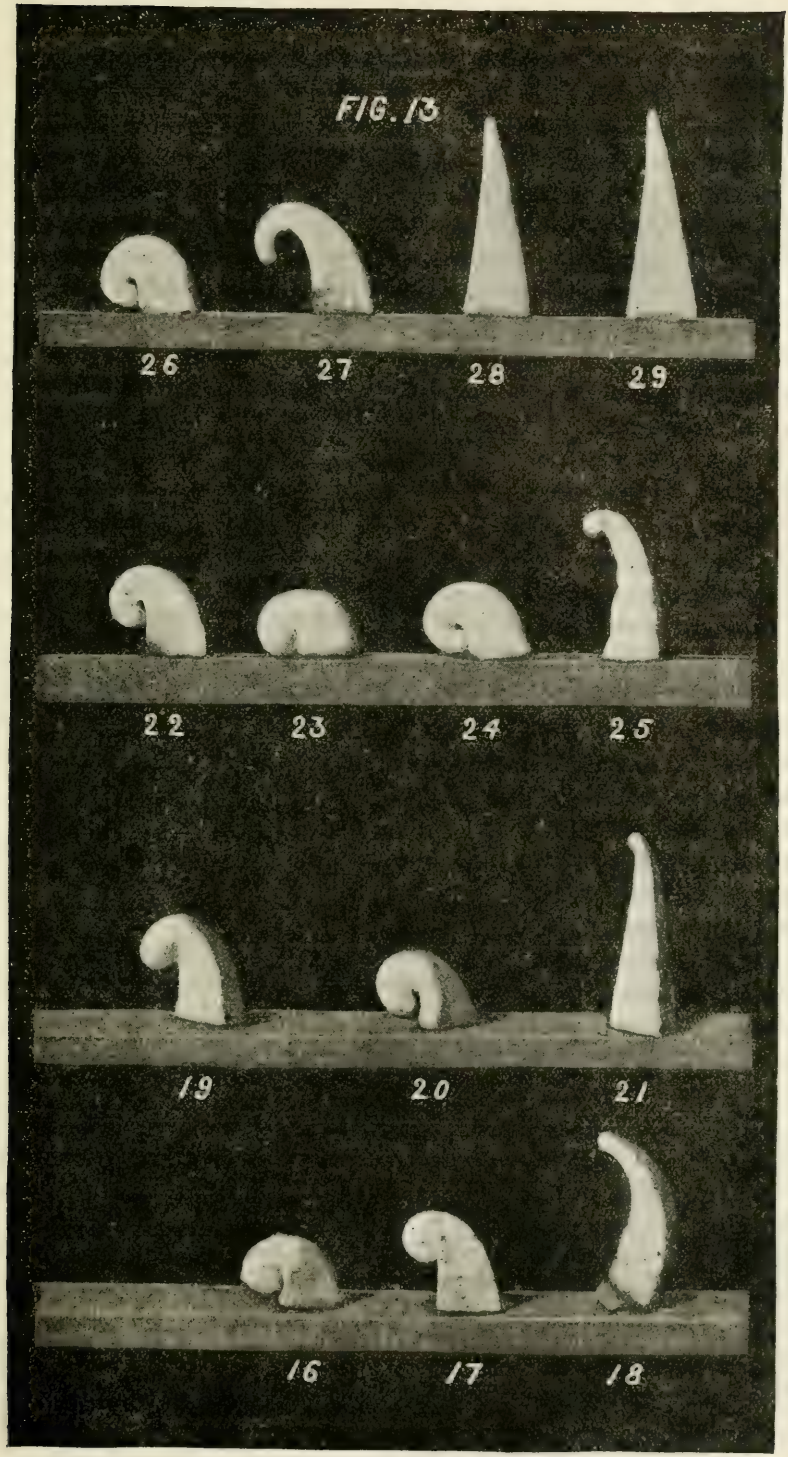
sample received, weighing about twelve pounds, was a mixture of friable kaolin and more or less disintegrated crystals of mica and quartz. As it was desired to have the purest kaolin obtainable, the original sample was dried and worked without grinding through a series of screens, to remove the mica and quartz; that only which had passed through a 100-mesh sieve (25 per cent. of the original amount) being reserved for the experiment after it had received a final grinding in a Morrell agate mortar.

The experiments Nos. 16 to 29 were carried on in the same manner as those with the clay from Mount Savage. The results obtained are brought together in Table V., and the cones are represented by Fig. 13. If they are compared with those obtained with Mount Savage clay it will be seen, first, that the critical mixtures in the corresponding cases contained the same amount of flux, and secondly, that those having a higher refractory quotient require also a higher temperature. An exception to this is found with the flux containing five silica and one calcium carbonate where cones Nos. 26 to 29, with a large refractory quotient, show lower temperatures than cones Nos. 12 to 15, with a small one. The explanation is wanting.

7. *Experiments with Brick-Clay from Cambridge, Mass.*—This clay is largely used in the brickyards of Cambridge. It is soft, light gray and plastic, shows a considerable number of impurities and is readily fusible. It was chosen to test in the new furnace Bischof's method of toning up a low-grade clay with a mixture of equal parts of silica and alumina, so it shall fuse only at an elevated temperature. The experiments are recorded in Table VI., and of the cones obtained, Nos. 30 to 36, only Nos. 32 to 36 are shown in Fig. 14.

Experiments Nos. 30 and 31 show how readily fusible the clay is; in fact, the specimens upon heating quickly swelled up into a spongy mass; even with 20 per cent. of stiffening ingredient, fusion occurred at 1300° C., and it required from 40 to 50 per cent. of the ingredient to enable the clay to resist a high temperature.

8. *Conclusion.*—In the foregoing experiments a definite standard temperature has not been sought. It seemed of greater importance to study first the behavior of several series of mixtures of clay, silica and lime at elevated temperatures, and to measure these. Of the fluxes tested, the mixtures of three silica to one calcium carbonate and five silica to one calcium carbonate have proved valuable; and further series of tests with a number of analyzed refractory clays will be needed to decide whether the proportion five silica to one calcium carbonate, which has so far proved the better of the two,



Experiments with kaolin from Blanford, Mass.

TABLE VI.—*Experiments with Brick-Clay, from Cambridge, Mass.*

No. of Experiment and Cone.	Components of Mixture.			Composition of Mixture.						Refractory Quotient.		Temperature, C.	REMARKS.
	Brick-clay, per cent.	Quartz, p.ct.	Alumina, per cent.	Al_2O_3 pr. ct.	O per cent.	SiO_2 per ct.	O per cent.	RO per ct.	O per cent.	Bischof, R. Q. B.	Seger, R.Q.S.		
.....	100	.....	.....	28.90	13.46	48.99	26.11	19.81	5.57	0.41	1.22	.....	Added for the sake of comparison.
36	40	30	30	41.09	19.15	49.66	26.47	8.12	2.27	2.14	4.95	?	Unaffected.
35	50	25	25	39.06	18.17	49.53	26.39	10.23	2.85	1.46	3.58	1610	Glazed on outside, but not fused.
34	60	20	20	37.03	17.25	49.42	26.34	12.02	3.37	1.11	2.82	1610	Fused.
33	70	15	15	34.99	16.30	49.31	26.28	13.96	3.92	0 86	2.24	1450	Fused.
32	80	10	10	32.96	15.35	49.20	26.22	15.91	4.47	0.67	1.81	1300	Fused.
31	85	7.5	7.5	31.94	14.88	49.15	26.19	16.88	4.74	0.59	1.64	?	Fused at once when half the gas was turned on.
30	90	5	5	30.93	14.41	49.09	26.16	17.86	5.02	0.52	1.48	?	

can maintain its position or will have to yield to a mixture that is more siliceous. In these final experiments it will be necessary to maintain a definite temperature (which is easily done with the furnace) and make it the standard. Experience so far seems to show that 1500° C. is the best. It is also proposed to make a parallel series of tests by Seger's direct method, and compare the results with those from the indirect silica-lime method.

THE DETERMINATION OF PHOSPHORUS IN COAL AND COKE.

BY JACOB LYCHENHEIM, SWEDELAND, PA.

(Virginia Beach Meeting, February, 1894.)

IN connection with the manufacture of special Bessemer pig, containing .030 per cent. or less of phosphorus, it was found that the phosphorus in anthracite varied so greatly that it was necessary to sample and analyze each car-load. This, of course, in addition to our routine work, involved a considerable amount of extra trouble, and some experiments were made, with the view of cutting down the time and labor of the determination to a minimum.

As there seemed to be no question that all the phosphorus is to be found in the ash, the old method of fusing 5 grammes of coal with 40 grammes of carbonate of soda and 40 grammes of potassium nitrate was not tried. It involved too much attention in preventing the fused mass from overrunning the crucible.

Method A, described by Blair in his *Chemical Analysis of Iron* (2d ed., p. 278) was taken as a standard, with which the other results could be compared. It was modified by precipitating the phosphorus as phospho-molybdate in the nitric acid solution of the basic acetate obtained by evaporating the hydrochloric acid solution to 10 c.c.; adding 35 c.c. of concentrated nitric acid and evaporating again to 10 c.c.; diluting to about 60 c.c.; washing into a shaking-flask; neutralizing, precipitating, and reducing in the well-known way. To save time, the fused residue was dissolved in hydrochloric acid instead of water.

Method B, the method described by Mr. Frank Crobaugh (*Journal of Analytical and Applied Chemistry*, vol. vii., page 223) was first tried, as most likely to give accurate results with the minimum labor. The results as shown by the table given in this paper, agreed fairly well when the amount of phosphorus was considerable; but when the amount was small, the results were uniformly too low. The method was used exactly as described. The ash of 5 grammes was fused with 5 grammes of sodium carbonate and $\frac{1}{2}$ gramme of potassium nitrate. The fused mass was dissolved in 150 c.c. of warm HNO_3 (sp. gr. 1.135) and filtered from flotant silica; 40 c.c. of molybdate solution was added, and the further treatment was as directed by Emmerton. It was thought that failure to precipitate all the phosphorus was due to the large amount of free nitric acid present and the absence of ammonium nitrate. Hence, in experiments Nos. 7, 8, 9 and 10, the solution was neutralized with ammonia before precipitation, and was then rendered acid, so that the solution contained about 7 or 8 c.c. of free nitric acid. In every case the results were abnormally high, a precipitation of either silico-molybdate or molybdic acid coming down. By method C the same curious results were obtained.

Method C.—In this variation the ash of 5 grammes was fused with 5 grammes of carbonate of soda and a pinch of potassium nitrate; the fused mass was dissolved in 40 c.c. of dilute hydrochloric acid in a No. 4 beaker, and diluted to about 350 c.c.; 10 c.c. of a strong ferric-chloride solution was added; and the phosphorus, precipitated as ferric phosphate by ammonia, was filtered in a 15 cm. rapid filter.

The precipitate was dissolved on the filter in 35 c.c. of nitric acid (sp. gr. 1.135). In all the experiments, except Nos. 7 and 8, the solution was neutralized as in method B; and in every case the results were very high. The yellow precipitate had a flaky, non-crystalline appearance. As this method did not seem to offer any particular advantage over method D, which we were using at the time, no further work was done upon it.

Method D.—This method is, I believe, the one in general use, and I have always found it to give most satisfactory results. The ash of 5 grammes is fused and dissolved as in method C, and the hydrochloric acid solution is evaporated to moist dryness, 30 c.c. of strong nitric acid is added and evaporated until all the brown fumes have disappeared (usually to about 10 c.c.). The solution is then diluted, filtered from the silica, neutralized, and precipitated with molybdate solution, etc.

The differences shown by Nos. 1 and 2 under methods D and E, as compared with A, are hard to explain. The insufficiency of the samples prevented a duplication of the work. D and E, which were done at a considerably later time than A, agreed quite closely.

Method E.—I had been under the impression that fusion of the ash or residue was necessary in all cases; but wishing to ascertain exactly what proportion was dissolved out by hydrochloric acid, and what remained in the residue, I tried the experiment of treating the ash exactly like an ore, viz., transferring it to a 5-inch casserole; adding 40 c.c. of strong HCl; evaporating to 10 c.c.; then adding 40 c.c. of nitric acid (sp. gr. 1.42) and evaporating until all brown fumes had passed off and the solution contained about 20 or 25 c.c. The nitric acid solution has a strong tendency to "bump;" and care must be taken to keep the contents of the casserole in motion. The results showed *that the phosphorus was almost completely soluble in hydrochloric acid and only a trace remained in the residue.*

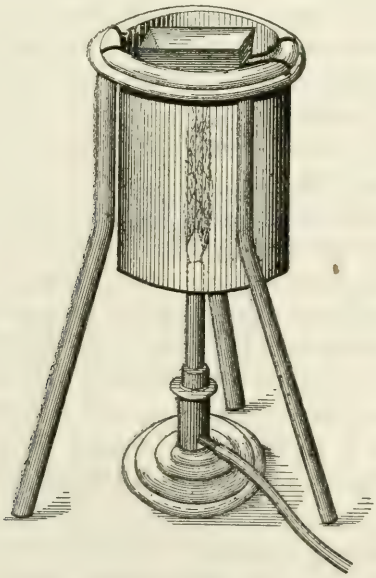
This, then, was a solution of the problem by which results could easily be obtained in from an hour to an hour and a half, with little labor and attention on the part of the operator. It is just as satisfactory and gives just as concordant results as the longer methods.

In all this work we have been using a very simple arrangement, by which the combustion of the carbon was considerably hastened. In fact, it was found much more satisfactory than the large fusion crucibles we had previously employed. Its comparative cheapness enabled us to run six analyses at a time, and also did away with the expensive necessity of laying in a supply of large crucibles. It con-

Table of Percentages of Phosphorus, as Determined by Different Methods.

Sample No.	Method A.	Method B.	Method C.	Method D.	Method E.
1	.088	.090	.162	.099	.098
2	.078	.075	.143	.091	.089
3	.004	No ppt.	.020	.006	.006
4	.012	"	.020	.012	.012
5	.018	.066	.031	.017	.020
6	.075	.076	.090	.075	.081
7	.015	.049	.015	.012	.012
8	.062	.160	.084	.064	.064
9	.020	.057	.034	.017	.019
10	.013	.036	.....	.012	.015

sists of a boat two inches square and $\frac{1}{2}$ inch deep made from .002 platinum foil. Care should be taken in making the boats that the corner flaps fit tightly, so that none of the ash will be lost by getting



Platinum Boat for Carbon-Combustion.

into the interstices. A tripod, Erdman chimney, and two pieces of thin platinum wire complete the arrangement, which is shown in the accompanying figure. The wires should be so bent as to be about $\frac{3}{4}$ inch below the top of the chimney.

The heat applied for the first five minutes should be a low red, in order that none of the coal shall be lost in the escape of the volatile matter. After that the gas should be turned on full, and a bright red heat maintained. It is not necessary that the sample should be ground very finely. By constant stirring we have effected the combustion of the carbon in half an hour, and with an occasional stirring it should never take more than an hour.

My thanks are due to Mr. Arthur Sharp for aid in the analytical work.

COAL-SECTIONS DEVELOPED BY RECENT OPERATIONS IN WISE COUNTY, VIRGINIA.

BY FRANKLIN BACHE, ABINGDON, VA.

(Virginia Beach Meeting, February, 1894.)

It is appropriate to the place of meeting, and of interest to those members of the Institute whose attention has been more or less directed to Virginia during the past eleven years, to know the result of recent developments of the Virginia coal-field.

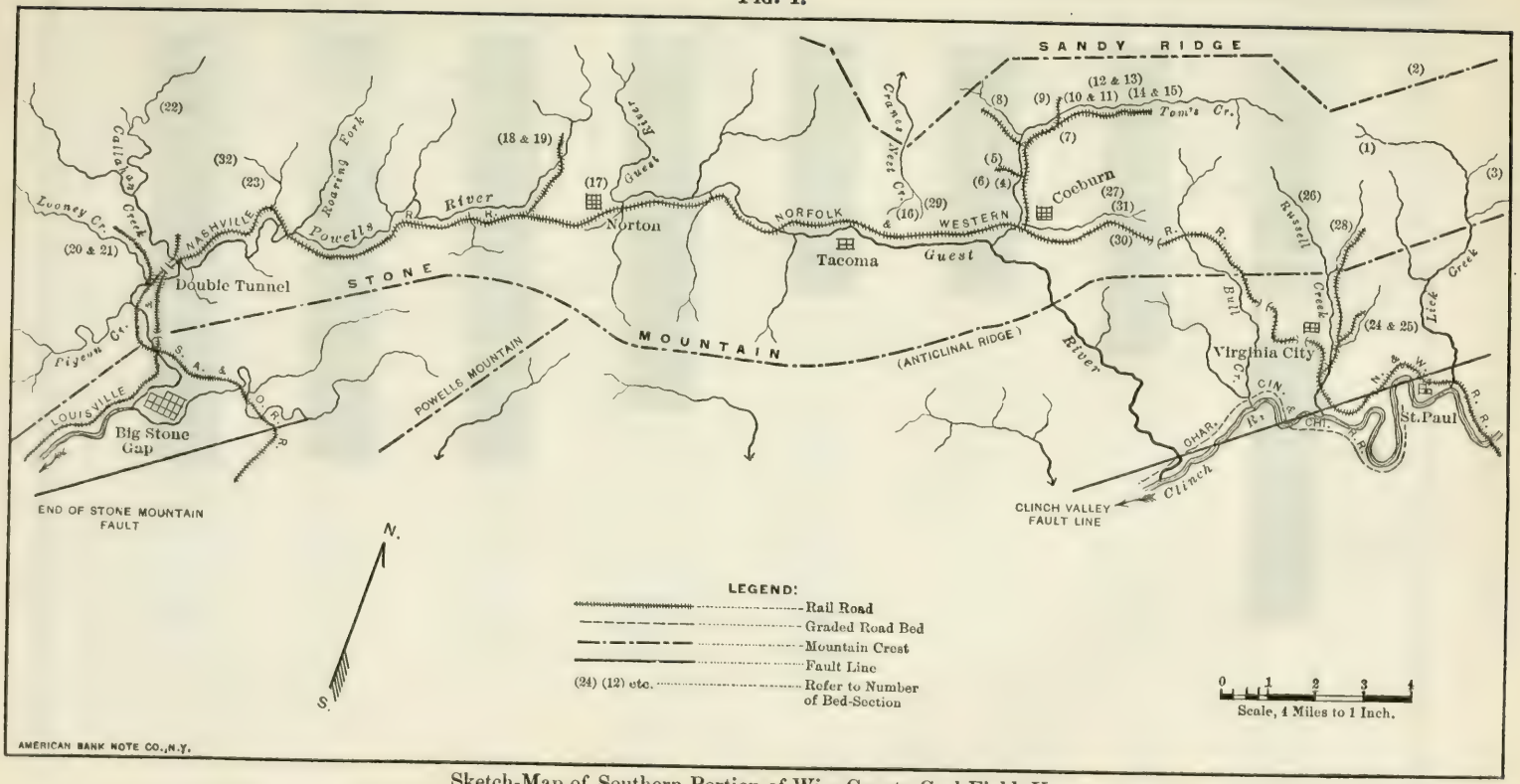
While, during the late "boom," the attention of the industrial pioneers of Virginia was mainly directed to iron-ore deposits and manufacture, it is safe to say that the coal to which, at that time, comparatively little attention was turned, and which did not directly foster a growth of "boom" towns, will prove the most reliable source of mineral wealth in the State.

At the time of the last Virginia meeting of the Institute, held at Roanoke, in 1883, the Wise county coal-field was untouched by a railroad, and its economic existence was scarcely considered.

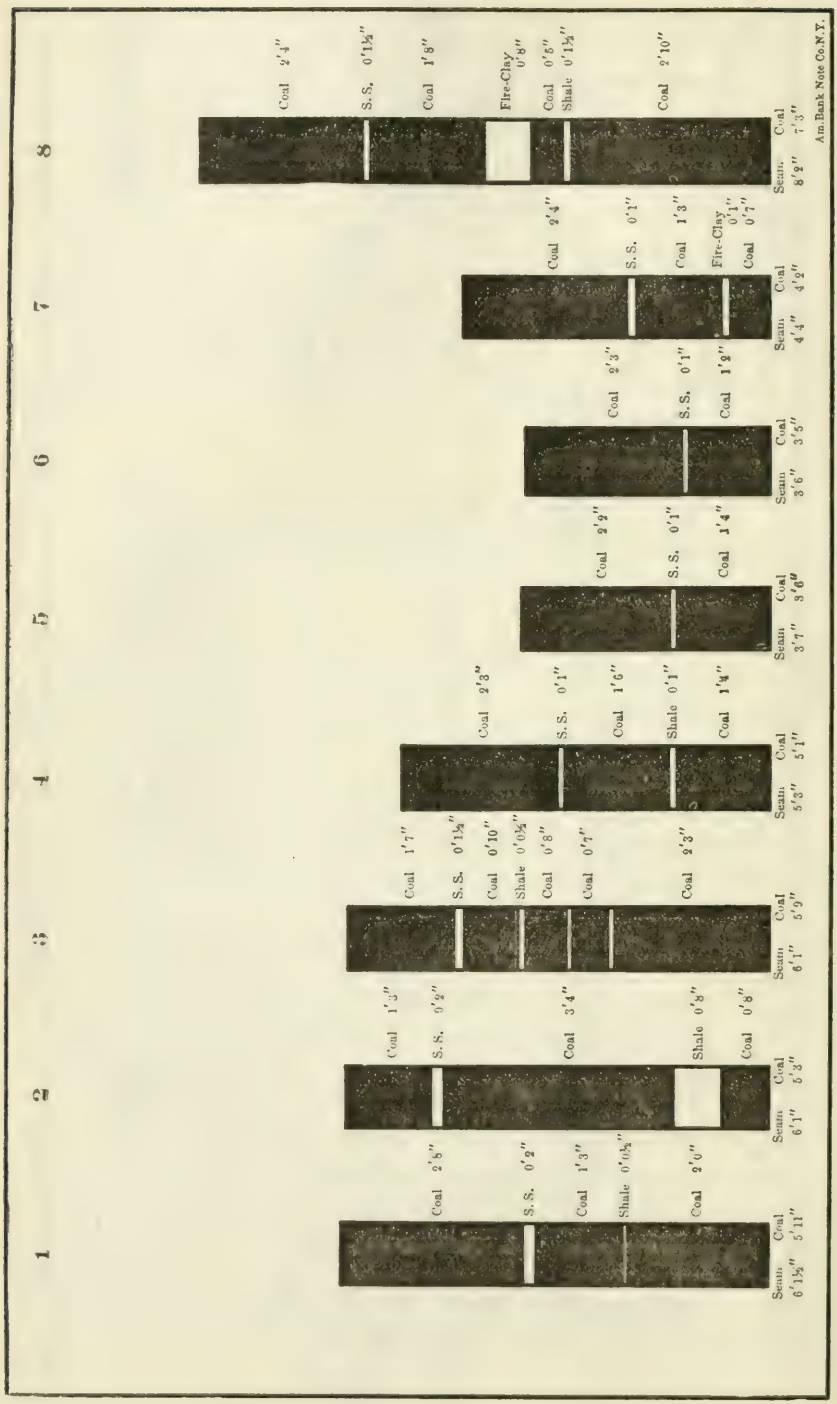
Although, in the meantime, a number of reconnoissances had been made, and a fair estimate of the value of the field formed, it was not until 1891, when the Norfolk and Western and the Louisville and Nashville railroads, respectively, completed their Clinch Valley and Norton extensions to a junction at Norton, Wise county, that any development took place, and not until 1892 were shipments of any importance made.

The coal-measures of Virginia are confined to the seven counties of Buchanan, Tazewell, Dickenson, Russell, Scott, Wise, and Lee,

FIG. 1.



Sketch-Map of Southern Portion of Wise County Coal-Field, Va.



Coal-Sections in Wise County, Va.

and are separated from the eastern Kentucky field by the Pine Mountain fault-ridge, as far east as the Breaks of Sandy, east of which point the measures in the two States are connected by an anticlinal ridge, which brings up the conglomerate at the top of No. XII.

The approximate area of these measures, as comprised in all or part of the above counties, is 2200 square miles. Of this area, it is estimated that two-fifths is of no economic importance, the coal being disturbed by its proximity to the fault-lines, or by the lower and comparatively barren measures being brought to the surface.

In Wise county, the area of economic importance is bounded on the south by the Norfolk and Western and Louisville and Nashville railroads. The total area of carboniferous measures in the county is about 540 square miles. Of this, about 400 square miles is of practical importance; the remainder being in disturbed areas.

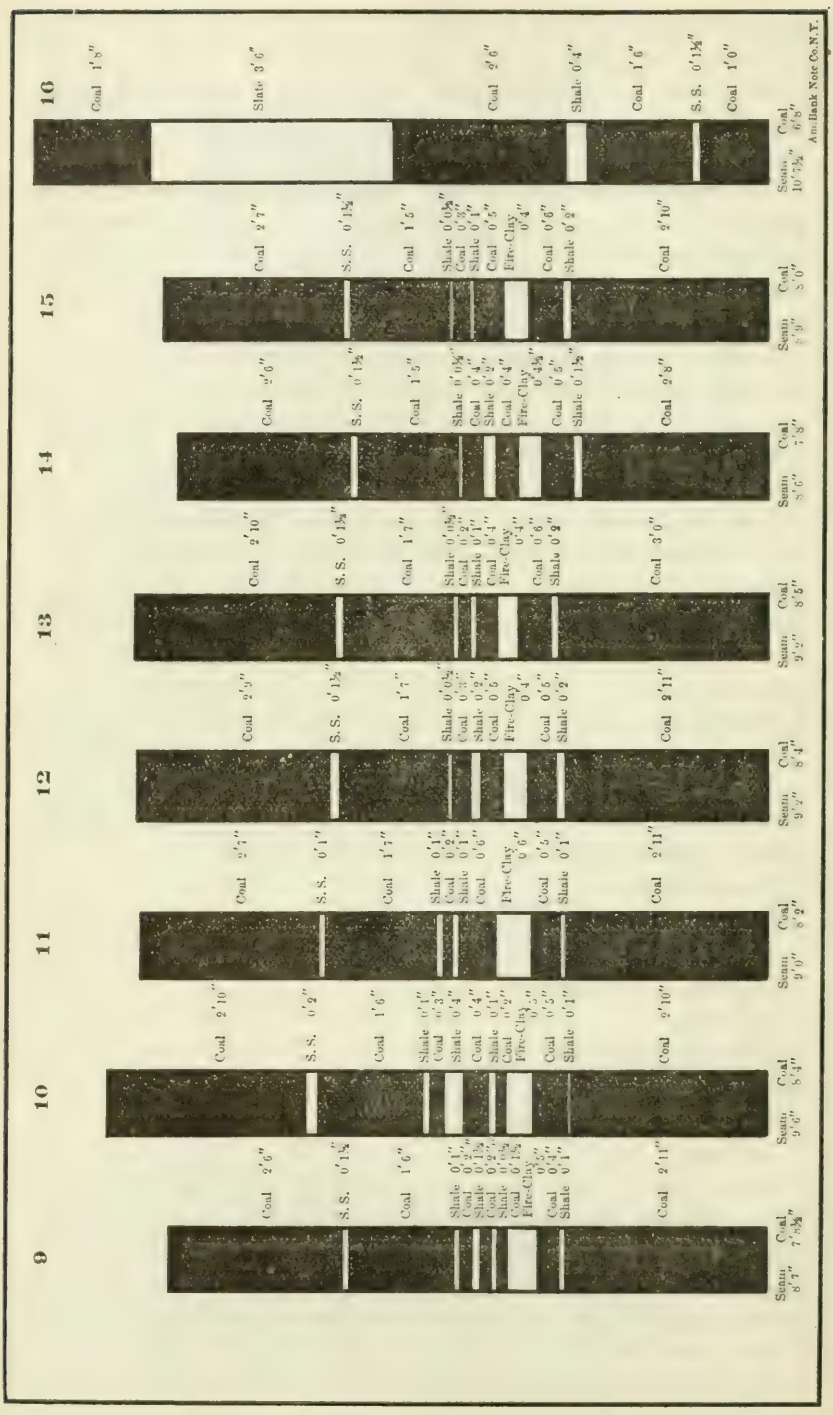
The development has all been made along the railroad lines that skirt the lower side of the field for about 36 miles, and on about 12 miles of laterals that have been constructed north from the main lines at various points. In 1893, 149,600 tons of coal were shipped from fourteen operations. Of this, approximately 129,000 tons were mined on the Norfolk and Western railroad, 600 tons on the Louisville and Nashville railroad, and 20,000 tons on the South Atlantic and Ohio railroad (running south from Big Stone Gap).

The seams of importance in the field are, in descending order: The Upper Banner, Lower Banner, Kennedy, Imboden, and Jawbone. They are separated by the following intervals:

Upper Banner.
Interval, 75 to 125 feet.
Lower Banner.
Interval, 75 to 200 feet.
Kennedy.
Interval, 150 to 200 feet.
Imboden.
Interval, 75 to 125 feet.
Jawbone.

In the eastern and central portions of the field, the Upper and Lower Banner, Kennedy and Jawbone seams have attracted the greatest attention. In the western portion, the Imboden is the only seam mined. In the center of the field, the seam mined is probably the Upper Banner, although its identity with the Upper Banner in the eastern part of the field has not been definitely determined.

The developments made in the course of mining operations have



Coal-Sections in Wise County, Va.

shown the coal-measures to be undisturbed, except when close to the Clinch Valley fault-line, and the seams to have maintained practically the same bed-section, when driven well under cover, as was indicated by the preliminary outcrop-openings.

The railroads skirting the southern edge of the field are, consequently, near the fault-line and its attendant anticlinal. Hence, the dips of the coal have been steeper in the mines first opened near the main railroad-lines than in the mines on the laterals built to the north. The regularity of the bed-section has also been affected by the same cause.

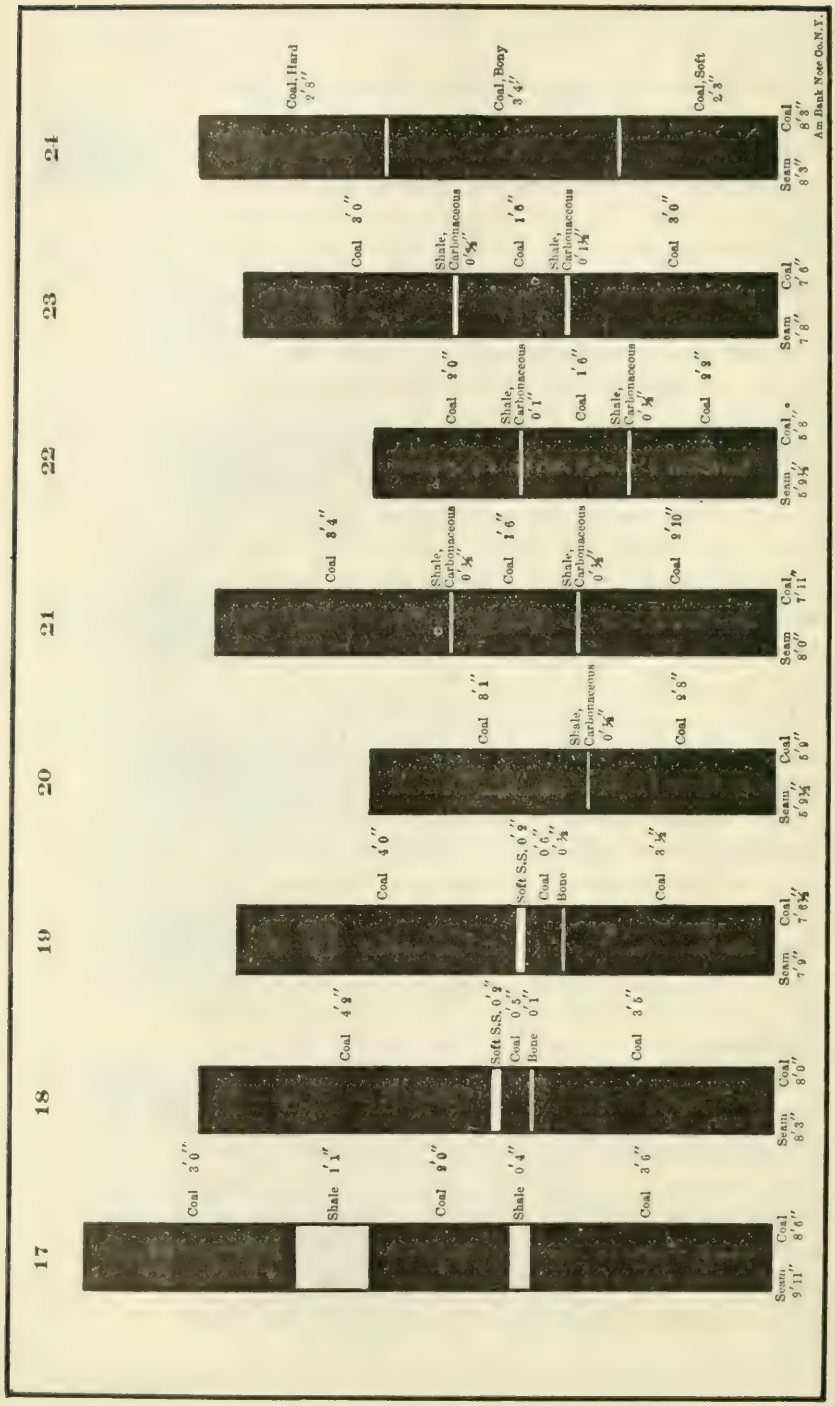
Of the five seams mentioned, the Upper and Lower Banner, Imboden, and Jawbone have shown the greatest reliability. The Kennedy, while in places showing large, clean bed-sections, has at other places entirely pinched out over considerable areas. Various published analyses show the coals to be of unusual purity as to ash and above the average of purity as to sulphur contents. A few of these analyses are appended, the authority for each being quoted.

The shipments have been too limited to determine definitely the uses for which each seam is best adapted. So far as the market has been tested, it seems that the Upper and Lower Banner and Kennedy seams will sell as gas and domestic coals, and as steam coal, when not in competition with the Pocahontas coal; the Imboden as a steam and domestic coal; and the Jawbone as a domestic coal, for which purpose it is eminently fitted.

One hundred and fifty bee-hive coke-ovens are in course of erection, intended to coke the slack made in mining the Upper Banner seam for gas and domestic purposes. This seam has been coked in quantity at other ovens, and has had a satisfactory furnace-test at Pulaski; and it will, no doubt, take its place among good furnace-cokes. The coal of the Imboden seam has also been coked in bee-hive ovens, and the result, both in appearance and analysis, is most satisfactory.

The accompanying bed-sections display a fair average of the developed seams. They have been taken at the heads of the longest entries, unless the coal at the heading showed a marked departure from the average of the mine. A few outcrop-sections are appended to indicate the condition of a seam at some point not developed by actual operations. The sections have all been taken from points on the southern edge of the field, that being the only portion developed in the absence of railroads in the remainder and larger portion of the county.

In different portions of the field, as one seam or another appears



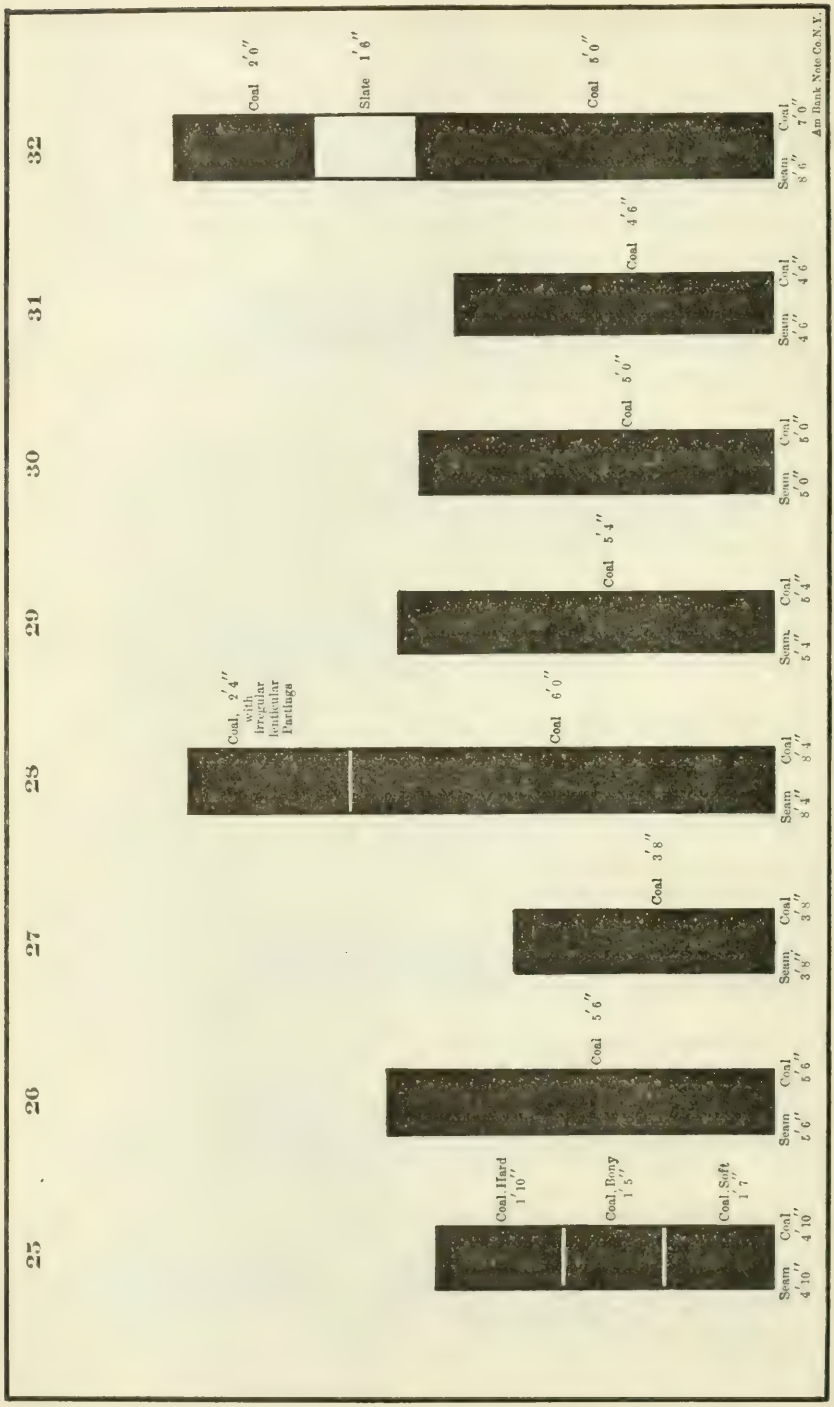
Coal-Sections in Wise County, Va.

to be the most available as to thickness and position, that seam will be mined to the temporary exclusion of the others. In some parts the most available seam has been determined. In other parts it is likely that the seam now worked will be abandoned for a more available one.

Referring the sections given to the nearest railroad station from which a branch is or could be built to tap the coal, and commencing on the east at St. Paul station, on the Norfolk and Western railroad, the following table indicates the extent and continuity of the coals:

TABLE I.—*Geographical Distribution of Coal-Exposures.*

At St. Paul Station, Norfolk and Western Railroad.	Section 1.—On Lick Creek, 6 miles northwest of station.
	Sec. 2.—On Open Fork of McClure's Creek, 6 miles northwest of station.
	Sec. 3.—On Big Laurel Branch of Lick Creek, 4 miles north of station.
At Virginia City Station, Norfolk and Western Railroad, 5 miles west of St. Paul.	Secs. 24 and 25.—On Russell Creek Branch of Norfolk and Western Railroad, ½ mile north of station.
	Sec. 28.—On Middle Fork of Russell Creek Branch of Norfolk and Western Railroad, 2 miles north of station.
	Sec. 26.—On Left Fork of Russell Creek, 3 miles northwest of station.
At Banner Vein Station, Norfolk and Western Railroad, 11 miles west of St. Paul.	Sec. 30.—On Little Tom's Creek, ½ mile east of station, on main line of Norfolk and Western Railroad.
	Sec. 31.—On Branch of Norfolk and Western Railroad, ¼ mile north of station.
	Sec. 27.—Half mile north of station.
At Coeburn Station, Norfolk and Western Railroad, 13 miles west of St. Paul.	Secs. 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, and 15.—On Big Tom's Creek Branch of Norfolk and Western Railroad, from 1 to 4 miles north and east of station.
At Tacoma Station, Norfolk and Western Railroad, 17 miles west of St. Paul.	Secs. 16 and 29.—On Guest River, main line of Norfolk and Western Railroad, ½ mile east of station.
At Norton Station, Norfolk and Western Railroad and Louisville and Nashville Railroad, 23 miles west of St. Paul.	Sec. 17.—On Guest River, ¼ mile northeast of station.
At West Norton, Louisville and Nashville Railroad, 24 miles west of St. Paul.	Secs. 18 and 19.—On Powell's River, Big Stone Gap Colliery Co., Branch of Louisville and Nashville Railroad, 2 miles north of railroad.
At Kelly View Station, Louisville and Nashville Railroad, 31 miles west of St. Paul.	Secs. 23 and 32.—On Right Fork of Mill Creek, ¾ mile north of station.
At Double Tunnel Station, Louisville and Nashville Railroad and South Atlantic and Ohio Railroad, 34 miles west of St. Paul.	Sec. 22.—On Callahan Creek, 5 miles north of station.
	Secs. 20 and 21.—On Looney Creek, South Atlantic and Ohio Railroad, 1½ miles northwest of station.



Coal-Sections in Wise County, Va.

TABLE II.—*Localities of Sections, Etc.*

No. of Sec.	Mine.	Seam.	Position of Section Measured.	Authority.	Date.
1	J. Salyer, no operation	Upper Banner	Outcrop opening	d'Invilliers	1892
2	Grissell, no operation	" "	Outcrop opening	"	"
3	Big Laurel Branch, no operation	" "	Outcrop opening	"	"
4	Speedwell Coal and Coke Company	" "	Head of Main entry, 1200 feet, under cover	Bache	1894
5	Kentucky Coal and Coke Company	" "	No. 2 Mine, head of Main entry, 300 feet, under cover	"	"
6	" " " "	" "	No. 1 Mine, head of Main air course	"	"
7	Jones Coal and Coke Company	" "	Head of Main entry, 1000 feet, under cover	"	"
8	Lee Coal and Coke Company	" "	Head of Main entry, 500 feet, under cover	"	"
9	Swansea Coal Company	" "	Head of Main air course, 800 feet, under cover	"	"
10	Pine Run Coal and Coke Company	" "	Head of Side entry, 500 feet, under cover	"	"
11	" " " "	" "	Head of Main entry, 500 feet, under cover	"	"
12	Coeburn Coal Company	" "	Head of Main entry, 600 feet, under cover	"	"
13	" "	" "	Head of Second Cross entry, 600 feet, under cover	"	"
14	Sexton Coal and Coke Company	" "	Head of Main entry, 600 feet, under cover	"	"
15	" " "	" "	Head of Fan entry, 500 feet, under cover	"	"
16	Greeno-Bodine Coal Company	" "	Head of Main entry, 300 feet, under cover	Fleming	1892
17	Norton Land Improvement Company, no operation	" " (?)	Head of Main entry, 600 feet, under cover	"	1893
18	Big Stone Gap Colliery Company	" " (?)	No. 2 Mine, Main entry, 200 feet, under cover	Bache	1894
19	" " "	" " (?)	No. 1 Mine, Main entry, 500 feet, under cover	"	"
20	Looney Creek Mine, South Atlantic and Ohio Railroad	Imboden	Head of Main entry, 700 feet, under cover	"	"
21	" " " " "	"	Head of third Right entry, 300 feet from Main entry	"	"
22	Pioneer Opening, Callahan Creek, no operation	"	Main entry, 50 feet, under cover	"	"
23	J. J. Kelly, no operation	"	Outcrop opening	"	"
24	Russell Creek Coal Company	Jawbone	First Right entry, 750 feet, under cover	"	"
25	" " " "	"	Head of Main entry, 1305 feet, under cover	"	"
26	J. W. Meade, no operation	Lower Banner	Outcrop opening	"	1893
27	Virginia Gas Coal Company	" "	Outcrop opening	d'Invilliers	1892
28	Clinch Valley Coal and Coke Company	Kennedy	Average of Mine	Bache	1894
29	Greeno-Bodine Coal Company	"	" "	d'Invilliers	1892
30	Virginia Gas Coal Company	"	East Mine (No. 2), average of Mine	Bache	1894
31	" " "	"	West Mine (No. 1), average of Mine	"	1892
32	J. J. Kelly, no operation	Unreferred	Outcrop opening	"	1894

TABLE III.—*Analyses.*

Name of Seam.	Fix. Carb.	Vol. Mat.	Water.	Ash.	Sulph.	Authority.
Upper Banner, Av. 3 analyses.....	59.394	34.028	.625	5.353	.600	McCreath
Lower Banner, Av. 3 analyses.....	60.081	34.195	.883	3.975	.866	"
Kennedy, Av. 3 analyses.....	59.888	34.331	.909	4.075	.797	"
Imboden (Coke), 1 analysis.....	94.040	.564	.068	4.740	.588	"
Jawbone, Av. 4 analyses.....	51.488	33.239	1.072	13.487	.714	"

SOLIDS FALLING IN A MEDIUM.—I.

THEIR INCIPIENT AND MAXIMAL VELOCITIES—A STUDY FOR ORE-CONCENTRATORS.

BY F. M. F. CAZIN, HOBOKEN, N. J.

(Virginia Beach Meeting, February, 1894.)

SOME twenty years ago the author of a book,* by which, for the first time, molecular action was made to account for nearly all phenomena in hydrodynamics, began his preface as follows:

"It is contrary to all experience, that any branch of science should suddenly cut short its own line of reasoning, and drop all connection with its past, and yet as quickly re-establish itself on an entirely new basis. It is, therefore, not without justification that this book should be met with caution and even distrust."

When the newly-discovered natural facts and laws presented in this paper were, in 1891, first laid before the officers of the American Institute of Mining Engineers, they might have been preceded by a preface to the same effect; and the expression would have found its justification by the actual experience these facts and laws met with. Since then, as the result of their publication in one of the most prominent

* Hugo Heinemann, Koenigl. Bau-Inspector in Hagen, *Die Rational-Theorie der Bewegung des Wassers, als Lehrbuch der Hydrodynamik und für den praktischen Gebrauch des Hydrotekten auf vollstaendig neuen Grundlagen bearbeitet.*

scientific and technical journals of North America,* they have found general concurrence on the part of scientists, and the time has come when mine-engineers should take cognizance of a progressive step in general science, which has an important bearing on one of the specific technical branches entrusted to their professional care.

These facts and laws, as first expressed† and discussed by the author of this paper, are as follows:

(1) A solid moving in submersion a distance (d) equal to its own length, when measured in the direction of motion, displaces of the medium in inverse direction and for the distance of its own length (of travel) a volume ($= B$) equal to its own immersed or submerged volume, and when so moving any other distance (of travel), the solid moves the volume B of the medium in inverse direction of its own motion for the total distance of such travel.

(2) A ship progressing at any stated velocity per second causes the movement in inverse direction within every such second of a quantity of water being equal to

$$D = \frac{B}{d} . v \text{ metric tons};$$

B signifying the ship's immersed volume or buoyancy in cubic meters;

d signifying the ship's length measured in the direction of motion on her water line in meters;

v signifying the distance of her travel per second in meters.

(3) A solid in moving in a medium displaces permanently for a distance ($= d$), in inverse direction to its own motion, a volume of the medium equal to the product of its transverse average section ($= B \div d$) and the distance of its travel, the transverse average section being as the solid's immersed or submerged volume, B, divided by its length, d, measured in the direction of its motion.

(4) A solid of non-uniform section transverse to the direction of its motion, when moving in a medium, displaces of the medium in inverse direction to its own motion permanently a quantity measured by its own transverse average section ($= B \div d$) multiplied by the distance of its travel, and it also dislocates passingly and in a lateral direction another quantity of the medium, that is measured by the difference between the transverse major and average sections of the solid as one factor and the distance of its travel as the other factor, this additional quantity of the medium being caused to passingly move or dislocate, but occupying the same position after the solid has passed, as it did previously thereto, and the quantity of the medium = distance of travel $\times B/d$ is inversely moved permanently for the distance $= d$ (from fore to aft).

(5) The resistance to the movement of all solids in a medium is measured by the permanent displacement in inverse direction to their own movement of a quanti of the medium, which is the product of the solid's own transverse average tion ($= B/d$) in the length of the solid's travel.

n time,

$$D = \frac{B}{d} . v \text{ per second.}$$

ing t, re-

* Compare "Resistance to Ship's Motion," by F. M. F. Cazin, in *of the Franklin Institute*, March, April, and May, 1893.

† *Journal of the Franklin Institute*, March, 1893.

And the conclusions for the measurement of power required to overcome the resistance thus defined, as drawn from these five theoretical assertions, are as follows:

It is the *quantity*,

$$f v = \frac{B}{d} \cdot v,$$

of the water for every second, which must *receive an impulse* to flow in a direction inverse to the ship's motion, and the impulse given to the water must be sufficient to cause its flow with the stated *velocity*. And this impulse is equivalent with the raising of the stated quantity,

$$f \cdot v = \left(\frac{B}{d} \times v \right),$$

of water per second to an elevation

$$S = \frac{v^2}{2\,g} \left(\text{or } S\frac{g}{2} = \left(\frac{v}{2}\right)^2 \text{ when } \frac{g}{2} \text{ is the unit for measuring distance} \right).$$

It is, therefore, also true, that

(6) The power required for propelling a ship at a stated velocity per second ($= v$) is as

$$P = f\,v\,S = \frac{B}{d} \cdot v \cdot \frac{v^2}{2\,g} = \frac{B}{d}\frac{v^3}{2\,g} \text{ kilogramme meters;}^*$$

f signifying transverse average section $= \frac{B}{d}$;

S signifying total distance of fall by acceleration of gravity;

B signifying the ship's immersed volume or buoyancy. When the total value is expressed in ton-meters, then B is expressed in cubic meters, but when the total value is expressed as kilogramme-meters, then B is expressed in the corresponding value of (0.1 meters)^3 = litres = kilogrammes;

d signifying the ship's length measured in the direction of her motion and on her water line;

v signifying the ship's velocity in meters per second;

g signifying twice the distance, which matter, when free to fall will pass in the direction towards the earth's center within the first second of such free fall. This distance, g, being as $g = 9.78009 - 0.05080 \sin^2 \varphi$ meters (φ = geographical latitude), travel of ships for equal speed and distance requires less power on northern than on southern geographical lateral travel, north of the equator.

When movement in another medium than water is under consideration, the factor for density of medium must become part of the equation, which then will read

---

* A railroad train, say of sixty cars of equal weight, each twenty feet long, moves with in one second a distance of 400 feet. When stopping again forty cars will be where forty cars were before, virtually not moved at all, and twenty cars, that were at the rear end, are there no longer, but appear at the front end, though they be not the same cars.

With B as the total weight (in vacuo) of the train in metric tons, with d as the length of the train in meters, and with v as the distance of travel per second $\frac{B \cdot v}{d}$ represents the mass moved per second, and $\frac{B \cdot v^3}{d \cdot 2 \cdot g}$ represents the power required to move the train horizontally, when running with lubricated cool axles.

$$P = \delta \cdot \frac{B}{d} \cdot v \times \frac{v^2}{2g},$$

δ being the density of the medium, with that of water as the unit.

(7) By obliquely advancing the ship increases the resistance to her progress at the precise inverse rate, as she shortens the distance between two perpendiculars on the direction of her motion, leaving the ship between them on the plain of the water's surface.

(8) For the same volume (B) of immersed or submerged solid the resistance to motion by the medium increases and decreases at the precise but inverse rate of the solid's length (d) measured (for ships on the water line) in the direction of motion.

(9) By increasing the length of a solid intended to move in submersion (ship-bottom) for a stipulated volume (B), the resistance by the medium to the motion of such solid is decreased at the precise same rate, viz.: as the value d increases.

When proof had been presented of the facts and laws thus stated, the author evolved one more conclusion therefrom, which it is the main purpose of the present paper to discuss, namely:

(10) The maximal velocity of fall of any solid within a medium is

$$C = \sqrt{\delta . d . 2g . \frac{\delta - 1}{\delta + 1}};$$

δ signifying the density of the moving body when compared with the same volume of the medium taken as unity; and d signifying the length of the solid, measured in the direction of motion.

To render proof of this 10th theorem is mainly the object of this paper, some practical consequences being also drawn therefrom.

If the horizontal movement of a solid in a medium moves the measured quantity of medium in inverse horizontal direction, then the falling solid raises the measured quantity of medium. In this light, the action is analogous to what occurs in Attwood's apparatus designed to demonstrate the modified velocities of impeded fall, or, of a heavier weight, P, when in its fall it raises a lighter counter-weight, Q.

It was found by Attwood, and is now a matter of demonstration in physical laboratories, that such impeded fall takes place with a modification of the value of g by a co-efficient,

$$p = \frac{P - Q}{P + Q},$$

or, in other words, that S, the total distance of fall for a given time, t, in vacuo, with acceleration of gravity, is to S_1, the fall during t, retarded by a counter-weight, as follows:

$$S : S_1 :: \frac{v^2}{2g} : \frac{v^2}{2g\left(\frac{P - Q}{P + Q}\right)}.$$

When a solid falls in a medium, the two weights, $P > Q$, are represented by δ and unity, and the co-efficient

$$\frac{P-Q}{P+Q} \text{ becomes } \frac{\delta-1}{\delta+1}.$$

Under the fact as expressed under theorems (1) to (9) above quoted, $B\delta v$ truly represents the force of the solid's fall per second, and

$$P = \frac{B}{d}\, v \times \frac{v^2}{2g\left(\frac{\delta-1}{\delta+1}\right)}$$

represents truly the resistance, which the medium offers to such fall. When these two values become equal, then the velocity forming a factor in these values has become *uniform* velocity per second, and is no longer variable under the law of acceleration by gravity.

What this velocity, to which there is no increase or acceleration, because the cause of acceleration—(force of fall, or gravity)—is neutralized by resistance to fall by the medium, is, can be learned from the equation:

$$B\delta v = \frac{B}{d}\, v \times \frac{v^2}{2g\left(\frac{\delta-1}{\delta+1}\right)} \text{ and } v = \sqrt{\delta d\, 2g\left(\frac{\delta-1}{\delta+1}\right)} = \text{C.}^*$$

In this equation, the value for resistance by the medium to the falling movement of the solid is set against the potential energy for falling in the solid, in order to find the velocity of fall which the falling solid will have attained at the instant, when its faculty of accelerating its fall is, under the velocity thus acquired, neutralized by the resistance of the medium, which resistance has become effective at the said acquired velocity.

This method, employed for ascertaining uniform maximal velocity of fall in submersion is merely analogous to that employed, though on quite different assumptions, by P. von Rittinger, namely, of ascertaining the velocity of a vertical stream of water, which will keep the solid in the same position relative to the earth, while the water is ascending.

To compare the two methods, and the results attained by them, to throw light on causes of error, and to reconcile conflicting conclusions, is not the purpose of this paper, as it requires a mathematical argument, which will more suitably occupy a separate paper.

* In order to distinguish this value for maximal uniform velocity from velocity under acceleration, it will be further expressed by C as analogous to Rittinger's value *c*.

The value C represents properly the maximal and uniform velocity, with which *any solid of higher density* than the medium will fall within such medium. And it also represents the result of a preceding fall with a modified acceleration $= g\left(\frac{\delta-1}{\delta+1}\right)$, which is designated as "incipient fall."

Here the further question arises as to the duration and total distance of such incipient fall, which precedes the fall with uniform maximal velocity of all solids falling in a medium.

The total distance of incipient fall, that is, the distance that the solid will fall in the medium before it acquires its possible maximal velocity, with which it proceeds to fall uniformly, until it reaches the bottom, is given by the following equation, in which S is the total distance of accelerated fall:

$$S = \frac{C^2}{2g\left(\frac{\delta-1}{\delta+1}\right)} = \frac{\delta\, d\, 2g\left(\frac{\delta-1}{\delta+1}\right)}{2g\left(\frac{\delta-1}{\delta+1}\right)} = \delta\, d.$$

The significance of this result is, that when a solid has fallen in a medium a distance of its length (measured in the direction of its movement) multiplied by its density—that is, when it has displaced in falling a weight of the medium precisely equal to its own weight, then it ceases to be further accelerated in its fall, but continues to fall with the uniform velocity $C = \sqrt{\delta \,.\, d \,.\, 2g\left(\frac{\delta-1}{\delta+1}\right)}$ per second.

The duration of incipient fall with the modified acceleration $g\left(\frac{\delta-1}{\delta+1}\right)$ for a distance $\delta\, d$, is given by the following equations, according to well-established natural laws:

$$S = \delta \,.\, d = t^2 \times \frac{g\left(\frac{\delta-1}{\delta+1}\right)}{2}, \text{ whence}$$

$$t = \sqrt{\frac{2\,\delta\, d}{g\left(\frac{\delta-1}{\delta+1}\right)}}; \text{ whence } \frac{C}{t} = \frac{\sqrt{\delta\, d.\, 2g\left(\frac{\delta-1}{\delta+1}\right)}}{\sqrt{\frac{2\,\delta\, d}{g\left(\frac{\delta-1}{\delta+1}\right)}}} \text{ and}$$

$$t = \frac{C}{g\left(\frac{\delta-1}{\delta+1}\right)}$$

Determining the velocity per second of such incipient fall (though in ore-dressing practice such fall does not continue in any case for a full second) we find the solid falling the distance $\delta . d$ within the time

$$t = \sqrt{\frac{2\,\delta\,d}{g\left(\frac{\delta - 1}{\delta + 1}\right)}} \text{ sec.}$$

According to the proportion $\sqrt{\frac{2\,\delta\,d}{g\left(\frac{\delta - 1}{\delta + 1}\right)}} : 1 :: d\,\delta : r_1$

the incipient velocity, v_1, is:

$$v_1 = \frac{\delta\,d\sqrt{g\left(\frac{\delta - 1}{\delta + 1}\right)}}{\sqrt{2\,\delta\,d}} = \sqrt{\delta\,d\,g\left(\frac{\delta - 1}{\delta + 1}\right)} = \frac{C}{2} = \frac{C}{2}$$

We may then correctly state these relations as follows:

(11.) A solid falls in submersion first a distance, $\delta . d$, with a modified acceleration, $g\frac{\delta - 1}{\delta + 1}$, and with a velocity per second $\frac{C}{2}$: but does so for a time only, being

$$t = \frac{C}{g\left(\frac{\delta - 1}{\delta + 1}\right)} = \sqrt{\frac{2\,\delta\,d}{g\left(\frac{\delta - 1}{\delta + 1}\right)}} \text{ second,}$$

until it attains, after falling such distance, $\delta . d$, that is, after displacing in its fall of the medium a weight equal to its own, a velocity,

$$C = \sqrt{\delta\,d\,2g\,\frac{\delta - 1}{\delta + 1}},$$

with which maximal and uniform velocity it then continues its fall until it touches bottom.

When comparison is made between different solids in relation to the different ultimate velocities of their fall in water, as conditioned by their different qualities, we may compare with one another solids (and their falling velocities in submersion):

(*a*) Of different densities and of different lengths (measured in the direction of motion);

(*b*) Of different densities only, but of equal lengths (measured in the direction of motion or fall);

(*c*) Of different lengths (measured as specified) but of equal densities.

For comparing ultimate velocities of fall in submersion of solids of the class (a), we have to make use of the following proportions:

$$C_1 : C :: \sqrt{2g} \times \sqrt{d_1\delta_1\left(\frac{\delta_1 - 1}{\delta_1 + 1}\right)} : \sqrt{2g} \times \sqrt{d\delta\left(\frac{\delta - 1}{\delta + 1}\right)};$$

that is,

$$\text{as } \sqrt{d_1\delta_1\left(\frac{\delta_1 - 1}{\delta_1 + 1}\right)} : \sqrt{d\,\delta\left(\frac{\delta - 1}{\delta + 1}\right)}.$$

But while these solids may differ from one another in either of their essential qualities of density and length (properly measured), it may be easily shown that, nevertheless, their ultimate uniform and maximal velocity of fall may be the same. To demonstrate this, we suppose $C_1 = C$; that is,

$$\sqrt{d_1\delta_1\left(\frac{\delta_1 - 1}{\delta_1 + 1}\right)} = \sqrt{d\delta\left(\frac{\delta - 1}{\delta + 1}\right)}, \text{ or}$$

$$d_1\delta_1\left(\frac{\delta_1 - 1}{\delta_1 + 1}\right) = d\delta\left(\frac{\delta - 1}{\delta + 1}\right).$$

The condition for equal final velocities of fall in a medium thus appears to be the equality of the product

$$d\delta\left(\frac{\delta - 1}{\delta + 1}\right)$$

for the two solids that are to have equal-falling velocities in the same medium. This involves the necessity, that in the same proportion as the values $\delta\left(\frac{\delta - 1}{\delta + 1}\right)$ relating to densities of equal-falling solids vary, the lengths (properly measured) of these solids must inversely vary.

For comparing ultimate, uniform velocities of fall in submersion of solids of the class (b), we have to make use of the following proportions, assuming equal lengths (properly measured) but different densities:

$$C_1 : C :: \sqrt{d\,\delta_1\left(\frac{\delta_1 - 1}{\delta_1 + 1}\right)} : \sqrt{d\delta\left(\frac{\delta - 1}{\delta + 1}\right)};$$

that is,

$$\text{as } \sqrt{\delta_1\left(\frac{\delta_1 - 1}{\delta_1 + 1}\right)} : \sqrt{\delta\left(\frac{\delta - 1}{\delta + 1}\right)}.$$

For comparing these velocities as between solids of class (c), having equal densities, but of different lengths (properly measured), the following proportions must be employed:

$$C_1 : C :: \sqrt{d_1 \delta \left(\frac{\delta - 1}{\delta + 1}\right)} : \sqrt{d \delta \left(\frac{\delta - 1}{\delta + 1}\right)};$$

that is,

$$\text{as } 1 \sqrt{d_1} : 1 \sqrt{d}.$$

And all these results may again be expressed in words as follows:

(12) The maximal and ultimate uniform velocity of fall in submersion of all solids of any form or substance being in each case, on the sole condition of $\delta > 1$,

$$C = 1\sqrt{2g} \times \sqrt{d \left(\frac{\delta - 1}{\delta + 1}\right)},$$

the ratio of velocity between solids of different lengths (properly measured in the direction of fall) and of different densities, is as the values of $\sqrt{d\delta \left(\frac{\delta - 1}{\delta + 1}\right)}$ for each, and the velocities are equal when the products,

$$d\delta \frac{\delta - 1}{\delta + 1},$$

relating to either are equal.

(13) The maximal, ultimate and uniform velocities of the fall of solids in a medium, when these solids are of equal lengths (measured in the direction of their movement) rate as the values of

$$\sqrt{\delta \left(\frac{\delta - 1}{\delta + 1}\right)}$$

for the several solids.

(14) The maximal, ultimate, uniform velocities of the fall of solids in a medium when these solids are of equal densities, are as the values for $1\sqrt{d}$ relating thereto, that is, as the square root of their lengths, measured in the direction of their fall.

As an illustration of all these results, which are so materially different from what, on the authority of Rittinger and others, has been assumed to be true, a case of common practice may be discussed. Selecting a case where particles of ore, graded so as to represent approximately spheres of 0.001 m. diameter (d measured as length in the direction of motion), the particles consisting in part of galena, with a density of 7.4, and in part of quartz of a density of 2.6, the former particle will first fall a distance of δd, or of 0.0074 m. with a modified acceleration, but with a velocity per second of

$C \div 2$. And after such incipient fall the particle of galena will continue to fall, until it reaches bottom, with a uniform velocity:

$$C = \sqrt{2g} \times \sqrt{d\,\delta\left(\frac{\delta - 1}{\delta + 1}\right)} = 4.43\sqrt{0.001 \times 7.4 \times \frac{6.4}{8.4}} = 0.3324$$

m. per second.

But the particle of quartz falls with modified acceleration a distance only of $0.001 \times 2.6 = 0.0026$ m., and will then continue to fall with the uniform velocity of:

$$C_1 = 4.43\sqrt{0.001 \times 2.6 \times \frac{1.6}{3.6}} = 0.15062 \text{ m. per second.}$$

From these results the conclusion must be drawn, that, unless modified by such influence as incipient fall may exercise, the particle of galena will reach bottom in less than half the time required by the particle of quartz for doing the same, in other words, that in the same grade of particles falling in water, galena will form the lower and quartz the upper stratum of deposit.

It should therefore be shown, whether this result is prevented by the effect of incipient fall, and if not prevented, to what extent the general result be thereby modified.

The incipient velocities being for galena as $0.3324 \div 2 = 0.1662$, and for quartz $0.15062 \div 2 = 0.07531$, the general result would be precisely maintained, if the time for incipient fall for both particles were the same.

The incipient time-fraction is in either case determined by

$$t = \frac{C}{g\left(\frac{\delta - 1}{\delta + 1}\right)}$$; and g being assumed as 9.8 m, we have

for galena:

$$t = \frac{0.3324}{9.8 \times \frac{6.4}{8.4}} = 0.044,$$

and for quartz:

$$t = \frac{0.1506}{9.8 \times \frac{1.6}{3.6}} = 0.034.$$

During these incipient time-fractions both particles fall, as already remarked, with half their general and proportionate ultimate velocities.

Therefore with equal incipient time of fall neither alteration nor modification of the general result of fall with uniform ultimate velocities, such as have been shown, would take place as a result of incipient fall with modified acceleration.

But as the time-fractions for incipient fall are not equal, a certain modification of the general effect of ultimate uniform velocities must result. And it is of practical interest to show the true nature of this modification.

It appears more to the purpose to illustrate the effect by the discussion of a practical instance of known application, than to further discuss the underlying theorems; therefore I shall continue to consider the case already selected as an illustrative one. Assuming the 0.001-m. galena and quartz particles subjected to the action of a jig working with 200 full strokes or reciprocations per minute, the plunger-down-stroke or the rising of bed-water will take $\frac{60}{2 \times 200}$ or 0.15 sec.

The behavior of either particle during this 0.15 sec. will be as follows:

	Sec.	Meters.
Of the	0.150,	
the 0.001-galena-particle will pass in incipient fall	0.044, falling	0.0074
leaving for fall with uniform velocity,	0.106, falling 0.106 × 0.3324, or,	0.0352
and hence will fall in 0.15 sec.,		0.0426
The 0.001-quartz-particle, will of the	0.150,	
pass in incipient fall	0.034, falling	0.0026
leaving for fall with incipient velocity,	0.116, falling 0.116 × 0.1506, or,	0.0175
and hence will fall, in total, in quiet water in 0.15 sec. the distance of,		0.0201

per stroke, which also means, that with a plunger-lift of 0.02 m., and with 200 revolutions per minute, the 0.001-m. quartz-particle will neither rise nor fall, but will be kept suspended in the rising stream. And if the velocity of the rising stream is increased as a consequence of impeded space of current, the 0.001-m. quartz-particle will, with a 0.02-m. plunger-stroke and 200 revolutions, be carried over by the stream.

The proportionate distances of fall per sec. with ultimate, uniform velocity exclusively, would be for the two 0.001-m. particles as:

	M. per Sec.
0.1506 : 0.3324 or, ...	0.453
while their real or total proportionate fall is as 0.0201 : 0.0426, or, . .	0.472
leaving a difference of proportionate fall =, ...	0.019

m. per sec. between the assumed and the real case.

It is thus shown, that, although the general effect is indeed modified nearly 2 per cent. by incipient fall, this modification is both favorable and (within the limits occurring in practical ore-dressing) not sufficiently important to deserve special consideration in separating ore-particles by their velocities of fall in water.

Intelligent mineral-separating practice in subjecting particles of as nearly equal lengths, d, as may be assorted or graded by sifting, to the action of an alternately ascending and receding or resting stream of water, selects as the adequate velocity of the ascending stream one, which is in excess of the uniform falling-velocity of the lighter of the particles, which are to be separated by density-classes, and below the uniform falling-velocity in water of the heavier particle; the stream-velocity selected being nearer to the former than to the latter. With plunger-velocities thus regulated, the actual stream-velocity, as a consequence of impeded space for flow, will be moderately increased, establishing in practice a true intermediate velocity between the two extremes. Thereby the lighter particles are found to follow the movement of the stream, while the heavier particles remain unmoved or fall back to the sieve-bed, on which they are spread.

A reciprocating movement of the water is evidently not required, as the ascending stream effects the separation; but the stratification of particles resulting as stated is not materially disturbed by the receding of the stream. The receding movement has been retarded by complicated, and mostly objectionable, construction of mechanical parts intended to produce plunger-movement, but the simpler method for such retardation of the receding stream, or even for its total avoidance, is found in supplying feed-water under the plunger* and sieve at the intervals in which the plunger makes its receding movement.

* Jigs equipped as here indicated were unsuccessfully tried early in the present century on German mines and were called *Setzpumpen*. The cause of failure consisted in the frequent giving-out of the necessary plunger-valves. This cause of failure is entirely avoided by a valve which can be interchanged from the exterior and even while the jig is in operation, or, as a further precaution, by the use of a simple, durable flap-valve, such as is shown in the accompanying figures.

In any given case, the conditions for adapting the plunger-movement to the rational requirements may be ascertained in a manner, for which the following may serve as a practical illustration:

With a 0.001-m. grade of quartz and galena, the ultimate velocities for which are in water as 0.15 m. and 0.33 m. per second, the ascending stream, to which they are exposed for the purpose of separating the single particles according to their different densities, should have a velocity of more than 0.15 m. and of less than 0.33 m. per second. The intermediate velocity is

$$\frac{0.15 + 0.33}{2} = 0.24 \text{ m.; and } \frac{0.15 + 0.24}{2} = 0.195 \text{ m.,}$$

per second complies closely with the conditions for separation, as above specified.

Assuming 0.2 m. per second as the desirable ascending stream-velocity, the jig-plunger, unless corresponding to unequal sieve-area, will have to pass a distance of $0.2 \times 60 = 12$ m. per minute.

For reasons previously discussed, the time for each exposure of particles to the ascending stream should be in excess of the time of incipient fall for both classes of particles, and should be sufficient for full development of ultimate uniform velocity of fall for either particle. The time per stroke, therefore, should be 0.0722 seconds or more for the single plunger-stroke and 0.1444 seconds for the full stroke. Of such strokes there are possible in a minute $60 \div 0.15 = 400$. The total distance of 12 m. to be passed per minute, divided into 400 strokes, leaves for each full stroke a distance of 0.03 m., and for the single stroke or plunger-lift 0.015 m. for 0.001-m. material or grade of ore.

The possibility of complying with all known conditions for successful mineral-separation by means of a rationally-constructed jig induced the writer at different times to turn his attention to such a construction. The first result some twenty years ago was the one-plunger and multiple-sieve jig, producing with a very simple mechanism a decreasing velocity of up-stream on successive sieves.* A one-lever jig, with a plurality of suspended plungers, producing the same effect by different mechanical means, replaced this first attempt, both doing good service in the treatment of ore containing more than one valuable mineral. The accompanying figures represent a jig of

* Compare: *Berg und Huettenmaennische Zeitung*, 1872, No. 22, and P. Ritter von Rittinger, *Zweiter Nachtrag zum Lehrbuch der Aufbereitungskunde*, p. 38.

Fig. 1

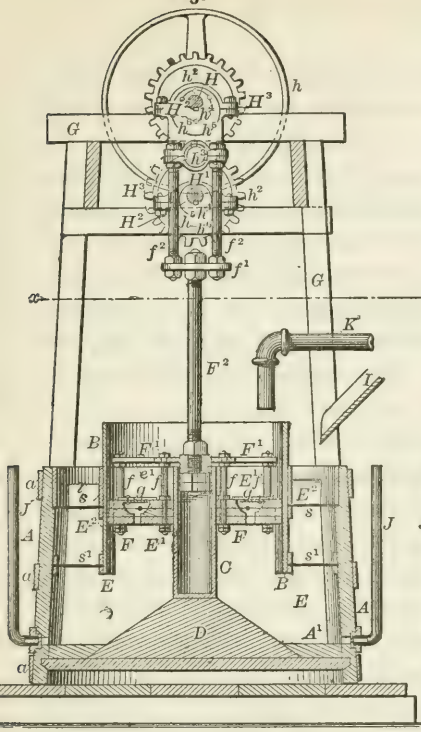


Fig. 2

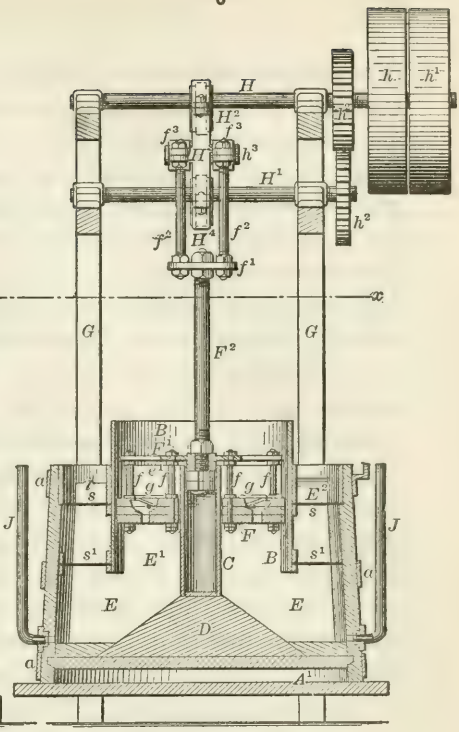


Fig. 3

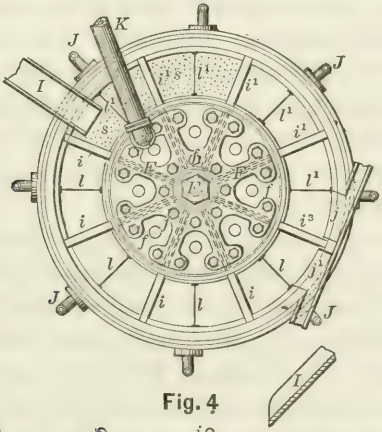


Fig. 4

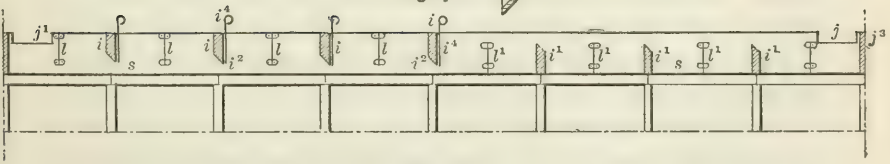
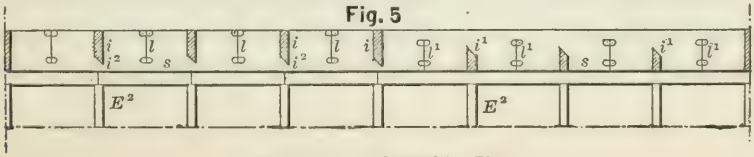


Fig. 5



Continuous Adjustable Jig.

more recent construction, in which an entirely new mechanical movement for evolving reciprocating straight-line motion from revolving motion is applied, for which movement patent has been allowed to the author, but as yet has not been issued. The distinctive character of the machine, as shown, lies in leading the different minerals contained in the ore above the sieve in two opposite directions, the lighter material passing to adjoining sieves on the surface of the bed, and the heavier passing in the opposite direction in close proximity to the sieves themselves, there being two outlets above the sieve, one for headings and one for tailings. It is evident that intermediate products (as to density) will collect on intermediate sieves.

The letters in the drawings mark the different parts as follows :

A is a circular wooden vessel, the inner space of which is divided into eight circumferential and one central (C) compartments, the former communicating with one another but only above the sieve-level, g, in the manner shown in Figs. 4 and 5. Each compartment has a hand-hole (not shown), a movable syphon-pipe and a discharge-valve (not shown) with a lever attached and a spring holding the latter in position. The lever serves for manipulating the valve, the attendant using his foot for such manipulation.

The vessel, A, is shown as a wooden cistern, but in improved form, is made of as many cast-iron sections as there are compartments, these sections having flanges on three sides, by means of which they are bolted together to form with the bottom-plate the vat, A′, of which the outer upper edge is represented in Fig. 4, with its two overflows, j and j', separated from one another by the wall, j^3. One outflow is for headings and the other for tailings, middlings being gathered from the sieves with scrapers.

B is an inner, circular, concentric tube-shaped partition, open above and below, dividing the upper part of the 8 compartments into two spaces each, thus forming an inner circle of 8 independent plunger-spaces and an outer circle of 8 independent sieve-spaces, all division walls, ii', ll', ee', except E^2, which supports the sieve-frames, being made of sheet-metal, and no faces being left on which the moving water would strike in a perpendicular direction.

Fig. 5 shows the face of the tube, B, in part, that is turned towards the sieves, which are placed outside thereof.

C is a central tube with 8 vertical incisions, intended to receive and hold on its outside the inner edges of the compartment-division-plates, and to give space inside to the hub of the spider, F′, from which the 8 plungers, F, are rigidly suspended. The tube, C, does

not serve as guide to the rod, F^2; the mechanical movement applied not leaving any want thereof, but causing in itself a true straight-line-reciprocating motion.

D is a hollow cone, which may be cast as a part of the bottom-plate. The tube, C, rests on this cone, which has on its slanting face grooves corresponding to those on the tube and intended to receive the edges of the partition-plates, E, E′, E^2; such grooves or incisions being continued also on the inner face of the bottom and vat-wall by means of strips, in case the vat is of wood, or by means of space left between the flanges of sections, if the vat is made of cast-iron.

E, E′, etc., are 8 radial division-walls, separating 8 compartment-sets, the outer edges of which are inserted in grooves, as above described; the upper part of each partition-plate having a slit, into which the tube-wall, B, is partly inserted; the inner upper edge reaching very nearly to the level of the outer wall of the vessel, A, and the outer edge reaching to the under-face of the ring-shaped sieves, *s*; the lower sieves being inserted between these partition-plates. By the level of the upper edges of these plungers the flow of feed-water is regulated, which in operating the jig must overflow all of these upper edges. Hence slides are used for regulating their level.

F designates 8 plungers, each closely filling one of the 8 plunger-spaces, which surround the tube, C. Each plunger has a central valve, opening when the plunger is raised, and closing when the plunger moves downward. These plungers are shown as made of wood, but are preferably made of a cast-iron ($\frac{1}{2}$ inch) plate with the valve sunk into a central aperture and flanged thereto in such manner, that the valves may be interchanged, while the jig is in operation. The valves, *g*, being submerged during operation, the water-pressure from above opens the flaps when the plunger ascends, a cross-bar limiting the movement of the flap. The valve opens when the plunger ascends, thus avoiding in part the receding of the water from the sieves. All 8 plungers are suspended rigidly from the spider, F′, each by 3 bolt-bars, *f*.

When the motive power is provided by an electric motor set on the shaft, H, the down-stroke of the plunger is accelerated by the use of a metal spring, working on the rod, F^2. The water, passing through the plunger-valves, ascends through the sieves and finds an outlet only through the two cuts, *j* and *j*′. It is on this account, that the water-feed-pipe, K, discharges at the most distant point from these outlets, *j* and *j*′, namely over the plunger corresponding to the

sieve-space, i^4–l'. The ore is fed on the sieves with or without water, according to its specific qualities.

G designates the supports for the bearings of the movement of the jig, shown as independent wooden posts, but made in improved form of cast-iron arches resting on the top of the vat, A.

H, H′, are two parallel shafts, geared for an equal number of revolutions by the gear-wheels, h^2, the one vertically above the other, and the one running inversely of the other, each having keyed to it one of two equal inversely-running eccentrics. H^2 and H^4 are both inclosed and run in the same frame, H^3, of which a point, h^3, moves in a straight reciprocating line. The rod, F, is connected with said point, h^3, by two straps, f^3, four bars, f^2, to straddle the lower shaft, H′, and a plate, f'. The stroke is made variable by an inner eccentric provided within those (H^2 and H^4) running in the frame, H^3, and set in different positions by the bolts, h^4, h^5, h^6. The cap enclosing the lower and outer eccentric is packed so as to make allowance for a slight longitudinal play in the frame of this lower eccentric; the upper one serving to impart power, and the lower to guide the point, h^3, in a straight-line reciprocating movement.

I is the discharge-end of the ore-feeder, feeding, as shown in Fig. 4, to the sieve by which a division into headings and tailings is first produced (i^4–l'). The percentage of headings being smaller than that of tailings, there is provided a slide, i (Fig. 4), for regulating the vertical dimension of the channel, i^2, similar slides being provided between each two sieves on the headings-side. The ore-outlets, j and j', have similar regulating slides (not shown); but there the upper edge regulates the level of outflow.

A certain regularity as to ore-quantity fed to this jig is as indispensable to its perfect work, as it is in the case of any other well appointed jig.

J represents 8 siphon-pipes, one for each sieve-compartment, serving two distinct purposes. A continuous discharge may be effected from any or all of the hutch-boxes, A′, by the use of these siphon-pipes. Where the action on any one or all of the sieves, even under adequate movement of plunger, is not sufficiently lively, these pipes may be used by means of hose-attachments for supplying water to the hutch-boxes. When detached, the apertures therefore permit the use of a water-jet for slushing or cleaning purposes.

K is the water-supply-pipe, discharging over the plunger-space that corresponds to the sieve, to which the ore is fed. The upper edges of the partition-plates, separating the different plunger com-

partments, may properly be reduced in level towards the opposite side of the vat, so as to cause regular flow and decrease of pressure on the tail-segment. (Compare remarks on E above.)

s, *s'*, are upper and lower sieves, made of coarse supporting wire-cloth, covered by and attached to wire-cloth of such fineness, as to retain the ore-size treated above sieves in the main, and provided at the start with appropriate ore-beds, the lower with a heavier bed than the upper one. To each sieve-pair there is, between the sieves, a hand-hole in the vat-wall (not shown in the drawing), with water-tight cover. The lower sieve and ore-bed cause an equalization of the ascending stream of water over the sieve-area, which contributes much to perfect separation being effected on the upper sieves.

I abstain from further remarks about the jig, the construction of which has thus been shown, but desire to say a few words more in regard to jigs in general.

With a construction permitting adaptation of number and size of strokes to any given requirement within the limits of common practice, with proper provision for above- and below-sieve water-supply, with an ore-supply harmonized with the water-movement and with a rational use made of the facilities thus offered by the machine, successful working can be accomplished in all cases, where the fundamental condition of difference in density between particles of similar dimensions is fulfilled. It remains to consider what limiting conditions there are to such similarity of dimensions.

If in practice jigs are not by far the highly appreciated machines, that they properly should be, the cause of this is not in the lack of practical virtue in the machine but mainly in their unintelligent application in general practice at even this late date. My individual experience during the last quarter of a century has convinced me, that the planning, constructing, and operating of establishments for ore-concentration is shunned by professional mine-engineers, who willingly have left and do leave the care of this special field to the convenience of machine-shops, and to skilled labor and its many blunders.

From the preceding discussion it has already become evident, that equality of particles in volume and length, with the greatest difference in density, would be the most favorable condition. The latter being the work done by nature and the former to be brought about by man, no absolute equality of volume and length can be attained for the mineral particles graded for simultaneous treatment. Similarity of dimensions alone is practically obtainable, and it remains to

determine to what extent similarity may vary from equality without refusing to do service in its stead.

The 12th theorem shows, that the condition to equal final velocity of fall of solids in a medium consists in this, that the product,

$$d\,\delta\left(\frac{\delta-1}{\delta+1}\right),$$

relating to both the solids, shall represent an equal total value.

Investigating the true nature of this condition, I once more select the specific case used heretofore as an illustration for applied theory, namely, the case of galena and quartz mixed.

To find the dimension d for a particle of quartz, the final falling velocity of which would be equal to the final falling velocity in water of a 0.001-m. particle of galena, we have by the 11th theorem the following equation:

$$0.001 \times 7.4 \times \frac{6.4}{8.4} = d \times 2.6 \times \frac{1.6}{3.6},$$

and in consequence the dimension d for the particle of quartz, which falls with equal final and uniform velocity with the 0.001-m. particle of galena, is

$$d = \frac{0.001 \times 7.4 \times \frac{6.4}{8.4}}{2.6 \times \frac{1.6}{3.6}} = 0.0049\ m.$$

And the time for incipient fall with modified acceleration relating to this 0.0049-m. quartz-particle appears as

$$t_1 = \frac{C_1}{g\left(\frac{\delta-1}{\delta+1}\right)} = \frac{0.1566 \times \sqrt{4.9}}{9.8\left(\frac{1.6}{3.6}\right)} = 0.07657$$

against $t = 0.044$ for the 0.001-m. galena particle.

It is thus shown that of the two equal-falling particles, the one of lower density passes the longer time in incipient fall, falling for $0.076 - 0.044 = 0.032$ sec. longer with only half the final velocity common to both, and that it will arrive at a given distant point later than the otherwise equal-falling particle of higher density. To take

the benefit of this difference in falling distance for the time of incipient fall would require

$$\frac{60 \times 2}{0.076 - 0.044} = 3750$$

strokes of the plunger per minute.

Such a number of strokes per minute is manifestly impracticable, and there is no good reason for seeking even to approach it. The utilization of the final velocities of fall is conditioned, not on the use of such a great number of revolutions per minute, but on the avoidance of a difference in the dimension d as large as that of 1 : 4.9 in the case under consideration. The general practical conclusion arrived at thus appears to be, that separation by density through utilization of difference in falling velocities in submersion is practicable for grades of ore that are so similar in their dimension d as not to differ to the extent of

$$1 \text{ for the heavier mineral} : \frac{\delta \cdot \frac{\delta - 1}{\delta + 1} \text{ for the heavier mineral}}{\delta_1 \cdot \frac{\delta_1 - 1}{\delta_1 + 1} \text{ for the lighter mineral}}.$$

And the greater the similarity of the dimension d in the particles forming a grade for simultaneous treatment within the said extreme limits of dissimilarity, the better will separation by density succeed.

It further appears from the preceding argument that what has been considered as equal-falling under the theories advanced by P. von Rittinger, and governing our ore-dressing practice hitherto, is not so, even in the limited application of the term to ultimate uniform falling velocity, yet is, in fact, of sufficient similarity of dimensions for practical separation by densities, leaving the conclusion intact, that the closer the similarity of dimensions the more effective the separation by falling velocities in a medium. And, again, the conclusion is inevitable, that the dimension, d, for length, measured in the direction of motion, alone and exclusively is of relative importance, and that transverse section and shape are without fundamental influence on the comparative velocity of immersed fall, the laws here stated applying to all similar shapes of solids, and not to spheres alone.

On the foundation of the newly discovered natural law, that a

stable volume, when moving in a medium, causes a quantity of the medium to move in inverse direction, the quantity of medium moving being equal in volume to the average transverse section of the moving stable volume, by the length of travel, and the distance for which the said quantity of medium is moved, being the length of the moving stable volume, measured in the direction of motion—on the foundation furnished by this natural law, and by the use of a new value in mechanics, viz., the value for transverse average section, it has been possible to measure correctly and clearly resistance to motion in submersion, to determine all velocities of fall of solids in submersion, whatever their form may be, and to provide a rational and general method for regulating practical ore-separation.

THE REFINING OF GOLD SULPHIDES PRODUCED BY THE PRECIPITATION OF GOLD FROM CHLORINE OR BROMINE SOLUTION WITH SULPHUROUS ACID AND HYDROGEN SULPHIDE.

BY WERNER LANGGUTH, PORTLAND, OREGON.

(Virginia Beach Meeting, February, 1894.)

SINCE the introduction of the improved method of precipitating gold from chlorine solution by SO_2 and H_2S at the Golden Reward chlorination-works, Deadwood, S. D.,* this modern method has been further adopted in the chlorination-works of the Portland Consolidated Company at Deadwood, and in those of the Black Hills Milling and Smelting Company at Rapid City, S. D. It has proved practically most successful in the handling of large quantities of gold solution, and constitutes up to date, with Mr. John E. Rothwell's most important improvement, namely, barrel-leaching under hydraulic pressure, the principal progress made in gold chlorination.

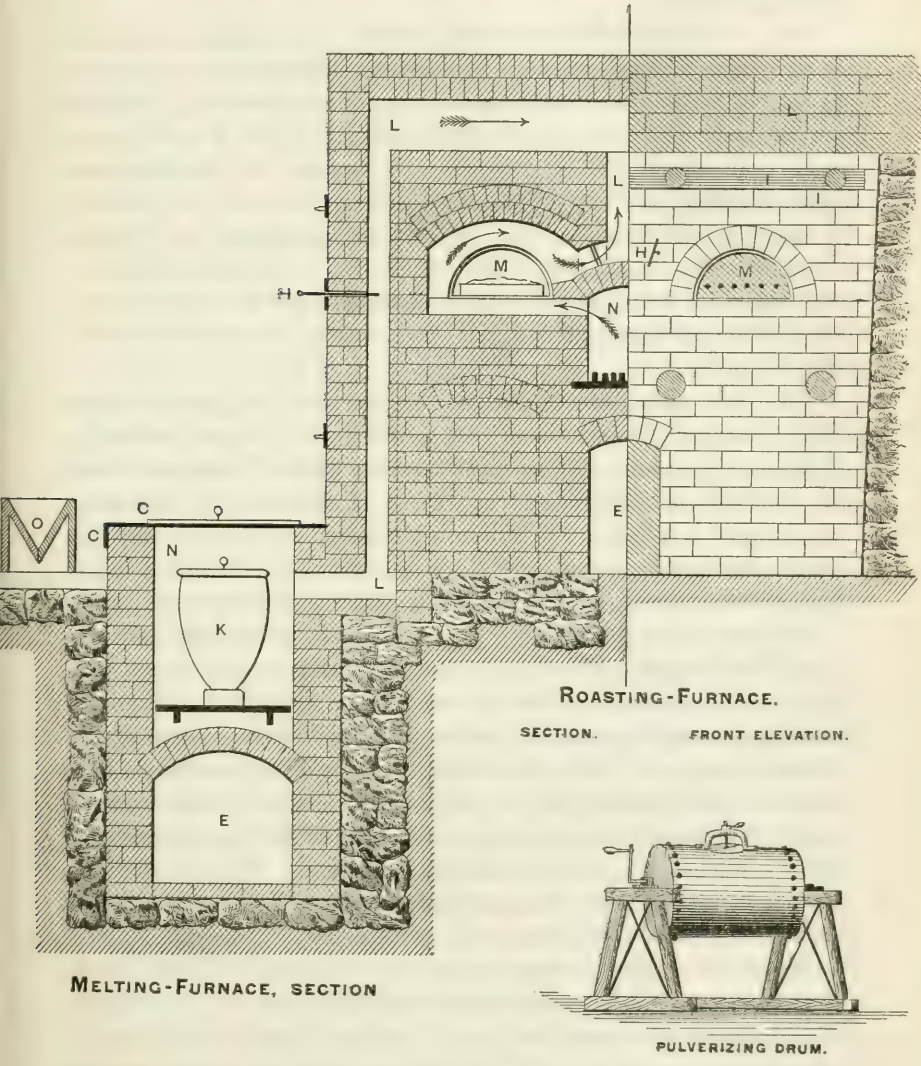
The method of refining the gold sulphides which result from this method of precipitation, was introduced by me in 1890 at the Golden Reward works, and in 1892 at those of the Black Hills Company. It has not suffered any changes during the last three years, and is substantially as described below:

* Described and discussed in the *Engineering and Mining Journal* of February 14 and March 21, 1891, and in *Trans.*, xxi., 314.

1.—*Drying and Roasting.*

The gold sulphides collected are dried already as far as practicable

FIG. 1.



Roasting- and Melting-Furnace, Golden Reward Works, 1890.

in the filter-press by passing compressed air through them, and are then transferred (care being taken to avoid loss in handling) to light sheet-iron pans, 20 inches wide, 36 inches long and 4 inches high.

Precipitate and filter-cloth are kept separate as much as possible. If dried well in the press, the precipitate is easily detached from the filter-cloth in pretty hard black-brown cakes. The pans, with precipitate and filter-cloth, are now introduced into the muffles of the roasting-furnace.

Fig. 1 is a sketch of the muffle roasting- and melting-furnace at the Golden Reward works. It gives the arrangement and front elevation, and makes a detailed description unnecessary. The parts are lettered as follows: C, cast-iron frame; D, brick floor of the refinery; E, ash-pit; H H, dampers; I I, braces; K, Dixon's plumbago crucible, No. 100; L L, flue to large chimney; M, muffle; N, fire-box; O, mould. The muffle is of cast-iron, 2 by 4 feet in plan, 12 inches high and 1 inch thick. The melting-furnace is 8 feet square, and its ash-pit connects with the outside of the building through an underground tunnel. The roasting-furnace is fired from the back side.

The later plant, constructed by the writer in 1892 for the Black Hills Company, is shown in Fig. 2, in which the parts are lettered as follows: A, chimney; B, cast-iron roasting-pans; C, cast-iron front; D, brick floor of the refinery; E, ash-pit; F, staircase; G, basement; H, damper; I I, braces; K, Dixon's plumbago crucible, No. 100; L, flue. The roasting-pans are 2 by 4 feet in area, with sides 4 inches high. The melting-furnace is round, and 3 feet in diameter.

At these works, the dried precipitate and the filter-cloth are transferred to the open cast-iron pans, B; the muffle is done away with, and the furnace is, in consequence, much simplified. Here the roasting-gases escape by the telescope-shaped stack or gas-catcher through the roof. The gas-catcher is balanced by counterweights. Its lower part is enlarged, and is lowered to about 4 inches from the top of the roasting-pans as soon as the furnace has been charged, to allow sufficient free access of air from all sides. If the draft is too strong, the gas-catcher is moved higher and can thus be regulated to get the best condition for roasting.

When the muffle or roasting-pans are charged, the heat in the furnace is kept gentle at first to drive off the moisture, and is raised little by little to a dark-red heat. Sulphur, arsenic and antimony are oxidized and driven off, and the filter-cloth quickly burns to ashes when detached from the precipitate. The whole treatment of a charge can be conducted within two to three hours. The mass now invariably presents a red-brown or yellow appearance, and only a very small percentage of arsenic and sulphur ought to be left. Some

care has to be exercised during the whole operation of roasting, not to lose any of the fine precipitate. The draft must be well regulated and stirring almost completely avoided. Steam-drying before roasting is unnecessary, and only adds to cost and labor.

2.—*Pulverizing and Fluxing.*

When sufficiently cooled, the roasted sulphides are carefully transferred by means of a hand-scoop and brush from the muffle or the pans into the pulverizing-drum (see Fig. 1), a cylindrical sheet-iron barrel, 3 feet in diameter and 4 feet long, running on trunnions, provided with an air-tight man-hole and revolved by means of a crank or pulley. Some good-sized cobble-stones put into it greatly assist pulverization.

A little borax, soda and niter is now added, according to the composition of the sulphides. Sometimes the solution from leaching has not been quite clear, and a perceptible amount of slimes (ore) has accumulated with the sulphides in the precipitating-tank; or the roasting before chlorination has not been as thorough as it might have been, and considerable quantities of arsenic and antimony have gone into solution with the gold in chlorination, and have been precipitated with SO_2 and H_2S, and not all eliminated in roasting the sulphides. All such circumstances have to be considered, and the necessary fluxes added, to secure a fusible slag of light specific gravity that will render possible the collection of the gold and a clear slag. If the ore treated in the mill is siliceous, the flux will have to be in general an alkaline one, such as soda, potash, etc. If, on the contrary, the ore is a basic one, a siliceous flux, such as glass or sand, etc., has to be added. If sulphur, arsenic or antimony has remained, niter or metallic iron may be added. Niter, however, must always be employed with caution, as it occasions violent action during fusion. As the conditions are always varying, the fluxing in each case has to be left to the discretion of the chemist or manipulator.

The fluxes are added direct to the sulphides in the pulverizing-drum, and become well mixed during pulverization. They should, at all times, be perfectly dry. Moisture in the flux, anywhere, will surely occasion loss during fusion, gold being carried away in the form of fine dust with the steam out of the crucible. Borax-glass is to be recommended instead of common crystallized borax.

The pulverizing-drum has proved to be the very best machine for this purpose; since, if it is carefully closed, no dusting and consequent loss are experienced.

3.—*Melting.*

The gold is now metallic, being reduced by heat in roasting;

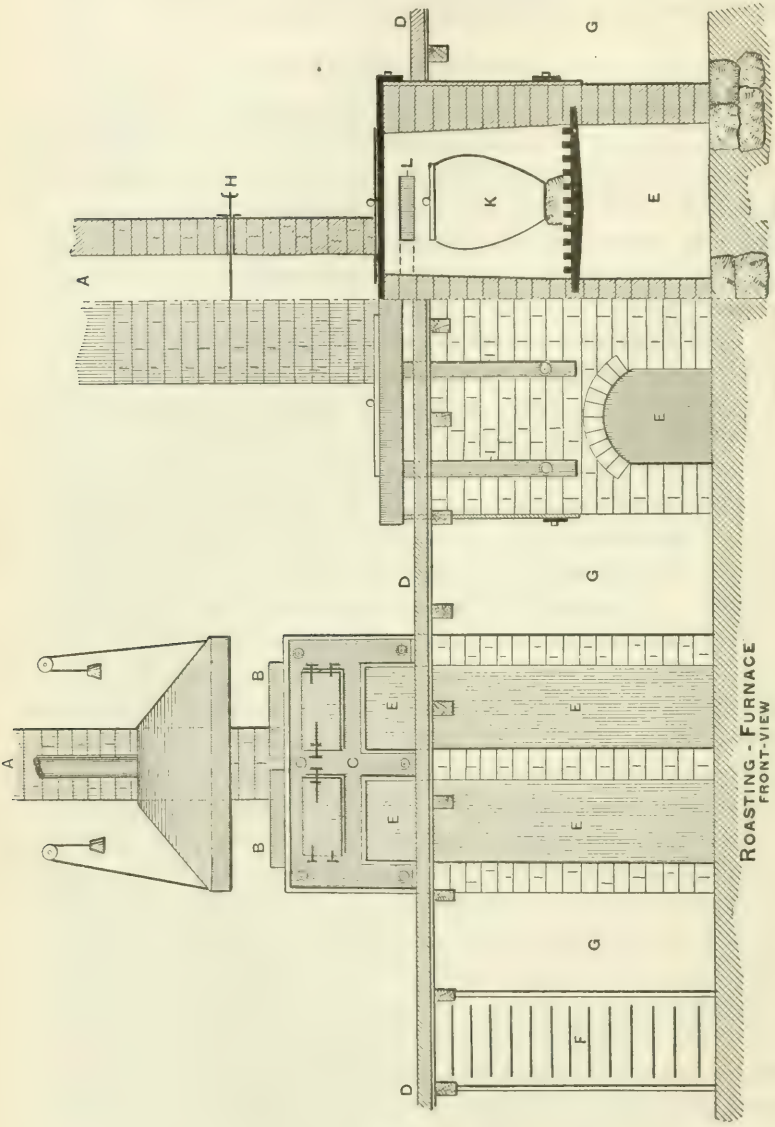


Fig. 2.

Roasting- and Melting-Furnace, Black Hills Chlorination-Works, 1892.

Au_2S_3 + heat = 2Au + 3S, and 3S + 6O = $3SO_2$. In melting the fluxed and roasted sulphides a crucible of good capacity, and yet easily handled, is of great importance. No. 100, Dixon's plumbago,

has proved a suitable size. A little borax-glass or slag is first put into the crucibles, and the mixture is then charged from the drum into the crucibles, each of which is filled to about 2 to 4 inches from the top, and a covering is given of borax-glass or rich slag from previous meltings, which will prevent loss by dusting while the contents are fusing. The crucibles are now placed in the furnace with the assistance of a pair of blocks and tackles and a basket-tongs. A lid is placed on the crucible, and the steadily increasing temperature soon fuses the contents without any boiling or violent action. After fusion the heat has to be kept at very high temperature for some time, to effect a complete collection of the smaller gold-globules. The crucible is then taken out and quickly poured into a conical mould of suitable capacity.

4.—*Bullion and Slag.*

The bullion separates from the slag in conical buttons. Each crucible-melt of good sulphides produces from 100 to 150 ounces of bullion, from .800 to .950 fine. Arsenic, antimony, copper, platinum and silver are the principal impurities. The buttons are remelted, as usual, and cast into a bullion-mould to be ready for shipment.

The resulting slags are still comparatively rich in gold. They are crushed and pulverized, and the gold-shots are panned out and added to the next melting. The tailings from this panning are dried and mixed with the slags of assays from the assay-office, or other lead-containing substances. Metallic iron is added, and the mixture is melted in crucibles which have served for sulphide-meltings a good many times already, but are considered not any longer quite as sound as is desired for this most important operation. The resulting lead-bullion is cupelled, yielding the remainder of the gold. The slags resulting from this second melting are too poor to be rehandled.

5.—*Losses.*

The losses in refining by this method are almost entirely mechanical, and depend, therefore, to a great extent on the care and skill of the operator. The flue-dust from the roasting- as well as the melting-furnaces has been assayed, at different times, after continued operations, but has shown comparatively little gold. There is, of course, some loss by volatilization in melting; but it is small, and

only noticeable when arsenic or antimony is present in large quantity.

I have once observed on the iron cover of the melting-furnace a white sublimate of arsenic, which showed in some places a beautiful pink color. Some of this pink sublimate was carefully gathered and assayed, and proved to contain considerable gold. Experience has shown me that this loss only occurs at very high temperatures (above the melting-point of gold), and in the presence of a large percentage of volatile metals, such as arsenic and antimony, and can be avoided altogether by careful roasting and fluxing.

6.—*Appendix.*

The chemical fact that bromine dissolves and extracts gold from ores is in itself nothing new; but that several thousand tons of gold-ore have been treated with technical as well as financial success, and the gold has been extracted not less easily—in fact, better and more cheaply—with bromine than with chlorine-solution, is surely worth knowing.

Last year the Asiatic cholera invaded Europe, and the price of chloride of lime advanced materially in consequence. I then employed bromine instead of chloride of lime and sulphuric acid, and practiced *barrel bromination*, at the Black Hills Works in Rapid City, S. D.

The cost of bromine was at first 35 cents per pound, but we received it later from the manufacturer, on contract, for 26 cents per pound. One to one and a half pounds, was all that was needed per ton of ore that had been roasted fairly well. On well-roasted ores, less than one pound was quite sufficient. The outlay of 26 to 40 cents for bromine per ton of ore, compared very favorably with 80 cents to $1 for chloride of lime and sulphuric acid; besides which, the handling was more convenient, and the extraction was from 50 cents to $1 better on ore that was not roasted as well as it ought to have been. We consequently changed altogether over to *bromination*. Thousands of tons have been brominated since, and I believe we have fully demonstrated, on a large scale, the technical and financial practicability of this branch of metallurgy. The methods of precipitation with the H_2S and SO_2, and the refining of the gold sulphides, as described above, worked as well on bromine-solution as it had done before on chlorine-solution, and made no changes of plant necessary.

THE CERRO DE PASCO MINING INDUSTRY.

BY OTTO F. PFORDTE, JERSEY CITY, N. J.

(Virginia Beach Meeting, February, 1894.)

THE silver-ore-deposits of the famous district of Cerro de Pasco were discovered in 1630 by Huari-Capcha, an Indian shepherd. In the early part of its history, the district yielded enormous riches. The records from 1784 to 1846 (excluding five years, 1821–25, during which civil war prevailed) show a product of 2,323,526 lbs. of silver; and the value of the total product since the discovery may be estimated at about $565,000,000.

The town of Cerro de Pasco is situated in the province of the same name, department of Junin, Peru, 14,250 feet above sea-level. The nearest railway station is on the line of the Callao and Cerro de Pasco R.R., at a distance of about three days' travel on horseback. The population of the town numbers about 6000, mostly native Indians.

Mines.—The mines are mainly located in the immediate neighborhood of the town and partly even under the principal streets, being distributed in several very large open cuts 250 to 350 feet deep called *tajos*, which extend in a nearly direct north and south line west of the town and are known as Tajo de Santa Rosa, Tajo Tingo or Portachuelo, Cajac Grande, Cajac Chico, Salsipuedes, Avella Fuerte, and several smaller ones. Tajo de Santa Rosa, the largest and most southerly, is closely connected on its north side with Tajo Tingo; while the others are entirely separate and lie northward in the order given; the extreme ends of the series being about a mile apart. Besides these, there are Tajo Descubridora a short distance east of Sta. Rosa and Tajo Matagente, about half a mile north of the main plaza in the town of Cerro de Pasco.

The argentiferous deposit consists mainly of a ferruginous sandstone, while the neighboring country-rock is limestone, conglomerate and slate.

The judicial fixed point from which all mining claims are measured, is a cross erected on a small elevation east of, and between *tajos* Sta. Rosa and Tingo, called Santa Catalina. This important

point, however, has not escaped displacement on account of the cavings in the ground, caused by the neighboring mines which have thus produced much disorder in the exact boundary-lines of the claims (*pertenencias.*)

The claims in the *tajos* are 30 by 60 *varas* (1 vara = 33 inches) in size. Those not located in the *tajos* are 100 by 200 *varas.* The close proximity of the claims and the general disorder underground are causes of frequent quarrels and litigation. At present but little ore is extracted by open cuts (*tajo abierto*), most of the mines being worked by means of small, steep and irregular inclines of which one entrance sometimes serves for two or three different claims.

The workings (*labores*) are very irregular and often dangerous, as but little care is given to the interior of the mines, except in very bad ground, where small timbers (*tincas*) and stones are used for support. The inclines generally receive a little more care.

Whenever a richer portion of ore (*bolsa*, literally, a purse) is found, all the ore is often extracted without leaving any bridges or pillars. Hence large boulders are sometimes seen in the roofs, which appear to be on the verge of falling, and occasionally do fall unexpectedly, so that accidents in the mines are common occurrences.

The ores are extracted by means of picks, and then broken with sledges to such a size that they can be placed in large leather sacks (*capachos*) holding from 75 to 150 pounds each, and carried out to the dump (*cancha*). In places where the ground is exceptionally hard, drills and powder or dynamite are used. Hoists or whims are not used, all the ores being carried to the surface by carriers (*apiris*). After the ores (*metales*) have been deposited on the dump they are separated into three classes, called *chancados* (ores spalled to a uniform size), *cernidos* (sifted ores remaining on a $\frac{1}{2}$ or $\frac{3}{4}$-inch screen), and *llampu* (the fines passing the screen). The last class contains but two *marcos* (1 *marco* = 7.5 ounces) of silver per *cajon* (6000 pounds), and is rejected. The first and second classes are measured separately in rudely-constructed boxes, three of which are considered as a *cajon*; and the ore is ready to be sold to the amalgamating-works (*haciendas*).

During the last century the necessity of drainage became apparent, and a tunnel, the *socabon de San Judas* was commenced in 1780 and completed in 1800. It is 3500 feet long and cost $100,000. Another tunnel, the *socabon de Quilacocha*, 88 feet below the former, was started in 1806, and, up to 1852, had been driven 8250 feet at

a cost of $1,000,000. At present, its total length is about 2 miles. At a distance of one mile from the mouth, it separates into two branches, aggregating about a mile in length. One of these branches connects with the Mesapata shaft, in the northern part of the town.

The Rumillana tunnel, said to be 180 feet below the preceding, or 400 feet from the surface, was started in 1877; it was calculated to be 2600 meters long, of which only about 300 had been completed when work was stopped.

Ores and Assaying.—The ores of the district from which by far the greatest portion of the silver is obtained, are oxidized surface-ores, called *pacos*, containing some silver in its native state. By long experience the miners are able to judge the quality of the ore by its appearance, though often the differences are very slight. Frequently, lots of ore are purchased by the mill-man (*haciendado*) without any special test, he being guided alone by the looks of the ore, and his previous experience with it. *Pacos*, at present, vary from 4 to 8 *marcos* per *cajon*. Ores having less than 4 *marcos* are worthless, as 4-*marcos* ore will just pay expenses. In general, the ores sold contain from 5 to 6 *marcos* per *cajon* of silver (say 12½ to 15 ounces per ton).

At the bottom of Tajo Tingo, a large bank of low-grade iron pyrites is seen; and it is very probable that pyrites (*bronce*) will also be found in the deeper workings of other *tajos*. These sulphides are so low in value that they cannot be worked profitably for silver with the present means, but are utilized in the manufacture of *magistral*, when they contain copper.

In the southwestern corner of Tajo Sta. Rosa, one mine produces a richer class of iron and copper pyrites, and also gray copper, the ore averaging about 35 *marcos* silver per *cajon*.

In Tajo Matagente cerussite is found scattered through the iron-oxide, and when properly separated by spalling is used by the smelters.

In some of the older mines, and in the Quilacocha tunnel, copper and iron sulphate crystals and incrustations, and iron-oxide incrustations are common.

Galena, and the richer sulphide ores, which are used by the smelters, are brought from mines which are not in the immediate neighborhood of the town.

Samples are taken from the *pacos* brought to the dump each day, after they have been spalled and sifted; all the samples of the week,

or of a lot, are then reduced and quartered, until a small general sample is obtained, which is ground very fine on a stone plate (*batane de piedra*) for the final test (*guia*). This test, which is merely an amalgamation on a small scale, is used throughout the district, and is conducted as follows:

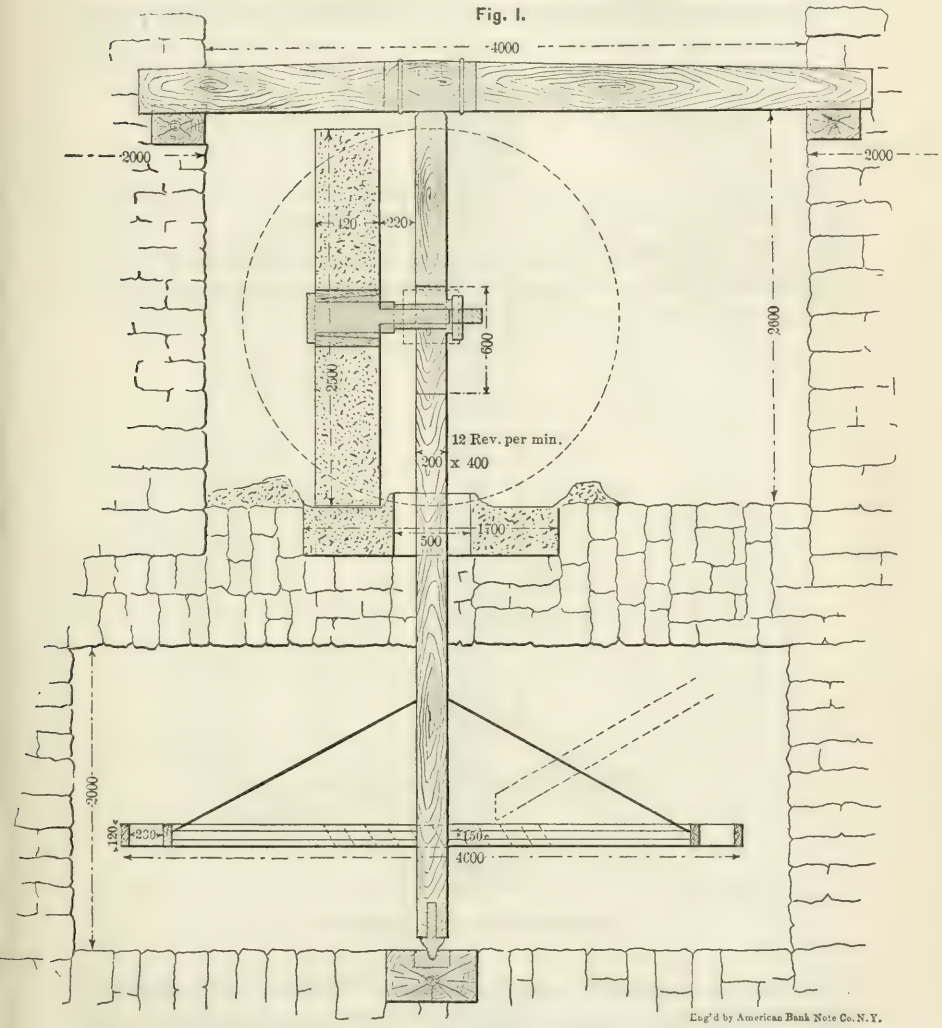
One pound of the finely ground pulp is moistened; 4 to 5 ounces of salt and a level teaspoonful of *magistral* (roasted ore containing copper sulphate), are added and thoroughly mixed by hand. Then, 2 *adarmes* (each equals 1.8 grammes) of mercury are added, and again mixed. The test is then left for 24 hours, and examined by washing a small portion of it in the *puruña*, a shallow earthen plate about 7 inches in diameter. In case too much *magistral* has been added, the test runs hot, the wash-water becomes milky, and a little lime-water may be added as a remedy. This test is generally run a trifle hot. In 48 hours it is completed, the pulp is carefully washed in the *puruña*, the mercury is all gathered, and the amalgam is skilfully separated with the thumb.

The amalgam and the mercury are then either weighed separately or merely judged by the volume of each. If the small globules of amalgam and mercury are equal in size, the ore contains 6 *marcos* per *cajon*; if the amalgam is one-half the size of the mercury, the ore contains 5 *marcos*, and if it is but one-third, the ore has 4 *marcos* at most, and will then pay expenses only. Ores of 5 *marcos* per *cajon* are worth from 8 to 10 *soles* (Peruvian silver dollars) per *cajon* at the mine, and 6-*marcos* ore is worth up to 15 *soles*.

Transportation.—Ore is transported from mine to mill with llamas or donkeys, the former taking 100 pounds, the latter 200 to 300 pounds at a load. If the mills are near the line of the Cerro de Pasco Mineral Railroad (which extends from the Cerro in a southwesterly direction for 8 or 9 miles, over a large plain, and passes by a number of mills), it is utilized for transportation, and the ores are then taken from the nearest station to the mill.

Mills and Milling.—The ores are amalgamated by the *patio* process in some sixty to seventy mills, most of which are at a distance from the mines, and may be divided geographically into two main groups. The first is composed of the mills situated in a large plain southwest of the town, beginning at the lower end of Quilacocha lake, 2½ miles from town, and extending 3 or 4 miles beyond. There are, in this group, 20 to 25 mills, of which many have easy access to the railroad, and almost all take their water from Quilacocha lake. The other group is distributed along the Puca-Yaco ravine, begin-

ning at 2 miles northeast of the town, and extending down toward Quinua about 4 miles. Quinua is 7 to 8 miles from Cerro de Pasco, and about 2400 feet lower, and contains the old mint which is now



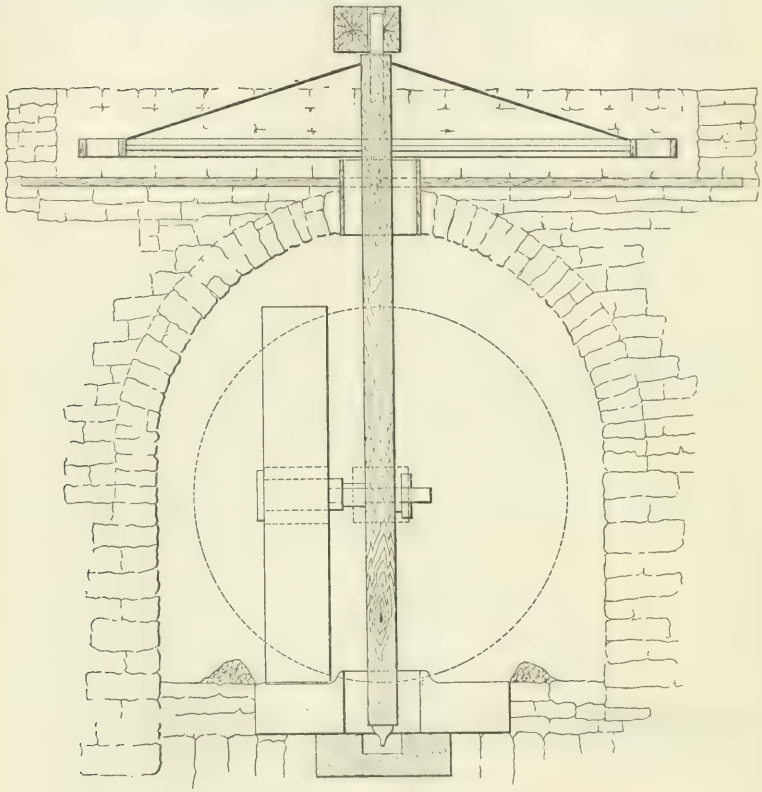
Vertical Section of an Injenio (Dimensions in millimeters).

in ruins. There are in this ravine 28 mills which take their excellent water-power from the brook flowing in it. The ores milled are nearly all from Cerro de Pasco, only a few of the lower mills taking ores from the neighborhood of Quinua.

There are several smaller mills on the road to Lima, and a few in the immediate vicinity of the town, among the latter being the largest and best-equipped mill, called "Hacienda Esperanza," half a mile southwest of the center of the town.

The plant which constitutes a mill or *hacienda* generally consists

Fig. 2.

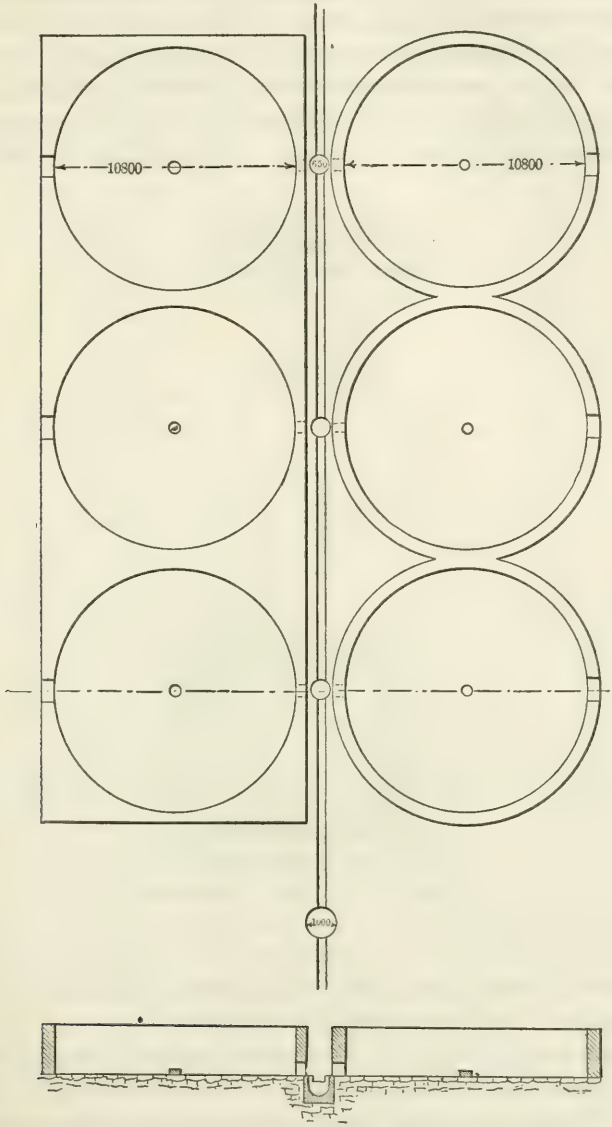


Vertical Section of a Tabladilla.

of a ditch, a horizontal water-wheel and Chilean mill combined, called *injenio* (Fig. 1), pulp and slime-settling tanks, built of crude masonry, a number of amalgamating *patios* called *circos*, which hold 5 *cajons* each. For continuous operation, each *injenio* requires 6 *circos*, 3 of which are emptied and recharged each month. Sluices and mercury-traps are attached to the *circos* (Fig. 3); a storehouse for chemicals and tools, an amalgam-filter and mercury-trough (Fig. 4), and a furnace and retort for distilling amalgam (Fig. 5). The

smaller mills sometimes lack the latter and use those of their neighbors.

Fig. 3.

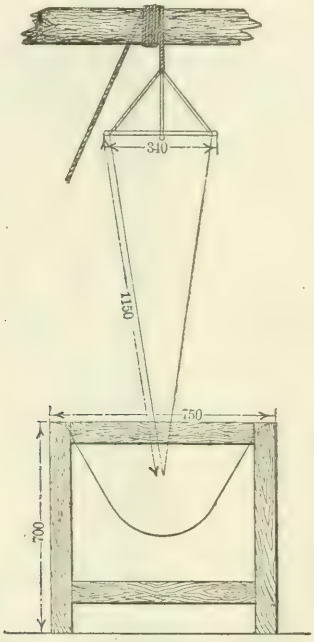


Circos (Dimensions in millimeters).

The size of the mills varies from one to twelve *injenios* with the required number of *circos*. In the drawings, Fig. 1 shows the vertical section of an *injenio*, which makes twelve revolutions per

minute, and grinds 1½ tons of ore in twenty-four hours. The mill consists of a vertical shaft called *peon*, the upper end of which revolves in the bearing of a horizontal cross-beam, the *madre*, while the lower end revolves on a pivot (*goison*). At the lower part of the shaft the water-wheel is arranged in a vault (*boveda*). The upper portion of the *peon* contains a slot for allowing a vertical sliding motion to a bearing of the mill-stone pivot, so that the height of the bearing can adjust itself according to the wear of the mill-stones,

Fig. 4.



Manga (Dimensions in millimeters).

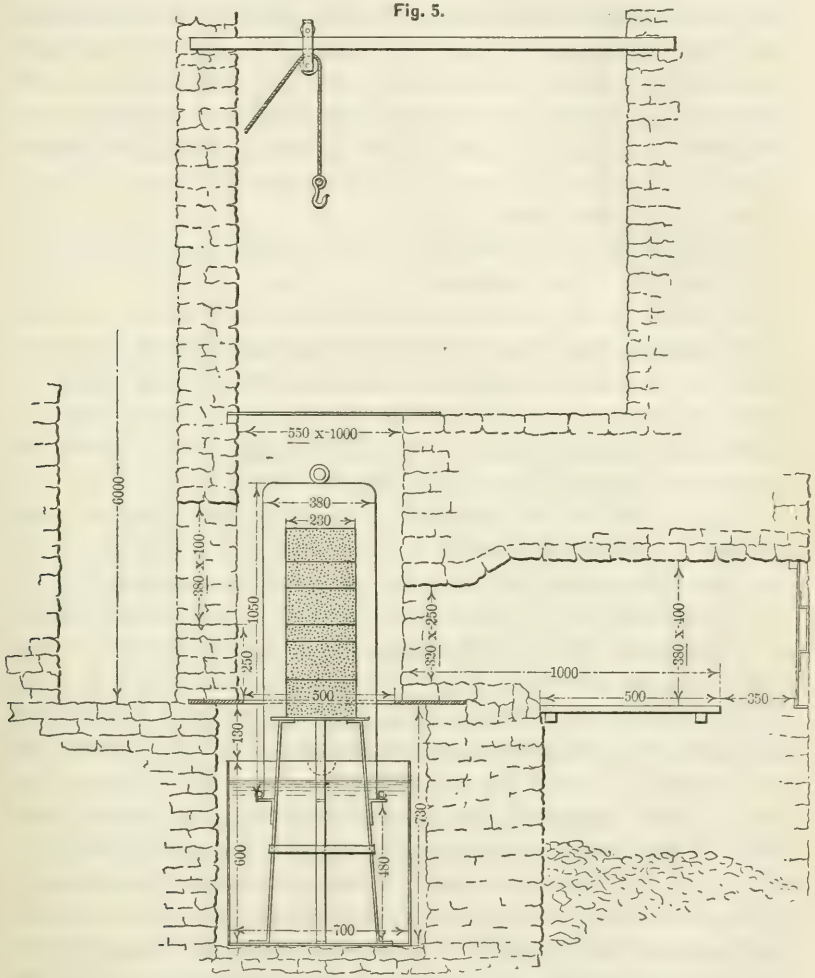
the upper one of which is called *voladera*, the lower, fixed one being the *solera*. When a *voladera* has been worn down to 1.5–1.7 meters in diameter, it is further utilized as a *solera*.

The *tabladilla*, Fig. 2, is an inverted *injenio* in which the horizontal water-wheel is arranged above the mill-stones. It is rarely used. Its dimensions are about the same as those of Fig. 1.

Fig. 3 represents the general arrangement of the *circos*, of which there are two types, one having a crude circular wall 0.6 to 0.7 m. thick, about 1.5 m. high, and 10.8 m. inside diameter; while in the other the *circos* are built in a square mass of masonry, as shown in

the figure. There are two openings in each *circo*, one for charging the pulp and admitting the animals; the other, a small hole near the floor, through which the pulp or *masa* is discharged into the *capitana* for washing. The floor of the *circo* is well laid with flat stones and

Fig. 5.



Retorting-Furnace (Dimensions in millimeters).

plastered carefully with cement to avoid losses in the crevices. In the center of each *circo* a low cylindrical stone, the *templador*, is fixed to serve as a stand for the mule-driver.

Fig. 4 shows a mercury-filter (*manga*), the upper part of which is made of leather sewed to an iron ring, and the lower part of canvas.

The filtered mercury drops into the *bilca* made of raw hide and held in a wooden frame below.

Fig. 5 represents a retorting-furnace, which contains a tripod set in a water-box. The pressed amalgam-cakes (*quesos*) are placed upon the tripod; a sheet-iron cylindrical cap (*caperuza*) is lowered so that its bottom shall be a trifle below the surface of the water in the box; the furnace is then closed with strong sheet-iron and temporary masonry, and a coal-fire is maintained upon the grate for about four hours with a gradually increasing heat until no more mercury is evaporated; finally, after cooling, the furnace is opened, the retort-silver (*plata piña*) is removed, and the mercury (*azogue*) is dipped from the tank into flasks.

Exceptions to the general arrangement described above are found at the few following *haciendas:*

At Esperanza there are twelve *injenios* and eighty *circos;* a steam-plant that has been in use for twenty-five years produces the power, and the pulp passes directly from the *injenios* to the *circos*, which is a great advantage in saving transportation. This *hacienda* has also a good repair-shop, a modern retorting-apparatus, and several important coal-mines, which produce good coking coal.

At the Trinidad mill, the first one in the Puca-Yaco ravine, a wooden over-shot water-wheel is used; and at the Quiyapata mill on Lake Quilacocha, the *tabladilla* shown in Fig. 2 is used.

At *hacienda* Puca-Yaco, the second mill of the Puca-Yaco ravine, a modern stamp-mill of ten stamps, weighing 450 to 500 pounds each, has been erected. The ore is stamped through a 30-mesh screen and passes into eight settling-tanks, from which it is charged into four 5-foot amalgamating-pans, and from these discharged into two settlers. There is also a small clean-up pan and a modern retort. Power is obtained from a turbine having 50 to 60 feet pressure. When water is deficient, a steam-engine is used. This stamp-mill is not used now, on account of the poor results it has given.

Interruptions of work in the Cerro are frequent, and mainly due to the lack of water, which is low during three or four months in the year, especially in those mills that are dependent upon the water from the lakes. Freshets sometimes destroy the dams; the replacing of the millstones causes delay; and financial difficulties frequently produce entire suspension of the work in poorer mills.

The millstones of the plain are of fine hard granite which wears very uniformly. They are obtained from quarries in Cerro Raco, an isolated mountain, about eight miles south of the town. Those

of the Puca-Yaco ravine are quarried near the bottom of the ravine and are also of very hard rock, but develop, in wearing, ridges and hollows in the working-face. These, however, are considered as rather advantageous than otherwise. These large stones are transported by passing a round stick through the center and fixing it into a rude frame, after which sixteen to twenty oxen are hitched to the frame and the stone is trundled to the mill. A *voladera* costs about 100 soles at the quarry (*cantara*) and 125 soles for transportation.

For milling the ores the sluice of the head-race is opened and the *injenio* is set in motion, making twelve revolutions per minute. Water is allowed to flow into the lower mill-stone, the circumference of which is dammed up a few inches with sods, excepting at a discharge-opening. The ore is fed gradually by a laborer and the pulp is carried to the settling tanks (*cochas*) by the overflow, from which the slimes pass into other tanks, where they can settle, in order to receive separate treatment subsequently in *circos;* the losses from the slimes being greater than from the pulp.

When one of the tanks is full the water is drawn off, and the pulp is allowed to dry a little to facilitate transportation to the *circos* which is done in leather sacks (*capachos*). Thirty-five heaps of about equal size are formed in the *circo*, each heap containing a certain number of sack-loads. This is the only method of measuring the contents of the *circo*, which when charged contains five *cajones* or fifteen tons. *Circos* of four or six *cajones* are rarely used.

The small pulp-heaps are then levelled over the floor making a *tortilla;* 40 *arrobas* (1 *arroba* = 25 pounds) of salt are added, and eight small horses are driven around in the circle and thoroughly mix up the charge, the driver standing on the elevated central *templador*. Then 15 to 30 pounds of *magistral* are scattered on the charge, which is again mixed by the horses; after that 80 pounds of mercury are finally distributed over the mass (*incorporar la masa*) which is again thoroughly mixed.

The horses are then introduced and driven in the *circo* for one and a half to two hours every eight or ten days until at the end of two months amalgamation is completed. In the meantime, however, the charge must be tested carefully by washing small amounts in the *puruña*, and mercury must be added when the previous charge is absorbed, until finally the total amount added is 230 pounds.

If the charge "goes cold" some magistral is added, and if too hot, some lime or lime-water.

Two months after charging, the entire mass is gradually shoveled

into a hemispherical washing-trough of about two feet diameter, called *capitana*, made of one stone, which is placed in front of the discharge of the *circo* so that one *capitana* serves for two adjacent *circos*.

Water flows into the trough; an intelligent, strong laborer enters the latter, and with his feet mixes the pulp and water so as to cause the amalgam and mercury to settle. The tailings (*relaves*) pass through several troughs (the connecting channels of which are lined with coarse cloth) and a mercury-trap, and are finally discharged, but always require careful testing with the *puruña*. There is generally a small loss in amalgam floating off; and in some mills, Indians re-wash the tailings by hand, making a small profit. Finally the *circo* is well cleaned, especially in the crevices, and after all the pulp has been washed and the mercury and amalgam have been cleaned of sand and pebbles, they are dipped into raw hides and carried to the filter. The sluices and mercury traps are also cleaned out and their product is added.

Filtering and Retorting.—The amalgam is then filtered off in a strong canvas filter (*manga*), which is suspended over a raw-hide mercury trough (*bilca*), and the filter is beaten with a flat stick from one to two hours, to press out all the mercury possible. The amalgam (*pella*) is then taken from the filter and pressed into cylindrical moulds of 230 mm. diameter, the height of each separate part being varied according to desire by placing a circular sheet of paper betwen the consecutive cakes. It is then taken to the retort, which is very simple in construction, and is locally known as *cape ruza* (see Fig. 5). Generally the retort is charged late in the afternoon, fired up with coal for three or four hours and allowed to cool until morning when the retorted silver (*queso*) is extracted, weighed, marked, and taken to the government refinery (*Casa de fundicion de barras*) in town, or sold to the *rescatador*, who has the privilege to redistil (*refogar*) the retort-silver before purchase, to verify its weight. The washing of the pulp, filtering and retorting are generally done about the first of each month, and the refinery is operated for three or four days, beginning on the second or third of the month.

Refining.—In the refinery the retorts are received and weighed; names and weights are entered in the record, and each lot is kept separate if it is sufficient to make one or more bars (*barras*) of 150 pounds or 300 *marcos* each; if not, the owner generally sells the difference directly to a *rescatador*. In the refinery the furnace has step-grates, and is arranged for two large iron crucibles, holding

about 150 pounds of retort each. These crucibles are suspended from cranes in order to facilitate the manipulation. The fuel is a bituminous shale, found near the town and costing 8 soles per ton. It requires about seventy-five to eighty minutes to melt a charge placed in a hot crucible. The melting is watched through small peep-holes in the cover of the furnace, and when it is completed the crucible is extracted with the crane, which catches under two lugs attached to the outer rim of the crucible. The latter is then set into and tightly gripped by a frame having a vertical toothed quadrant, which is acted upon by a worm-wheel turned by hand, whereby the crucible is gradually brought into a horizontal position, thus pouring the molten silver uniformly into an iron mould set beneath.

Sprouting is prevented by scattering a handful of dry straw in each mould before pouring, and some more straw on top of the silver after it is poured. The crucible is then scraped clean, recharged, and set into the furnace. After the silver in the mould has solidified, it is dumped upon an iron plate with the crane, and then pulled into a water-trough for cooling, after which it is withdrawn; the rough edges are hammered down; and the bar is weighed, marked, and delivered to the owner, together with the scrapings. In melting, a loss from 2 to 6 *marcos* is found per bar on its retort weight, mainly due to the mercury contained in it. Each iron crucible will stand about 40 meltings before it becomes useless.

A charge of 2 cents per *marco* must be paid for melting. All the silver produced in the district must pass through the government refinery, and the total monthly product of the district averages, at present (1890), from 40 to 45 bars of about 150 pounds or 300 *marcos* each, by far the largest part of which is obtained by amalgamation.

Silver-Lead Smelting.—This is carried on in Cerro de Pasco on a small scale, exclusively in *pachamanca* reverberatory furnaces, of which a description and sketch will be found in *Trans.*, xxi., 27. There are four of these old furnaces in town, but only two or three of them are in intermittent operation. The fuel used is coal, bituminous shale, and *taquia* (dried llama-dung). The number of medium-sized slag-dumps seems to show that smelting was more flourishing in former times, when the ores were richer. As lead has no commercial value at the Cerro, it is allowed to run into the slag or litharge, and is used again in smelting siliceous charges.

There are several establishments in town for the production of

magistral, which is made by calcining iron and copper pyrites to a dead roast in a reverberatory furnace.

A small leaching-plant has also been erected in town, but its results were unsatisfactory.

Labor, General Resources, and Finance.—Labor at the mines and mills is almost entirely done by native Indians (*peons*), and, whenever possible, it is done by contract, so that a man can earn a little more than at day's wages, which average from 50 to 60 cents per day.

One of the difficulties with the peons is their great fondness for alcoholic stimulants, which is generally indulged toward the end of the week by consuming large quantities of *chacta*, a liquid distilled from the juice of the sugar-cane. Hence, but little work is done on Fridays and Saturdays. The natural resources of the town, excepting the mines, are very scanty. In the neighborhood good coking-coal and bituminous shale are found; at Quinua, there are strata of bituminous shale, and also limestone formations deposited by hot springs. Sheep- and cattle-raising is carried on largely in the plain of Lake Junin, 10 to 12 miles distant, while the lake presents excellent opportunities for hunting geese and ducks. At the foot of the Puca-Yaco ravine, 7 to 8 miles distant from town, and 2400 feet lower, a good agricultural region is found in a long but narrow valley called Quebrada de Quinua, where almost every available spot of soil is utilized. The valley contains the agricultural villages of Quinua (11,800 feet above sea-level), Cajamarquilla (11,400 feet), Malanchac (10,800 feet), and Huariaca (9700 feet). Jarria (11,700 feet) is situated in a side-valley, a day's travel from town, which produces excellent potatoes. The climate in these valleys is very mild and warm when compared with that of the Cerro.

Wood for fuel is brought from the neighboring valleys, while that used for building must be brought from a greater distance.

Salt for amalgamation is brought from the great salt-mine San Blas, one day's travel from the Cerro. The price of the salt varies from 1.40 to 2.20 soles per 100 pounds, and, as there is no competition excepting that of sea-salt, which takes 6 to 7 days to reach the Cerro from Huacho, the owner of the San Blas mines makes his own prices, and has all of the Cerro amalgamators under his thumb.

As already remarked, the millstones of very fine granite (*ala de mosca*) are made in the Cerro Raco, 8 miles south of the town, some also being made in the Puca-Yaco ravine.

All other articles of convenience or necessity must be brought

from the coast with the exception of such as skilled artisans can make from the materials on hand. This exception includes silverware, goods made of leather, and the product of spinning and weaving. The Indians make all their own cloth from sheep or alpaca wool.

The financial system is very disadvantageous, especially to the poorer mill-owners, if they are obliged, as most of them are, to have recourse to the money-lender (*habilitador*), who advances money at a high rate of interest, and to whom the amalgamator is obliged to turn over his entire product, receiving 10, 20, and even 30 cents less per *marco* for his silver. It is not uncommon that some mill-owners are constantly in debt, and perhaps finally lose their mills.

Literature.—Further information concerning the topography and geology of the Cerro de Pasco may be found in the paper by Mr. A. D. Hodges, Jr., on that subject, *Trans.*, xvi., 729. The *Eng. and Min. Journal* of Dec. 21, 1878, contains a fairly good local map, and a number of views of the country. A very elaborate, though now somewhat antiquated description and history is given in De Rivero's *Memorias Cientificas*, vol. i., Brussels, 1857.

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*THE FRANKLINITE-DEPOSITS OF MINE HILL, SUSSEX COUNTY, NEW JERSEY.**

BY FRANK L. NASON, NEW BRUNSWICK, N. J.

(Virginia Beach Meeting, February, 1894.)

RECENT explorations with a diamond-drill, originated and conducted by Mr. J. A. Van Mater, Superintendent of the Lehigh Zinc and Iron Company, have revealed some very important geological facts, which, it is believed, will be of special practical interest to mining engineers. During these explorations the writer was frequently called in consultation, and hence the facts here recorded are almost entirely the results of personal observation.

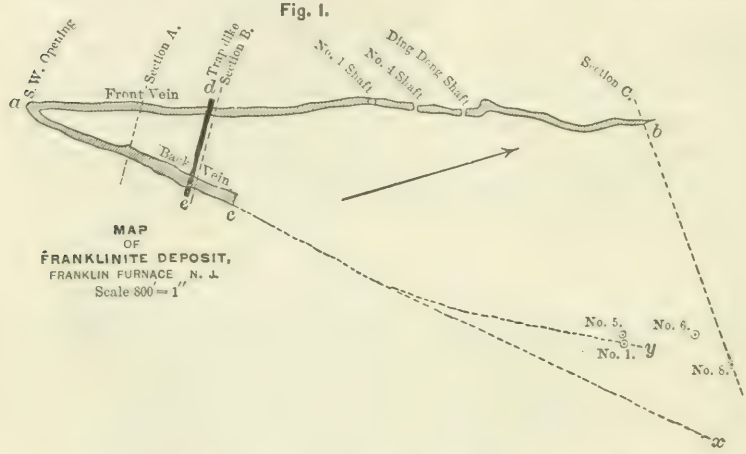
The franklinite-deposit upon which the explorations were conducted has a long continuous outcrop. Figs. 1, 2 and 3 give a map and sections. Beginning at the extreme southern point,

* This mine is at Franklin Furnace, in Hardyston township, 2 miles north of a nearly similar deposit at Stirling Hill. Both deposits are on the line of the New York, Susquehanna and Western Railroad, 67 miles from New York city.

known as the southwest opening (see Fig. 1, *a*),* the outcrop strikes N. 16° E. for 2970 feet, where it disappears from the surface. Again starting from the southwest opening, a shorter outcrop, continuous with the first, runs N. 53° E. for 480 feet, when it also disappears from the surface (at *c*, Fig. 1). At the point of its disappearance a trap dike of mica-diabase cuts across the ore-body, and extends nearly east and west for a long distance on either side of both outcrops (see Fig. 1, *d*, *e*).

When the ore-body was worked on this shorter arm the ore was taken from an open cut up to the dike. For a long time it was supposed that the dike was the terminus of the ore in this direction. An opening was finally driven through the dike, and ore was again

Fig. 1.

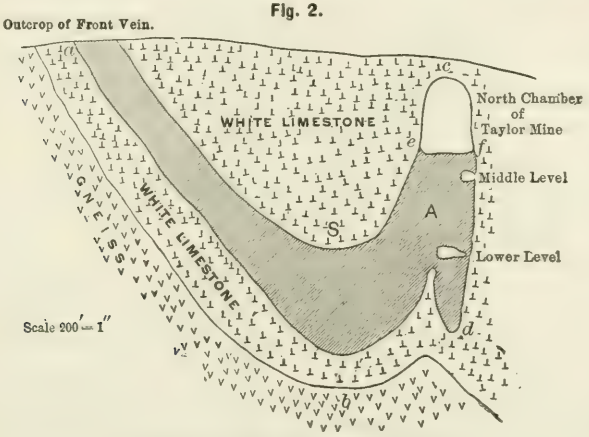


struck within a distance of 20 feet. The removal of the ore was then begun from behind the dike, and it was found that the ore instead of coming again to the surface, *pitched* underground at an angle of 27° to 30°. At the present time the ore-body has been mined out on this slope for a distance of over 300 feet. The height from the slope to the roof of the mine is about 90 feet. The ore has been mined out clean with no pillars or timbers. There is practically no foot-wall to the mine, the bed standing vertically. The country-rock on either side slopes at a slight angle toward the ore-body for a distance, then, converging more rapidly toward the top, meets in the form of a hyperbolic arch directly overhead. (For the shape of this arch see Fig. 2, north chamber of Taylor mine.)

* The drawings for this paper were made by Mr. R. K. Painter and Mrs. F. L. Nason.

With the exception of a small lot of ore previously taken from the southwest opening, the removal of the ore from the open cut and the opening into the chamber north of the trap-dike was the only development work done on South Mine Hill prior to the late exploration. Nothing was practically known of the front vein (see Fig. 1, *a*, *b*).* In 1877 a lease of 500 feet of the front vein on North Mine Hill was taken by Mr. Charles W. Trotter, of Brooklyn. The dividing-line of the hill was east and west, about midway between the points, *a* and *b*, Fig. 1. Here the deposit is much narrower and leaner than on the back vein. Mining operations were begun here shortly after. Shafts were sunk on the vein by Mr. Trotter; and, still further north, other shafts were sunk by the (then) New Jersey Zinc Company to depths ranging from 80 to 180 feet. At

Fig. 2.



Franklinite-Deposit, Section B (Fig. 1).

the present time the Trotter mine is worked by the Lehigh Zinc and Iron Company, which has two shafts now in working order. Shaft No. 1 is sunk on the slope of the deposit to a depth of 400 feet; and Shaft No. 4 to 500 feet. These shafts show that the ore-body slopes or dips to the S.E. at an angle varying from 55° to 60°.

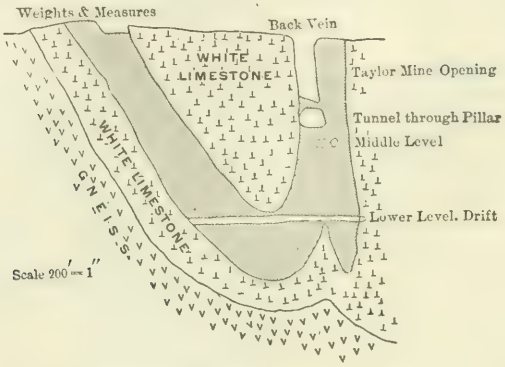
At the 400-foot level of the Trotter mine a diamond-drill hole in the foot-wall showed the width of the deposit from the foot to the hanging-wall to be upwards of 50 feet. The greater part of the ore-

* About 1872, Congress ordered the preparation of official standards of weights and measures, and at the request of Colonel Fowler, a Representative from New Jersey, and owner of this property, the necessary zinc for the alloy was taken from this front-vein outcrop. The opening, which has not been touched since, is still visible, and goes by the name of "Weights and Measures" (see Fig. 3).

body is, however, too lean to be worked profitably. Though the ore-body is leaner in the workings at North Mine Hill, there are, even at the depth of 500 feet, no signs of exhaustion, and there are reasons, which will be pointed out later, for believing that the ore-body will improve with increased depth.

Returning to the back vein: about the year 1888, at the suggestion of Dr. George H. Cook, a cross-cut was run from the lower level of the Taylor mine (see Fig. 3) to the foot-wall of the front vein. The main part of the cross-cut was in ore, but about midway between the ends the upper part of the drift, for 50 feet or more, cut through the ore-body and into the limestone forming the

Fig. 3.



Franklinite-Deposit, Section A (Fig. 1).

immediate country-rock. Previous to the running of this cross-cut it had been popularly supposed that there were two distinct and independent deposits at Mine Hill, called the back and front veins. Geologists, however (Dr. Cook especially), held the opinion that the two beds were in reality one and the same, and that the back vein (see Fig. 1, *a*, *c*) was but the folded eastern edge of the body having the longer outcrop. Among the facts leading to this conclusion on the part of geologists, the relative widths of the two beds stood out prominently. At the southwest opening, the distance across the opening, which represents the ore taken out, is 75 feet. The entire distance from wall to wall, including lean ore, is upwards of 100 feet.

The average width of the outcrop on South Mine Hill is about 35 feet, though it reaches 40 feet in places. The width of the deposit in the chamber (see Fig. 2, *e*, *f*,) is 70 feet. The distance between the walls of the open cut is likewise about 70 feet. Taking the fact

that the back vein was just twice the average width of the front vein, in connection with the beautiful arch in the north chamber, the idea suggested itself that the back vein was a collapsed anticlinal fold of the eastern edge of the deposit.

The fact that the southwest opening seemed to lie in a trough-shaped depression suggested the idea that the front and back veins were connected by an underground synclinal trough. This induced Dr. Cook to have the cross-cut driven, which is shown in Fig. 3. The completion of the cut is regarded as establishing the view that the apparently two veins or beds were, in reality, but one. The bed has been folded first into a synclinal trough, and second, its eastern edge was folded into a collapsed or compressed anticline. The relations of these two folds is shown in cross-section, Fig. 2, where the synclinal trough is marked by S, the anticlinal fold by A. When it was found that the back vein extended beyond the trap-dike, and before the significance of the steep pitch to the N.E. was fully understood, it was expected that the ore-body would again appear either at or near the surface on the line of strike. Search was made, but attempts to find the outcrop were unsuccessful. Long costeaning-trenches were dug, at right angles to the line of strike, down to the underlying rock, but no traces of ore were found. Shallow pits (the deepest being only 50 feet) were dug in the white limestone, and afterwards a shaft was sunk to the depth of 180 feet. It went all the way in white limestone, showing no traces of ore save a little magnetite. The full significance of the pitch was not understood. As a matter of fact, if either the pits or the more pretentious shaft had been carried to the proper depths, all would have struck the deposit sought for. A very simple calculation would have convinced the prospectors that it was useless to look for ore at a less depth than 300 to 600 feet.

In 1888, or 1889, a diamond-drill hole was put down by the New Jersey Zinc and Iron Company, about 600 feet north of the trap-dike. The drill was set at an angle of 30° to the surface, or at (calculated) right-angles to the dip of the front vein; and it struck the ore, passed through the limestone foot-wall, and the bed of magnetite which underlies the limestone formation, and was stopped in the gneissic rock, 1100 feet from the starting point. This drill traversed only one bed of zinc-ore. It evidently passed over the crest of the anticlinal fold in the back vein. It did nothing more, therefore, than to prove the extension downward of the front vein, at a point south of the No. 1 shaft in the Trotter mine.

In 1890, the first intelligent prospecting was begun by Mr. J. A. Van Mater, for the Lehigh Zinc and Iron Company. Mr. Van Mater had been connected with the deposits on North Mine Hill since about 1882. Though not a trained geologist, he has been a close observer of the position of the ore-body in the limestone, and its general appearance as disclosed by the mining operations of both the New Jersey and Lehigh companies. His observations led him to the conclusion that the richer part of the ore-body lay to the east of the Trotter mine, and that the Trotter mine itself was but the leaner western part of the main ore-body. He was also the first to put into practical form the significance of the pitch of the back vein. From the size of this vein, he concluded that it could not, necessarily, be expected to pinch out at a less distance than the leaner outcrop at the north end of the front vein.

The writer frequently discussed this question with Mr. Van Mater, but was not called in official consultation until several of the drill-holes had been put down.* The first drill-hole was located 1975 feet from the trap-dike, where the ore on the back vein disappears. At a depth of 947 feet the ore was struck. The drill continued in ore for 26 feet. The hole was carried to a depth of 1378 feet, and was abandoned when it entered the gneissic rock. At 1100 feet, it had passed through 13 feet of magnetite.

The drill was then moved 50 feet east, and a hole was again carried down to the gneissic rock, which was struck at 1244 feet. Much to the surprise of the drillers, no ore was struck in this hole. The drill was then moved 25 feet to the west, a point midway between holes No. 1 and No. 2. The hole was carried to a depth of 1095 feet. Magnetite was struck at 1069 feet and 1085 feet, but no franklinite ore.

Bore-hole No. 4 was then started a little north, and about 25 feet west, of No. 1. A sand seam was encountered at 328 feet. The drill was carelessly stopped by one of the workmen who was tending it. The consequence was that, before the head driller could be summoned, sand packed in around the rod and bit, and 100 or 200 feet of the rod was lost.

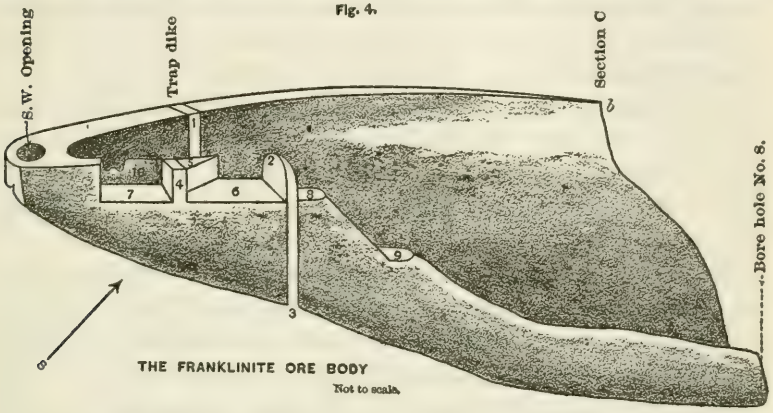
Drill-hole No. 5 was started at nearly the same spot, and was carried to a depth of 1112 feet. Ore was struck at a depth of 890 feet

* It may not be out of place to say here, that the writer was connected with the Geological Survey of New Jersey, as assistant geologist, for three years, and hence had ample opportunity to study the geology of this deposit. It was with regard to the geological aspects of the case that he was consulted.

and left behind at 1015 feet, giving a thickness of 125 feet all told. Bore-hole No. 6 was started at a point 300 feet from No. 1, and about 75 feet further to the west. It was carried to a depth of 1083 feet. At 937 feet ore was struck, and proved to be 108 feet thick. In both of the above holes magnetite was struck under the franklinite-bed.

Bore-hole No. 7 failed to strike the ore-body sought for, though put down to a depth of 1327 feet. Magnetite was found as usual. Bore-hole No. 8 was begun February 24, 1891, at a point 500 feet

Fig. 4.



1, 2, 3, Trap dike; 4, Pillar of ore; 5, Lean ore left standing; 6 and 7, Floor of open-cut, Taylor or Buckwheat mine; 8–9, Slope parallel with pitch of ore-body, and 90 feet from roof; 10, Lean ore left standing; *b*, Point where back vein disappears under the surface; *s*, Standpoint of observer.

from No. 1, and in line with it, but a little west. Drilling was stopped after a depth of 1180 feet had been reached. The deposit was struck at 1009 feet and 135 feet of vein matter, 77 feet of which was good ore, was traversed. Magnetite was also found under the bed of franklinite, showing that the bed outcropping on the surface under the franklinite is very persistent. Unfortunately, this bed of iron is too thin to be worked.

The results of these holes were considered to be favorable enough to warrant the sinking of a shaft. Work was soon after begun, and a fine shaft, 10 by 20 feet in size, has already been carried down to a depth of 700 feet. It is proposed to sink it to 1100 feet.

From these borings the general shape of the ore-body is thought to be proved. This shape is roughly shown in Fig. 4. From this figure it will be seen that the anticlinal fold continues from the trap-dike to at least as far as bore-hole No. 8, and that, of course, the syn-

clinal trough follows a little to the west. Another point appears to be true, to wit, the compression of neither the anticlinal nor of the synclinal fold seems to be so acute at the north as at the south end of the hill.

It will be remembered that the strike of the back vein is N. 53° E. This strike would carry the ore-body along the line *cx*, Fig. 1. In fact, drill-hole No. 1 was put down 100 feet, and No. 8 300 feet, west of this line. When it is recalled that No. 1 passed through only 26 feet of ore and that at 25 and 50 feet east the drills missed the ore altogether, it is easy to draw the conclusion that from the point *c*, Fig. 1, the ore-body, as represented by the anticlinal fold, curves to the west. That the fold preserves much the same shape, where the borings are made, as is proved at the Taylor mine, is shown by the fact that No. 2 showed no ore; No. 1, 25 feet west, showed 26 feet; and No. 5, about 25 feet west of No. 1, showed 125 feet of ore. No. 8 is a little further west, and shows 135 feet of ore.

It will appear from the workings in the mines, briefly described as the Taylor, at South Mine Hill, and the Trotter, at North Mine Hill, and from the development of the drill-holes, that this is one of the largest mineral deposits in this section of the United States. In fact, in point of size, it will compare very favorably with any of the mines in the whole country. The width of the bed, reduced to a plane, at the southwest opening, is about 100 feet. Following down the slope of the front vein, west of the chamber, then rising up the west slope of the anticlinal fold, and then down the eastern slope, the folds reduced to a plane would be at least 750 feet across. From the Trotter mine east to No. 1 bore-hole, reducing the ore-body to a plane as before, the width is about 1800 feet. In other words, if the ore-body were flattened out, it would have an average width of at least 800 feet. The average thickness of the ore-body at the fold in the Taylor mine is 35 feet. (The width across the fold is 70 feet, or double the true thickness of the bed.) At the outcrop of the Trotter mine the bed, from foot to hanging-wall, averages 15 feet.

The average thickness of the bed is, therefore, at least, 25 feet. The length of the bed from the southwest opening to the extreme northern outcrop is 2900 feet. The continuation of the longer axis under ground at an angle of about 27° will add materially to this length. From the thickness of the bed at the last drill-hole, it seems to be safe to assume that the ore-body extends at least 600 feet farther to the north, making the total length, so far as can be judged,

about 3500 feet. The bed, then, as known at present, is 3500 feet long, 800 feet wide and 25 feet thick. These figures are not given as demonstrated, but the deposit is certainly a large one, and the writer believes that the area covered by the ore-body is rather under- than over-estimated. To say, however, that all the material from margin to margin and from foot- to hanging-wall is workable ore, is another matter altogether. In places, the ore-body in the Trotter mine is sadly interfered with by intrusive granites, and often the ore-body is cut out altogether. In the Taylor mine the only eruptive rock encountered has been the trap dike. The white limestones which form both the immediate* foot- and the hanging-wall are in many places filled with intrusive sheets of granite as well as bosses and tongues from large masses. It can hardly be expected that the great unexplored area, covering practically the whole synclinal trough, will be exempt from these troublesome disturbances. In opposition to this rather pessimistic view, it can be stated, that though all of the drill-holes encountered granite in the limestone, none was certainly struck in the ore-body, though there were some rather suspicious signs. In bore-hole No. 5, considerable garnet was brought up in the cores. At the Trotter mine, this mineral, and others of a like nature, are sure signs of the proximity of a granite dike. In bore-hole No. 6, granite lay directly on top of the ore, and both garnet and feldspar were found in the ore-body. In bore-hole No. 8, feldspar and garnet were several times encountered, from the time ore was struck at 1009 feet to 1109 feet. From 1109 feet, though, to 1144 feet, where the drill left the ore, granite was frequently encountered. This was, however, in the leaner part of the ore-body. It is more than likely, therefore, that this troublesome rock will be found intruded into the vein-matter, and will make more or less trouble for the miner.

The outcome of the recent prospecting undertaken on this property has been eminently successful, although previous attempts in the same direction were complete failures. The factor which determined the success of the later attempts was not blind luck nor obstinate perseverance, but simply a greater mastery of the elements of the problem.

After all, these conditions were comparatively simple. The south-

* The term, immediate foot-wall, is used because only 10 to 20 feet of limestone separates the ore-body from the Archæan gneiss at the outcrop. The deposit itself lies in the metamorphosed white limestone, formerly believed to be Archæan, making the franklinite-deposit to be Cambrian in age.

east direction and the angle of the dip of the front vein were known. After breaking through the trap dike, the pitch of the fold to the northeast and its angle were known. In the light of these facts alone it was folly to expect to find the disappearing ore-body in the shallow pits, which were put down. Doubtless, more money was spent in these futile and scattered efforts than in all the subsequent prospecting with the diamond drill. The one series of efforts was made in the ordinary happy-go-lucky style; the other was a carefully laid plan made after thorough study.

THE TORSIONAL THEORY OF JOINTS.

BY GEORGE F. BECKER, WASHINGTON, D. C.

(Virginia Beach Meeting, February, 1894.)

Complexity of Rock-Fractures.—The strains to which rocks have been subjected are manifestly very complex, and it is entirely safe to presume that every possible mode of deformation and rupture is exemplified. The most superficial inspection of any ordinary mountain region is sufficient to assure the observer that the rocks have been pressed, stretched, bent, buckled, twisted, and shorn. The study of torsional rupture cannot fail, therefore, to throw light on geological phenomena. The question is how areas which have been broken in this manner are to be distinguished from those which have yielded to other systems of forces.

Phenomena of Joints.—It is well known that a large part of the more homogeneous rocks and some very heterogeneous rocks are intersected by partings often called joints. These partings are frequently flat surfaces even when seen in very large exposures, but are sometimes surfaces of moderate, single, or double curvature. Joints usually occur in groups, in each of which the several partings are parallel to one another, and several such groups often intersect the same rock-mass. In such cases the different systems make large angles with one another. Nearly all students of the subject of jointing have reached the conclusion that joints are faulted surfaces, the dislocations usually being of small amplitude; and this conclusion receives abundant support from the study of thin sections under the microscope. It has thus been shown that much even of what would be regarded in hand specimens as mere slaty cleavage

consists, in reality, of innumerable microscopic faults. For this reason, Mr. Daubrée rejects the term joint, as failing to imply the existence of relative motion, and has introduced the terms diaclase and paraclase as substitutes.*

Although the word joint does not imply relative motion, it does not necessarily preclude such movement, and, as it is universally intelligible, I prefer it. In nearly all cases where joints are suitably exposed they show slickensiding. In many hundreds of joints I have found polished surfaces, although the throw of the faults was so small as scarcely to be determinable. Slickensiding is a very important genetic characteristic of joints, for wherever it prevails over any considerable portion of the parting, it is good evidence that the joint walls have not only undergone relative motion, but have remained in contact during the dislocation.

It does not follow that gaping joints should be infrequent, for a rock-mass once affected by joints will present but small resistance to any disturbing force, and such a force may readily spread the joint walls.† Indeed, it is somewhat surprising that joints are so often found closed. When a bar of metal is cut by shears, the two parts are in contact immediately after the continuity is destroyed and they slickenside one another, but under ordinary conditions they then fall apart. The comparative rarity of gaping joints is explicable in part by the presence of water. The surface tension of thin films of water leads to adhesions which seem to me of much importance. Thus, if two surfaces of rock are distant 0.01 inch from one another, and if the space is filled with water, the rock surfaces are drawn together, in consequence of the surface tension of the liquid, with a force equal to 13.5 pounds per square foot; and if the opening is only 0.001 inch wide, the pressure will be 135 pounds per square foot.‡ There can be no doubt that many jointed rock-masses, which project above the local water-level, are prevented from disorganization by this means.

Explanatory Hypotheses.—Jointing is now regarded by all inves-

* *Bull. Soc. Géol. de France*, vol. vii., 1879, p. 108.

† Prof. William King, *Trans. R. Irish Acad.*, vol. xxv., 1875, has collected much evidence to show that joints were originally close. Mr. Daubrée also draws from observations by himself and others the conclusion (*l. c.*): "A cutting or shearing force, then, was operative during the formation of joints." This is equivalent to asserting that contact existed during and immediately after rupture.

‡ The pressure is equivalent to that of a column of water, the height of which is c centimeters when the distance of the surfaces from each other is d centimeters, and c d = 162/981.4. Compare Tait's *Properties of Matter*, 1890, p. 258.

tigators as of mechanical origin. It has been referred by eminent authorities to simple tensile stresses, but observers have long protested against this explanation because such joints would gape from the start and would not be slickensided. Even if a tensile stress leading to rupture were associated with slipping, slickensides would be produced only on a few small portions of the surfaces involved. These surfaces can never be true mathematical planes, and a rupture like that just suggested might bring into contact protuberances however small, but in such cases slickensides would be confined to small portions of the surface. In heterogeneous masses, particularly in conglomerates, tensile stresses leading to rupture would produce rough and irregular partings. Even in masses so homogeneous as steel, tensile rupture takes place on very uneven surfaces.

For these reasons, jointing has been explained most satisfactorily by reference to pressure, though it does not follow that all jointing is thus brought about. Mr. Daubrée has shown by direct experiments with simple pressure that all the more usual phenomena of joints can be artificially produced, the line of pressure making an angle of about 45° with the joint planes. In this case the immediate cause of the jointing is shearing stress, and the walls of the joints are not only in juxtaposition, but are pressed together during the rupture and dislocation. The dynamic theory of this case is not difficult even when the strains are finite, and the angle between the joint surfaces is greater than 45°.*

Some geologists hesitate to accept the explanation of jointing by pressure in a given direction, on the ground that an accompanying lateral pressure of sufficient intensity would preclude rupture. It is a very important truth that, under proper conditions, rupture can not, and flow must, take place. A mass may be subjected to such confining forces that rupture is impossible by any deformation or change of volume; but this is true of tension, torsion or shearing as well as of simple pressure. Since rock fractures are abundant, it is certain that conditions permitting of rupture as well as those compelling flow, have frequently prevailed.

While Mr. Daubrée and others refer many joint-systems to the action of pressure, the famous author of the *Études Synthétiques* has also made beautiful experiments on the torsion of glass plates, which produces systems of fractures highly resembling joints in their dis-

* *Bull. Geol. Soc. Amer.*, vol. iv., 1893, p. 57.

tribution. He has consequently expressed the opinion that torsion, as well as pressure, has led to joint-systems.*

Character of Torsion-Fractures.—Mr. Daubrée has minutely described the phenomena of the rupture by torsion of glass plates, these being mounted on paper to avoid the scattering of the fragments. Torsion, as he describes it, produces two main sets of fractures, approximately at right angles to one another, and usually at nearly 45° to the axis of torsion. The fissures cut the broad surfaces in lines which are nearly straight, and the surfaces of fracture are approximately warped surfaces, the inclination to the vertical in some cases reaching 50° on each side. Some few fissures reach from edge to edge of the plate, while many do not entirely cross it, and sometimes single fissures neither reach the edge nor any other crack, and are thus isolated in the mass. The fissures which reach the edge of the plate cut the narrow face at angles with the line measuring the thickness, which varies greatly—from 20° or less up to 50° or thereabout. Besides the more regular systems of parallel fissures, fan-shaped groups are not uncommon. In the neighborhood of the finer groups of fissures the glass loses its optical and thermal isotropy.

By the kindness of Prof. T. C. Mendenhall I have been enabled to make experiments similar to those of Mr. Daubrée in an apparatus which permits of gradually straining the plates and of measuring the angle of torsion. Besides common window-glass, I employed glass ground on one side, for the purpose of making sure that surface-tension played no part in the result. Well-polished plate-glass was also used, sometimes in simple strips and sometimes cut in the shape of a cross-section of an I-beam, in order to confine the initial rupture to points remote from the jaws. In a large part of the experiments the cut edges were ground with emery, so that imperfections of the edges might not influence the result. Many different dimensions were selected, and the plates varied from nearly square rods to sheets wider than they were long. It was found best to substitute a thin fabric, known as "cheese-cloth," for the paper on which Mr. Daubrée mounted his plates. The paper, in drying, exerts a considerable tension, and the specimens mounted on cloth, besides being more easily handled after fracture, show smaller tendency to fan-fracture.

* Mr. W. O. Crosby also adopts the torsional hypothesis, with the modification that he supposes the final rupture to be determined by shock.—*Am. Geologist.*, vol. xii., 1893, p. 368.

All of Mr. Daubrée's descriptions are illustrated by my specimens,* and I have but few observations to add. It scarcely requires mention that the curvature of the surfaces of rupture is ordinarily such as to permit of the free torsion of the broken plate. Sometimes, however, short cracks extending from the main fissure to the edges of the plate are so warped as to obstruct torsion. Hence, when the axis of torsion is not in the vertical, all the principal faults produced in the experiments are reversed, the hanging-wall rising relatively to the foot.

When the breadth of a glass plate is large relatively to its thickness, the surfaces of rupture are, as Mr. Daubrée remarks, nearly coincident with warped surfaces; but when the breadth is only a few times the thickness, the departure of the surface from a warped surface is well marked. In such cases it is interesting to note that *one* cropping of the fissure is usually wonderfully straight while that on the opposite side is an inflected curve. From the point of view of pure dynamics the exact shape of these surfaces would be interesting, but I have reason to believe that the geometrical character depends essentially upon that of the cross-section of the twisted bar; and since it will seldom or never be practicable to determine the shape of a twisted rock-mass, there seems no geological interest in ascertaining the precise form of the surface of rupture. It is probable that the forms are all closely related to the warped surface.

Among the excessively fine cracks in the glass which are mentioned by Mr. Daubrée, there are some which are superficial. These are usually near the middle of the plate; they are very straight, and invariably parallel to the straighter edges of the ruptures on the same surface, and do not seem to penetrate quite to the center of the plate. They are clearly incipient fractures, and the observations indicate that rupture begins on one of the broad faces in a very straight line, the surface twisting as it spreads through the plate in such a manner as best to relieve the torsional stress. Even on rather thin plates it is visible that one cropping of each well-formed crack is straighter than the cropping on the opposite face, and that the straight cropping has a definite relation to the direction of twist. When torsion is so applied as to tend to twist the bar into a right-handed screw, the straight lines are inclined like the thread of a left-handed screw, and *vice versa*. Thus the straight croppings on one

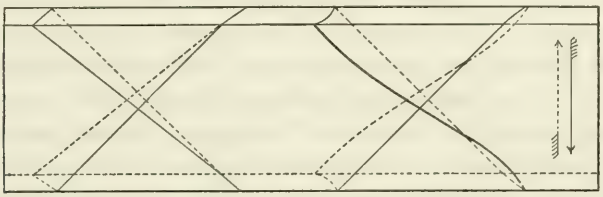
* I have not tested the anisotropy in the neighborhood of the terminations of cracks not crossing the plates. As the mass at such points is in a permanent state of strain, anisotropy is to be expected.

surface of a plate are at right angles to the straight lines on the other side of the plate.

The angles at which the cracks cross the narrow edge-surfaces of the plate vary considerably, as Mr. Daubrée has observed. I cannot find that the inclination of these lines varies in any regular manner with the width of the plate. The average angle which the cracks on these narrow surfaces make with the long edges seems to be about $63\frac{1}{2}°$, or the angle whose tangent is 2. These croppings are in fact curved lines in almost all cases, and the curvature is such that the acute-angled fragments bounded by the edge of the plate and the ruptures are somewhat grooved.

The following diagram shows two warped surfaces intersecting a bar supposed 4 times as wide as it is thick and a sketch of two of the more complex surfaces, referred to above, generalized from a considerable number of cases.*

FIG. 1.



Diagram, illustrating intersection of surfaces of rupture with a bar.

Rupture by Shear.—In a pure (or irrotational) shear the resultant load on any section passing through the center of the strain ellipsoid is the same. On the sections perpendicular respectively to the greatest and least axes the load is wholly normal. On the sections of unchanged area (which stand at 45° to the greatest and least axes when the strain is very small) the load is purely tangential.† In a homogeneous mass thus strained, rupture may conceivably occur by the tension along the greatest axis, or by pressure along the smallest axis, or by tangential motion at 45° to the axes. A mass ruptured by tension sometimes breaks perpendicularly to the direction of tension with a smooth surface, and sometimes, as in the case of mild steel, with a rough or "cupped" surface, the facets of which are in the

* If the axis of the bar is the x axis, y the distance from this axis parallel to the broad surface, z the distance parallel to the narrow surface, b the breadth, c the thickness, then the following equation represents both the warped surfaces.

$$x = \frac{yz}{b} \pm \frac{y}{b}\left(b + \frac{c}{2}\right).$$

† *Amer. Jour. Science*, 3d series, vol. xlvi., 1893, p. 339.

direction of the planes of unchanged area. A mass broken by uniformly distributed pressure breaks in the direction of the planes of unchanged area. A mass ruptured by tangential stress also breaks on these planes. Thus a homogeneous mass subjected to a shear can break in only two ways, viz.: perpendicular to the greatest axis or at 45° to this axis. In the present state of knowledge concerning the constitution of matter, experiment only can determine which rupture will occur. The experiments on glass plates in connection with the theory of torsion, due to the genius of Barré de Saint-Venant, render a decision very easy for this substance.

Points From the Theory of Torsion.—When a rectangular bar of an elastic, homogeneous solid is twisted through a small angle the following conditions are fulfilled. The lateral surfaces of the bar become warped; the length of every edge remaining substantially unchanged, while diagonal lines are elongated or contracted, but the volume of the mass is neither increased nor diminished by a sensible amount. At the surfaces of the bar the resultant strains lie in these surfaces. The directions of maximum extension and compression are at 45° to the axis of torsion, and at right angles to each other. If the rotation of the torsion-couple is positive (opposite to that of the hands of a watch) the directions of greatest extension on all the sides are inclined in the same sense as the thread of a right-handed screw. The strain at any point in the mass is a pure or irrotational shear. The points of maximum strain are on the surface and nearest the axis; in other words, the danger line is the median line of the broad surface; and on the narrow surface of the prism the strain is also greater half way between its long edges than elsewhere.

Most of these conclusions can be exemplified in a very easy and striking manner with a rectangular bar of rubber, such as is in common use for erasing pencil marks. The surface of such a mass, cross-hatched with lines parallel to the edges, shows when the bar is twisted, that deformation is greatest nearest the axis and insignificant near the edges of the prism. Lines at 45° to the edges are greatly extended or shortened by twist, and a very small circle is converted into an ellipse with axes at 45° to the edges.

It evidently follows that rupture should begin at the middle of the broad surface. If rupture takes place by slipping on the planes of no distortion of the shear ellipsoid, the incipient cracks in the mass will be parallel to or perpendicular to the axis of torsion. If the mass yields to the tensile component of the shearing stress these cracks will coincide in direction with the lines of greatest linear

compression and stand at 45° to the axis of torsion. This is precisely what occurs in the experiment on glass. On the other hand the theory shows that the resultant stresses within the body of the plate are not in general parallel to the surfaces, and it is therefore to be expected that curved surfaces of rupture should ensue.

Character of Torsional Rupture.—It now appears certain that the experiments on the torsion of glass are equivalent to the application of a system of tensions peculiarly distributed, and that the fissures produced by torsion in any mass physically resembling glass, will exhibit the peculiarities of tensional fractures together with some marked characteristics of their own. The fissures will gape from the start, excepting at certain points, which alone will be slickensided. The surfaces will be rough, excepting when the mass is uncommonly homogeneous. The surfaces will also show double curvature, which will be very strongly marked unless the mass of rock affected by torsion is immense. If the axis of torsion is vertical the particles originally in contact will separate along horizontal lines, and there will be no faults as these are usually estimated. In all other cases every surface will be faulted, and every master fault will be reversed. The net-work of fissures will show marked regularity, provided that the exposure is an approximately flat surface nearly parallel to the axis of torsion, but any section forming an angle with the axis of torsion will expose a very complex arrangement of curved partings. If rocks break under torsion at all as glass does, a pronounced characteristic will be the frequency of fissures completely "cut off" by others, these being much more numerous than "master" joints.

Conclusion as to Jointing.—It appears from the foregoing that though torsional rupture may be of frequent occurrence in disturbed regions, the systems of dislocations familiar under the designation of joints cannot aptly be ascribed to pure torsion, while direct pressure will produce the phenomena called jointing. But the forces acting upon a rock-mass are in general very far from simple; indeed it may be assumed that every variety of stress exists in a strained rock-mass. Nevertheless a general idea of the nature of rupture even under such conditions may readily be reached. The strain ellipsoid at any point must either be torn apart by tension or cut across by shearing, and in all ordinary cases it is easy to distinguish these modes of rupture. In a mass subjected to a complex stress the orientation of the strain ellipsoid will vary from point to point, and so will the direction of the rupture; but smooth slickensided surfaces, though curved, will still be due to pressures acting at

approximately 45° to the local direction of the surfaces when the material is a hard and homogeneous one; and tension cracks will form at right angles to the local direction of the effective tension. Thus in typically jointed areas, curvature of the joint planes indicates that the direction of the effective pressure has varied from point to point, as it does for example in the compressed portion of a flexed bar. The existence of curved joint-planes is consequently by no means inconsistent with the ascription of jointing to pressure, while it does indicate that the entire system of forces, operative and inoperative, which has been brought to bear on the mass is complex. The action of pressure on heterogeneous materials is much more regular than the action of tension; and it might be possible in some cases from the study of curved joint surfaces to infer the nature of the complete stress system.

THE SILVER-MINES OF LAKE VALLEY, NEW MEXICO.

BY ELLIS CLARK, PHŒNIX, ARIZONA.

(Virginia Beach Meeting, February, 1894.)

THESE mines were discovered by George W. Lufkin in August, 1878, and were worked almost continuously until August, 1893, a period of fifteen years.

The ore-deposits in this locality lie close to the surface, and it is strange that the Spaniards, passing and repassing for nearly three centuries, did not discover them, particularly as they are marked in several places by large outcrops of black flint and iron, which would lead even a casual observer in a mineral country to suspect the proximity of ore. The mines are situated about six miles from the old Santa Fé trail, and fifteen miles from the Rio Grande.

The geology is such that a skilled geologist passing over the ground would probably suspect, from the peculiar arrangement of the formations, the occurrence of ore. The formations in the district, eruptive and sedimentary, are nine in number; and of these eight are to be found within one thousand feet of the Grande workings.

Topography.

The topography of the Lake Valley district shows its dependence on the surface and underground geology, in a degree more marked

than the writer has elsewhere observed. (Plate I. exhibits this relation.) The most prominent topographical features are two rhyolite peaks, Porphyry Hill, situated one mile southwest of the mines, and rising 1000 feet above the plain, and the so-called Rhyolite Range, three miles to the northwest, rising 1500 feet above the general level.

Starting from the base of the first-mentioned peak, the Silurian limestone-quartzites form a gradually descending ridge which expires near the lake that gives its name to the district. This ridge is bounded by the Barrenda valley on the northwest, and by narrow shale "draws" on the southeast.

Across these shale valleys, the Lower Carboniferous Blue Limestone gives rise to Apache Hill, which at a height of 700 feet above the plain overlooks the mines.

Finally Monument Peak, an isolated knoll of andesite with a height of 500 feet, bounds the district on the east.

Geology.

The oldest formation in the district appears to be the Silurian limestone-quartzite, which, taking its rise along a fault at the base of Porphyry Hill, extends for a distance of several miles to the northeast.

The width of this Silurian limestone-quartzite on the surface is one mile at the center of the district, diminishing to one-half mile at the northern extremity.

The actual thickness of this formation cannot be determined, since its base is uncovered nowhere in the district; but the exposed thickness, from a point in the valley near Crystal Springs to the highest point of the quartzite, is 600 feet.

Specimens of *Favistella stellata*, and the *Favosites venustus* have been found in the quartzite, which, with the *Monocraterion lesleyi* found in the limestone swell, in the northern part of the Barrenda valley, appear to be sufficient to establish the age of the formation as Silurian.

The black and green shale, the next formation in point of age, lies apparently uncomformably on the quartzite; it takes its rise close to the Grande workings, forming a valley from 500 to 1000 feet wide, which, broken at one point only, constitutes the natural outlet to the northeast, and has been utilized for the stage-road running to Hillsboro and Kingston.

This shale overlies, and forms for three miles a contact with, the

quartzite. It has been prospected in numerous places by shafts to depths of 50 and 100 feet, some of the shafts being within a few feet of the quartzite outcrop; but in one place only has its thickness been determined, and the underlying quartzite reached. This is at a point 1200 feet northwest of the Grande workings, where a prospecting-shaft was sunk through 130 feet of shale to a red limestone, 12 feet thick, under which was the quartzite. This shaft was sunk with the object of prospecting the so-called "second contact." No ore was found; but at the very base of the shale, nodules of about the size and shape of a potato, formed of iron pyrites, were encountered.

The point in the shale at which this shaft was sunk is one that has experienced some erosion; and the probable thickness of the shale is 160 feet. No fossils have been found in it, and its dip is difficult to determine, but may be said in a general way to correspond with the general dip of the district, which is 15° to the southeast.

Succeeding the shale is a limestone called the "Nodular limestone," from its structure, which consists of nodules of limestone up to 2 or 3 inches in diameter, surrounded by a weak limestone cement. The nodular limestone is 48 feet thick, as shown on a cliff on the northwest escarpment of Apache Hill. It appears first near the Grande workings, and follows Apache Hill its entire length, spreading out in a large patch on the northeast slope, where it has been exposed by the erosion of the Blue limestone. The Nodular limestone then disappears, but shows again on the southeast side of the shale valley leading to the lake. Its continuity is broken at one point by a small andesite outburst, but it reappears and continues for a half mile, until it is faulted and disappears near the cottonwoods.

The Nodular limestone is followed by a thickness of 24 feet of Blue limestone, which is locally known as the "Footwall lime," from the fact of its underlying the ore-deposit. The Blue limestone is very pure, containing from 3 to 5 per cent. of silica and 45 to 50 per cent. of lime. It is occasionally broken by faults which have been filled in with a ferruginous flint, and are locally known as flint "dikes," but which do not appear to have had any special influence on the segregation of ore. Nodules of flint are occasionally found in connection with some of the layers of this limestone.

Overlying conformably the Blue limestone is a shaly limestone, generally yellowish-white, but containing strata of red, purple and brown, which, from the fact of its being composed to a great extent

PLATE II.
MAP OF
THIRTY STOPE, BRIDAL CHAMBER & CAROLINA WORKINGS,
Office Shaft No. 2
SHOWING SECTION LINES
100 Contours 10 Feet Vertical Distance apart.
Scale of Feet. December, 1892.
Formation in Drifts & Stopes Shown by Lines & Crosses as Follows:
Drifts, Stopes or Inclines on Contact, or in Blue Lines, with Ore.
Drifts or Inclines in Rhyolite.
Drifts or Inclines in Blue Lines.
Outcrop of Ore Leading Porphyrite
Drifts or Inclines in Nodular Limestone
Outcrop of Rhyolite.
Shaft
Winze
Am. Bank Note Co. N.Y.
Daly Shaft
47'
Office Shaft
Workings
Storehouse
No.102
31'
No.101
42'6"
Robert's Shaft
89'6"
No.11
92'
Carolina Shaft
30 Shaft
No.10
21'6"
No.103
44'6"
Bolter Shaft
19'6"
FIVE
WEST
TWENTY-
30 Air Shaft
Roadside Shaft
Henry Shaft
25 Cut-Shaft
14 Shaft Workings
5400
5370
5330
CUT
OPEN
STOPE
INCLINE
UPPER LEVEL
THIRTY
GOWAN
MILDRED
CHAMBERS
BRIDAL

of fossils, principally crinoids, has been called the Crinoidal limestone. Other fossils abound; spirifers being numerous, and trilobites rare. The Crinoidal limestone is very siliceous, containing 35 per cent. of silica and 30 per cent. of lime. It also contains nodules of flint of a somewhat different grain and texture from those found in the Blue limestone, and capable of being distinguished from the latter by an experienced eye.

This Crinoidal limestone, from the fact of its overlying the minor ore-deposits, and to some extent the greater ones, is locally known as the "Hanging-Wall lime." It forms the summit and the lower part of the eastern flank of Apache Hill, continuing like a gigantic finger until it is eroded in the neighborhood of Phillips's well. It then reappears near the small andesite outbreak of the shale valley, with a width of a thousand feet and a length of three-fourths of a mile, beyond which it is eroded, leaving the Blue limestone exposed.

To the southeast of the Grande and Bella workings, the various formations flatten out and are in some places nearly level, and at others have a slight dip to the southeast, disappearing under a flow of the Monument Peak andesite. In two places, close to the graveyard, the thin coating of andesite has been eroded, exposing small patches of Crinoidal lime. At other points, near the town, prospect-shafts of moderate depth (less than 25 feet) have been sunk through the andesite and andesitic breccia to the underlying Crinoidal limestone. It is probable that the sedimentary formations are entirely cut off to the eastward by the Monument Peak eruptive fissure.

The total thickness of the sedimentary formations may be approximated as follows:

Silurian.	Feet.
Quartzite and limestone, ...	600
Lower Carboniferous.	**Feet.**
Black shale, 100 feet; green shale, 60 feet, ...	160
Nodular limestone, ...	48
Blue limestone, ...	24
Crinoidal limestone, 125 feet: upper, 25 feet; topmost, 25 feet,	175
	1007

Eruptive Formations.

The eruptive formations of the district are four in number, hornblende-andesite, rhyolite, obsidian and porphyrite. All of these, with the exception of the obsidian, occur within 500 feet of the mine-workings.

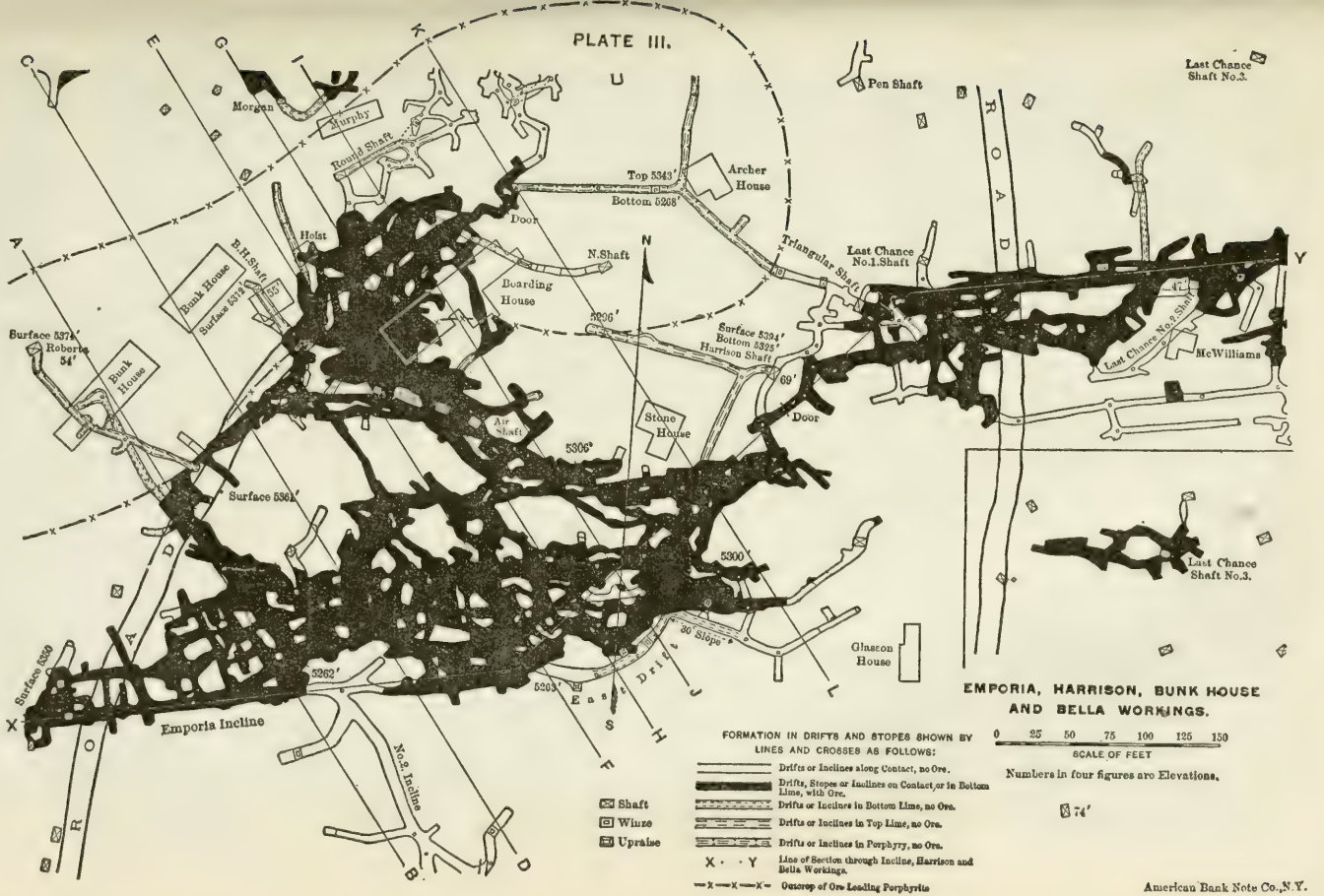


PLATE III.
Morgan
Murphy
Round Shaft
Top 5343'
Bottom 5208'
Archer House
Pen Shaft
Last Chance Shaft No.3.
Door
Hoist
N.Shaft
Triangular Shaft
Last Chance No.1 Shaft
Bunk House
Surface 5373'
R.H.Shaft
Boarding House
5296'
Surface 5394'
Bottom 5325'
Harrison Shaft
Last Chance No.2 Shaft
McWilliams
Surface 5374'
Roberts 54'
Bunk House
69'
Door
Air Shaft
Stone House
5306'
Surface 5361'
5300'
Last Chance Shaft No.3.
30° Slope
Glasson House
Surface 5259
5262'
5269'
East Drift
Emporia Incline
No.2 Incline
Shaft
Winze
Upraise
FORMATION IN DRIFTS AND STOPES SHOWN BY LINES AND CROSSES AS FOLLOWS:
Drifts or Inclines along Contact, no Ore.
Drifts, Stopes or Inclines on Contact, or in Bottom Lime, with Ore.
Drifts or Inclines in Bottom Lime, no Ore.
Drifts or Inclines in Top Lime, no Ore.
Drifts or Inclines in Porphyry, no Ore.
Line of Section through Incline, Harrison and Bella Workings.
Outcrop of Ore-Loading Porphyrite
EMPORIA, HARRISON, BUNK HOUSE AND BELLA WORKINGS.
0 25 50 75 100 125 150
SCALE OF FEET
Numbers in four figures are Elevations.
5374'
American Bank Note Co., N.Y.

The rhyolite is the oldest of these formations, and by far the most prominent, though it covers but 2 of the 16 square miles shown on the map. The rhyolites appear to be two distinct flows. The first, which composes Porphyry Hill, southwest of the ore-bearing ground, shows a gray rock with a feldspathic matrix, the quartz crystals being small and mica rare. It is hard on the surface, but quite soft where it has been encountered underground, in the workings of the Grande. This rhyolite has accompanied a great fault almost in a straight line, with a northwest and southeast strike. It has cut all the sedimentary formations, beginning at the Crinoidal limestone and continuing down to the Silurian quartzite-limestone.

This flow of rhyolite has a distinct and very much distorted bedding; but the dip of the bedding is completely disguised, and specimens from almost any point on Porphyry Hill show twistings and convolutions like a gnarled oak-root.

The contact between the rhyolite and the other formations, eruptive and sedimentary, is strongly marked where not obscured by débris. Beginning near the Grande workings, the contact between it and the andesite has a southerly strike for three-fourths of a mile. It then turns abruptly to the west, carrying that direction for a mile, and showing an obsidian dike on the southwest side. It then turns a right angle to the north to a point on the hill above Crystal Springs, and thence continues to the southeast, to a point near the Grande, faulting in its way the Silurian quartzites and the Lower Carboniferous shales and limestones.

The second flow of rhyolite shows at four points in the district, namely, the two hills to the northwest and southeast of Crystal Springs, and the two peaks in the extreme northern part of the map known as Rhyolite Range. In these four hills the mineralogical features of the rhyolite are similar, the rock being gray-white, and consisting of a feldspathic matrix with prominent quartz crystals and considerable mica.

The flows of this rhyolite have been very thin, sometimes only as thick as a sheet of cardboard. They have occasionally distinct colors, varying shades of gray, brown, and, sometimes, brick-red; and the bedding is so distinct and so regular as to give, in places where a harder layer has been left undecomposed between two soft ones, the impression of a stratified and sedimentary rock.

The dip in the four hills is regular, identical, and about 15° to the east. Except along the northeastern flank of Porphyry Hill, where the sedimentary formations are cut off, and on the southwestern flank,

where a small obsidian dike occurs, both classes of rhyolite are bounded on all sides by the andesite.

The hornblende-andesite is the predominating rock of the district, covering about three-fourths of the area that has been mapped. Whether this large amount of andesite is the result of a single flow, or has come from a number of isolated fissures and craters, is difficult to determine from the present topography; but the writer inclines to the belief that it has come nearly simultaneously from a number of points which are now to be recognized as conical peaks within the area mapped.

On the western side of the district, the erosion has been such that the andesite hills reach to the 5800-foot contour, or 800 feet above the plain; on the eastern side, Monument Peak, which bears rather distinct indications of being the root of a volcano, rises abruptly to the height of 5500 feet, or 500 feet above the plain.

At Monument Peak, the main fissure has a north and south direction, determining the outlines of the hill, and a later crater has been developed a few hundred feet to the eastward, leaving a conical knoll with a knob of nearly perpendicular sides, 100 feet in height, which forms the most distinct landmark in the district when approached from the north, south or east.

The andesite that surrounds the flanks of Porphyry Hill appears to have come from two series of fissures or isolated openings, grouped in a concentric manner, at distances of a mile and a mile and a half respectively from the highest point of Porphyry Hill as a center. Continuing this analogy, Monument Peak would belong to the fissure represented by the second circle. Two diminutive outbreaks of andesite, one at the summit and the other on the southeast side of Apache Hill, at a distance of a mile from the summit of Porphyry Hill, would appear to belong to the fissure represented by the first circle.

The small isolated knoll of andesite on the side of the shale valley, in the northeast corner of the area surveyed, does not appear to have any marked connection with any of the other outbreaks. It has broken through the Nodular and upheaved the Blue limestone, which shows in a disrupted stratification on its flanks. Its action was purely local, as both the Nodular and the Blue limestone have their regular dip and strike within a hundred feet of the outburst.

The remainder of the andesite appears to constitute a sheet of varying thickness, eroded to a few feet only in places, notably southeast of the town of Lake Valley, and entirely gone at two points near the graveyard, where it has left the Crinoidal limestone exposed,

and at a point 1500 feet east of Phillips's well, where the Blue limestone shows. The andesite of the entire district is nearly identical, consisting of a feldspathic matrix and prominent porphyritic crystals of hornblende. Generally the color is reddish from oxides of iron, but occasionally the hornblende predominates over the feldspathic matrix, giving an olive-green color to the rock.

The obsidian is a very subordinate formation, and occurs in quantity only on the southwest flank of Porphyry Hill, in a curved dike half a mile long and a few hundred feet wide.

The porphyrite is, economically, the most important formation in the series, since all of the ore-deposits, with the exception of a small one on the Apache, have been found in close proximity to it, and the two most important ones, the Bridal Chamber, on the Grande, and the Bunkhouse, on the Bella, were directly under it and in contact with it.

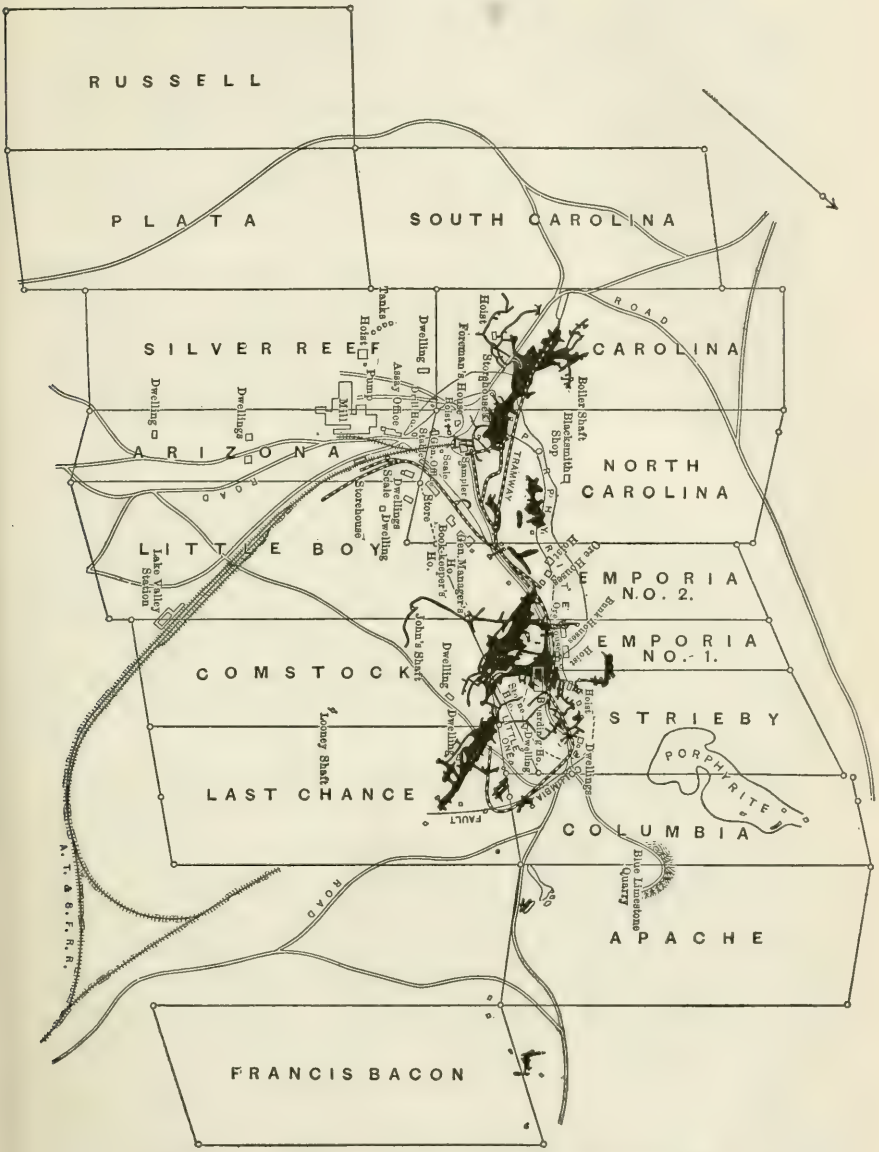
It occurs in two troughs, the first excavated in the Blue limestone of Apache Hill, and the second (the one along which the ore occurs) between the Blue and the Crinoidal limestone. The latter has a length of 1600 feet, beginning on the Bella with a width of 300 feet, and a southwest direction, parallel to the strike of the sedimentary formations, and gradually narrowing, with occasional slight swells, to a width of 50 feet, which it maintains to the Grande workings, where it widens to nearly 100 feet, and makes a right-angle turn, overlying the Bridal Chamber and paralleling the course of Porphyry Hill to a point near the mill, where it narrows to a knife-edge and disappears.

This porphyrite carries in places from a trace to one ounce of silver per ton. It generally shows as a soft, clayey, gray, canary, chrome-yellow, or white material, sometimes having a curly, birds-eye structure. Its metamorphic effect on the limestones in contact with it has been very slight, their color being changed from blue or gray to white for a few inches only. Rounded bowlders of limestone in a porphyritic cement are frequently found near its base.

That this porphyrite is an overflow, and not a dike cutting through the sedimentary formations, though for a long time doubted, may now be considered as proved. Every attempt to penetrate through it has been successful, and has shown it to be resting on either the Blue or the Crinoidal limestone.

The prospect-holes by which this has been proved are: three shafts in the upper trough, where the porphyrite was found to have an average thickness of 30 feet, resting on blue limestone; a winze in

PLATE IV.



THE SILVER MINING CO., OF LAKE VALLEY, N. M.

November, 15, 1893.

0 200 400 600

Scale of Feet

the Strieby, which reached the Blue limestone at a depth of 125 feet from the surface; an upraise from the Emporia Incline, which cut through the Blue limestone into the porphyrite above; some shallow shafts, less than 25 feet deep, which reached the Blue limestone along the narrow part of the overflow; the Bridal Chamber shaft, which passed through 20 feet of wash and porphyrite and 20 feet of ore, and ended in limestone; and a shaft to the south of the Grande, where the overflow of porphyrite is 50 feet wide and 28 feet deep, resting on Crinoidal limestone.

The contact of the Crinoidal and Blue limestones has yielded practically all the ore that has been taken from the property; and at two points—where the porphyrite has overflowed this contact, making a junction of the two limestones with the porphyrite—have occurred the great bonanzas of the Bridal Chamber and the Bunkhouse.

Mineralogy.

The mineralogical characters of the Lake Valley ores vary with the different openings. The Twenty-five Cut, Thirty Stope and Bridal Chamber have yielded siliceous ores. The Emporia Incline has yielded principally neutral ores, and the Bunkhouse, Columbia and Apache have yielded ores more or less basic in character.

Each of these openings, besides its main yield, which may be considered in carload-lots, has yielded smaller quantities of ore of very different type.

The ores from Thirty Stope consist principally of gray, brown, chocolate, pearl and green flint. The green flint has generally yielded a grade of ore higher in silver than the other colors. The flint has been much broken and crushed, the fragments subsequently having been cemented by veins of transparent quartz. The manganese and iron contents are small, the silica contents large. An average of the ore from this working for the past few years would be 65 per cent. silica, 6 per cent. iron and 12 per cent. manganese, with 20 ounces of silver per ton. Fine specimens of pyrolusite showing crystalline structure are found in these workings.

Much of the Bridal Chamber ore consisted of cerargyrite; and the little ore that has been mined from the chamber in recent years has been similar to the Thirty Stope ore, but slightly less siliceous.

The Emporia Incline ore, when considered in carload-lots, is a neutral ore, consisting of 30 per cent. silica, 12 per cent. iron and 18 per cent. manganese, the remainder being limestone. It is generally brownish-black, with a tendency to brown, and frequently carries

from 1 to 5 per cent. of lead in the form of galena. Its contents in silver vary from 30 to 50 ounces to the ton.

Galena is one of the accessory minerals in this working, sometimes occurring massive and compact, with a fine-grained, crystalline structure, and sometimes in a pulverulent mass, known locally as "gray metal," which is easily powdered under the pressure of the fingers, and in mining is carefully scraped and gadded out of the containing cavities, and allowed to fall on canvas lying on the rock-floor. Blasting is not practiced on this variety of ore, on account of its value and the danger of scattering and losing it. It contains from 200 to 500 ounces of silver per ton and 50 to 60 per cent. of lead.

The Bunkhouse workings have yielded several distinct varieties of ore from different parts of the mine. The ore from the central portion of the body, taken out shortly after its discovery, varied in silver from 200 to 500 ounces per ton and was basic, containing a considerable proportion of cerargyrite. Subsequent workings developed large bodies of chocolate-colored, manganiferous ore, with an average composition of 8 per cent. silica, 12 per cent. iron and 24 per cent. manganese, the remainder consisting of limestone and gypsum. The silver-contents were 20 to 30 ounces per ton. Overlying this ore were considerable bodies of what became known as "fluxing-ore," averaging 35 per cent. excess of bases over silica, and carrying 5 per cent. silica, 10 per cent. iron and 30 per cent. manganese, the remainder consisting of limestone. The silver-contents were always low, increasing and diminishing with the silica-contents and averaging 5 ounces to the ton. The color of the fluxing-ore was black.

Underlying the previously-mentioned fluxing-ore, at the base of the ore-series, well into the Blue limestone, was a deposit of mangano-ferruginous ore, which was nearly neutral, with an average composition of 30 per cent. silica, 12 per cent. iron and 18 per cent. manganese. Its silver-contents were low, seldom above 8 ounces per ton, and the ore was commercially useless. In appearance it is not unlike the Incline ore, having a blackish-brown color, inclining to black.

Nomenclature.

The ore-formation is locally known as a "blanket;" but this term is correct for a portion of the property only, and there but partially. For convenience, the names of the three original companies have been taken, and the different parts of the property will be described under the heads of the Grande, the Bella and the Apache.

1. The Grande workings include Twenty-Five Cut, Thirty Stope, the Bridal Chamber and the Carolina workings, the former three being on what may be loosely termed the outcrop, and the latter on the dip. Only the first three have yielded ore in quantity.

2. The Bella workings include the Emporia Incline, the Harrison, the Bella Chute, the Bunkhouse and the Strieby, all of which have yielded ore in quantity.

3. The Apache workings include those northeast of the Bella, to the extremity of the ore-bearing ground, and have yielded considerable iron flux and a comparatively small amount of commercial ore.

The approximate yields from the different workings have been:

	Oz. of silver.
Bridal Chamber,	2,500,000
Thirty Stope,	1,000,000
Emporia Incline,	200,000
Bunkhouse,	300,000
Bella Chute,	500,000
Twenty-Five Cut,	200,000
Apache and scattering,	300,000
Total,	5,000,000

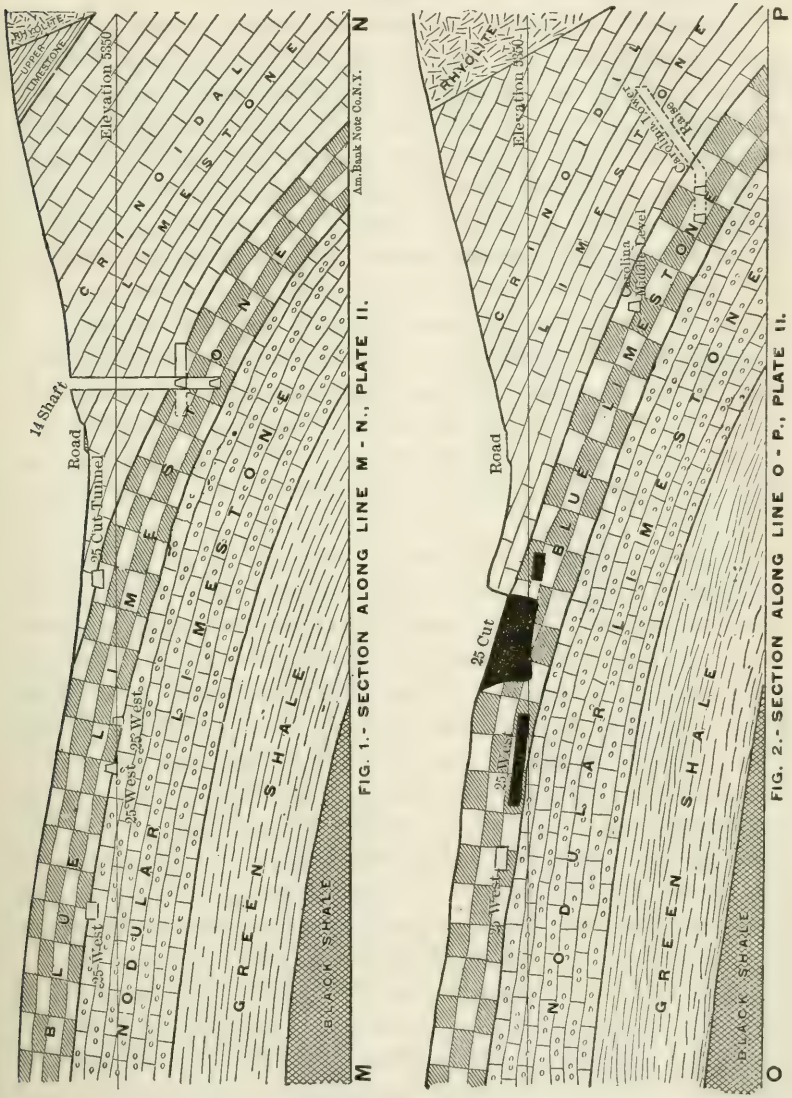
1. *The Grande Workings.*

Plate II. gives a plan of the workings of the Grande, and the eruptive and sedimentary formations are shown in the sections MN to YZ (Figs. 1 to 7).

Section MN, Fig. 1, which is drawn through Twenty-Five Cut tunnel and No. 14 shaft, is, so far as is known, just beyond the western extremity of the ore-bearing ground. The workings in Twenty-Five West are considerably below the ore-bearing contact of the Crinoidal and Blue limestones. The Twenty-Five Cut tunnel is driven 62 feet along this contact, the Crinoidal showing in the roof and the Blue limestone in the floor. The contact is here what is locally known as "frozen," and contains neither iron nor ore. One hundred feet from Twenty-Five Cut tunnel, No. 14 shaft was sunk on a diamond-drill-hole, the bore of which may still be seen at the corner of the shaft. At 62½ feet the contact was struck, and ferruginous flint was found in quantity, with an occasional piece rich enough in silver for an assay, but no commercial ore. At a distance of 175 feet from No. 14 shaft, the rhyolite appears, cutting off all the sedimentary formations.

Section OP, Fig. 2, which is drawn through the middle of Twen-

ty-Five Cut, shows, first, workings in Blue limestone, which failed to find ore; then a stope in Twenty-Five West, which yielded a quantity of very rich ore. A fissure, two to three feet wide and as



long as the stope, shows in the roof, while the floor is in unfissured Blue limestone. This fissure in the roof, though by no means a proof, gives a hint as to the direction in which the origin of the ore is to be sought.

Twenty-Five Cut, next on the section, shows an opening 50 feet wide and 25 feet deep, in which the ore was found in a solid body. It was not of specially high grade, and, from the traces left on the walls, must have been very siliceous.

To the south of Twenty-Five Cut the Crinoidal limestone appears, and a little ore has been taken out of the Blue limestone immediately under the contact, at which point the ore-chute is cut off, and is not again met with on this section. One hundred and thirty-seven feet along the dip the section cuts the Carolina middle level, which, at this point, is in Blue limestone, a short distance below the contact; and 75 feet further the Carolina lower level is crossed, still in the Blue limestone. An upraise from this point was driven across the contact into the Crinoidal limestone without finding ore, stopping about 45 feet from the rhyolite which it was intended to reach.

Section QR, Fig. 3, begins on the Boiler-shaft fault, which has a throw of 65 feet. The shaft was sunk a number of years ago and reached a depth of 103 feet, having passed through the mud filling, incident to the fault, and ending in the Nodular, which, at this point, was slightly mineralized with pyrites and a trifling amount of galena.

A lease was taken in 1892 on this shaft, and it was sunk 72 feet deeper, penetrating the green shales to a depth of 37 feet. As might have been expected, nothing was found at a depth of 162 feet below the ore-bearing contact. One hundred and twenty-five feet from the Boiler shaft, the workings of Twenty-Five West showed ore, and close by we find Thirty Stope, 87 feet long and 8 to 12 feet high, of solid ore. This stope occupies a space in the Blue limestone, and has the Crinoidal limestone for a roof over its entire extent. The section then cuts the upper level where, on the contact, a large body of siliceous iron was found, but little of which was of sufficient richness in silver for shipment as ore. The middle level, as well as the two branches of the lower level, are on the contact; the rhyolite cutting off the limestone at a further distance of 20 feet.

Section ST, Fig 4, shows the ore first at Thirty Stope, where it outcropped in places, or was covered with a thin shell of Crinoidal limestone. The stope is 125 feet long on the line of section, and 12 to 37 feet thick, carrying solid but siliceous ore. The Crinoidal limestone overlies Thirty Stope on this section for its entire length, dipping at a much steeper angle as it proceeds to the southward. Fifty feet from Thirty shaft, the ore-chute is cut off abruptly, and although an incline was driven along the contact of the two lime-

stones to a point near the Carolina shaft, where the rhyolite was met, and the middle level was driven under Thirty Stope for a distance of one-half its length, the ore appears to have been cut off on all sides, as nothing more of it was found.

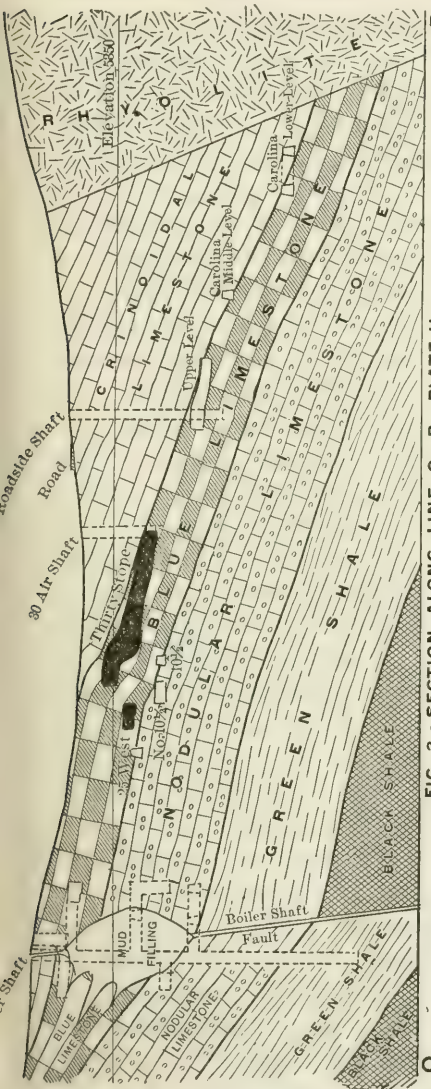


FIG. 3.- SECTION ALONG LINE Q - R., PLATE II.

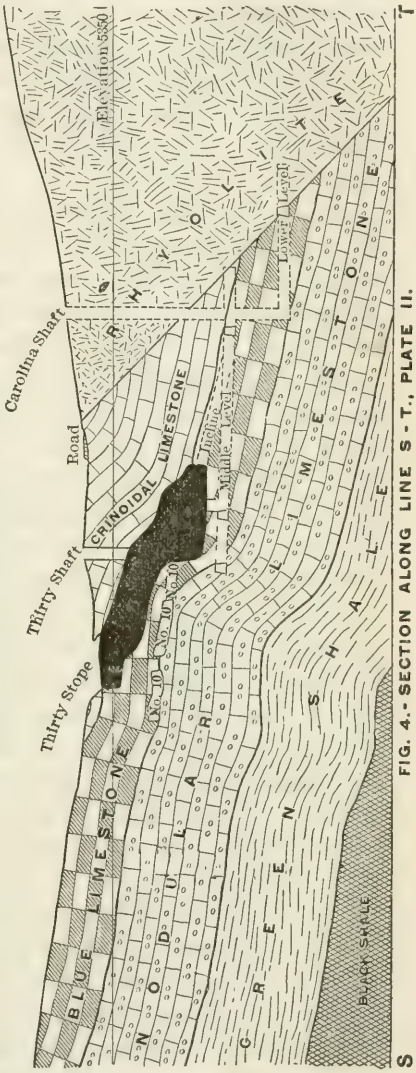


FIG. 4.- SECTION ALONG LINE S - T., PLATE II.

The Carolina shaft is 125 feet from Thirty shaft, and 118 feet deep, the rhyolite occupying the first 62 feet, and the remaining 56 feet being Crinoidal and Blue limestone. Its lower level is driven

along the limestone-rhyolite contact, having Blue limestone on the west, and rhyolite on the east; and a branch-level has penetrated the rhyolite for 25 feet.

Section UV, Fig. 5, shows Thirty Cut, where a body of ore, 62 feet long by 15 feet thick was mined. It filled a depression in the Blue limestone; and the section is notable for the fact that the porphyrite is shown for the first time, the section cutting it at the point where it is making its right-angle turn from south to east. The Carolina lower level, the eastern end of which is cut in this section, finishes in rhyolite, which continues to the end of the section.

Section W X, Fig. 6, cuts the thin flow of porphyrite which overlies the Blue limestone, and which covers the Bridal Chamber. This remarkable deposit was contained in a depression in the Blue limestone, and shows on the section as 104 feet long by 20 feet thick. The line of section, which is drawn through the Joint and Roberts shafts, passes through the western portion of the Chamber, the total size of which may be given as 100 feet square, with a thickness of from 10 to 20 feet. This space contained horn-silver, much of which was so pure that it was sawed and cut out in blocks, instead of being blasted. The exact yield of the Bridal Chamber is not known; but from the available figures the production may safely be put down as 2,500,000 ounces of silver, which is probably more than was ever taken out of a pocket of the same size. The Roberts shaft cuts through 31 feet of porphyrite, and is connected with the Bridal Chamber by a drift 50 feet long through Blue limestone; this shaft ends in the Nodular limestone at a depth of 86½ feet. No. 11 shaft cuts through 40 feet of porphyrite, and ends in the Blue limestone at 92 feet. Beyond this shaft the Crinoidal limestone is exposed for 37 feet, to a point where it is cut off by the rhyolite.

Section YZ, Fig. 7, marks the eastern limit of the known ore-deposits on the Grande. At the north end of the section, the ore was found on the surface, and considerable amounts were quarried and shipped from this point. The contact then gradually descends until, at the Daly shaft, a fair-sized pocket was found at a depth of 50 feet, the walls of which have not yet been entirely cleaned of ore, although the recent yield has been but moderate. The Office shaft is 137 feet south of the Daly shaft, and is 120 feet deep, the Blue limestone having taken a steeper dip. The Office workings have been connected with the Daly workings by a drift and upraise passing through the Nodular without finding ore. The short drifts

and incline near the bottom of the Office shaft show ferruginous flint in quantity, but without sufficient silver to make its mining profitable.

The Crinoidal and upper limestones extend to the southward 112

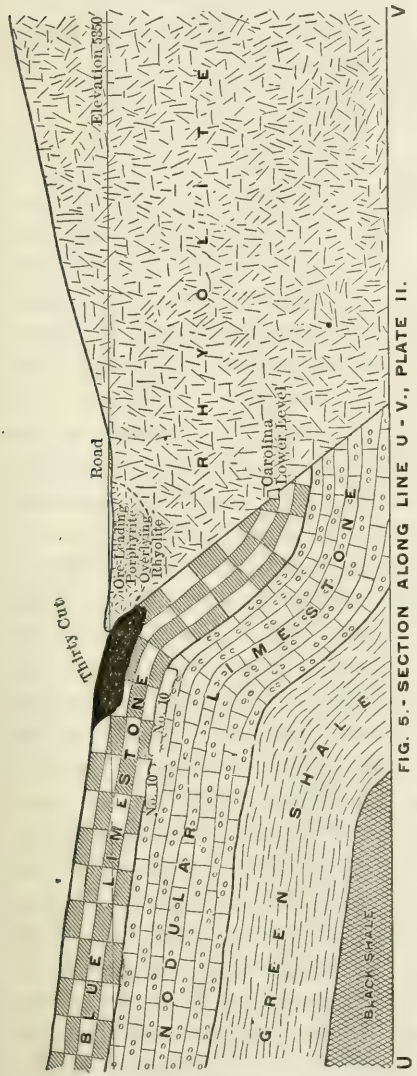


FIG. 5.- SECTION ALONG LINE U - V., PLATE II.

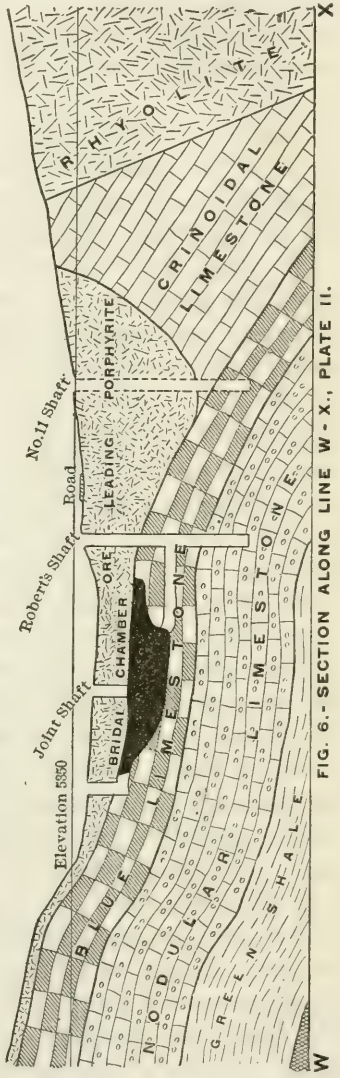


FIG. 6.- SECTION ALONG LINE W - X., PLATE II.

feet from the Office shaft, where the small and narrowing porphyrite is met in a trough 50 feet wide and 28 feet deep, through which Office shaft No. 2 has been sunk into the Crinoidal limestone. Beyond this porphyrite, the highest member of the sedimentary series,

the topmost limestone is met, and continues for 62 feet, until cut off by the rhyolite.

This topmost limestone is of a lighter blue-gray color than the Blue limestone, is thickly bedded, and occurs at two points only in the district, distinctly at the point already mentioned and indistinctly on the summit of Apache Hill. Erosion has caused its disappearance in all other parts of the area examined.

2. *The Bella Workings.*

The Bella workings, as before mentioned, consist of the Emporia Incline, the Bunkhouse, Harrison, Strieby and Bella Chute, and the Last-Chance workings. The first four of these are shown in the map, Plate III., and section, Fig. 8. On this map the porphyrite forms the northwestern boundary, and is about equal in area to the ground from which ore has been mined. The general parallelism of the trend of this porphyrite to the ore-chutes is noticeable. The workings from the Bunkhouse shaft are in an "L"-shaped pocket, the upright portion of the "L" underlying the porphyrite, and the horizontal portion going off in the form of a chute along the contact between the Crinoidal and Blue limestones to points where it is connected with the Incline and the Harrison workings. The Emporia, Harrison, and Bella workings form a single chute, lying strictly along the contact between the two limestones (Fig. 8). The Incline ore-body begins as a narrow chute, which was covered by a few feet of wash, and continues easterly, for 400 feet, to the bottom of the basin, where the ore is found at a depth of 150 feet from the surface, the deepest point in the district from which ore has been mined. The chute then bends sharply towards the northeast, rising gradually for 250 feet along the Harrison workings, being 69 feet from the surface and 125 feet from the porphyrite. Again turning towards the east, it forms two large, almost cylindrical, level troughs, directly under the contact and in the Blue limestone. This course it continues for 450 feet, until it is cut off by the Columbia fault. Along the Bella chute, the rise of the deposit and the fall of the ground bring the ore to within from 25 to 40 feet of the surface. The Incline ore, especially in the higher parts of the chute, has generally contained merchantable quantities of lead. It was low in iron, somewhat higher in manganese, with sufficient silica to make it a neutral ore.

The central portion of the Bunkhouse, at the angle of the "L," contained much horn-silver; the other portions yielded a basic ore

with some iron, high in manganese and very low in silica. The eastern workings along the contact yielded an ore low in silver and silica, but so high in iron and manganese that it was much sought after as a flux; and at present the iron and manganese waste is being assorted

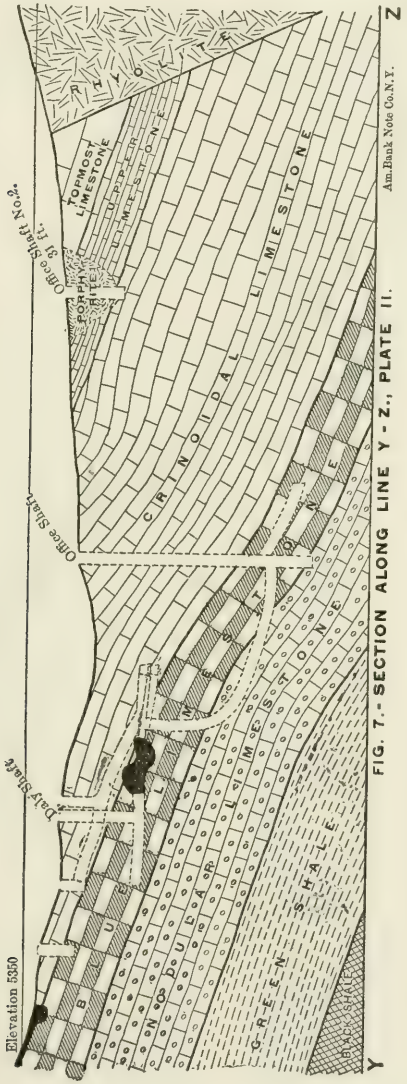


FIG. 7.- SECTION ALONG LINE Y - Z., PLATE II.

from the dump, and sold advantageously as a flux for the siliceous ores of Hillsboro.

Section AB, Fig. 9, shows the overflow of porphyrite through

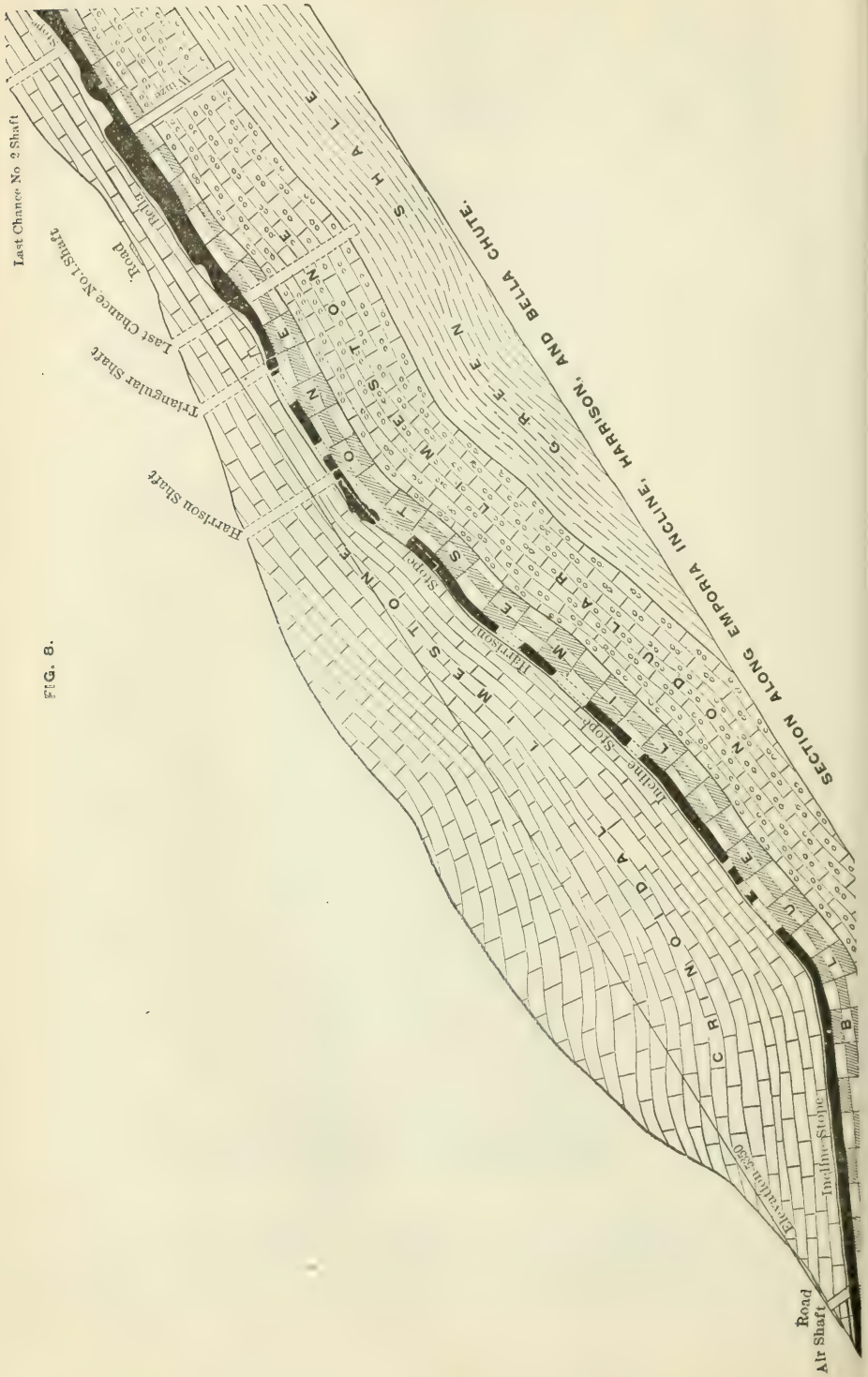

SECTION ALONG EMPORIA INCLINE, HARRISON, AND BELLA CHUTE.
Last Chance No 2 Shaft
Road
Last Chance No 1 Shaft
Triangular Shaft
Harrison Shaft
Air Shaft
Road
Incline Stope
Harrison Stope
Incline Stope
Elevation 3250
B
L
E
S
H
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FIG. 8.

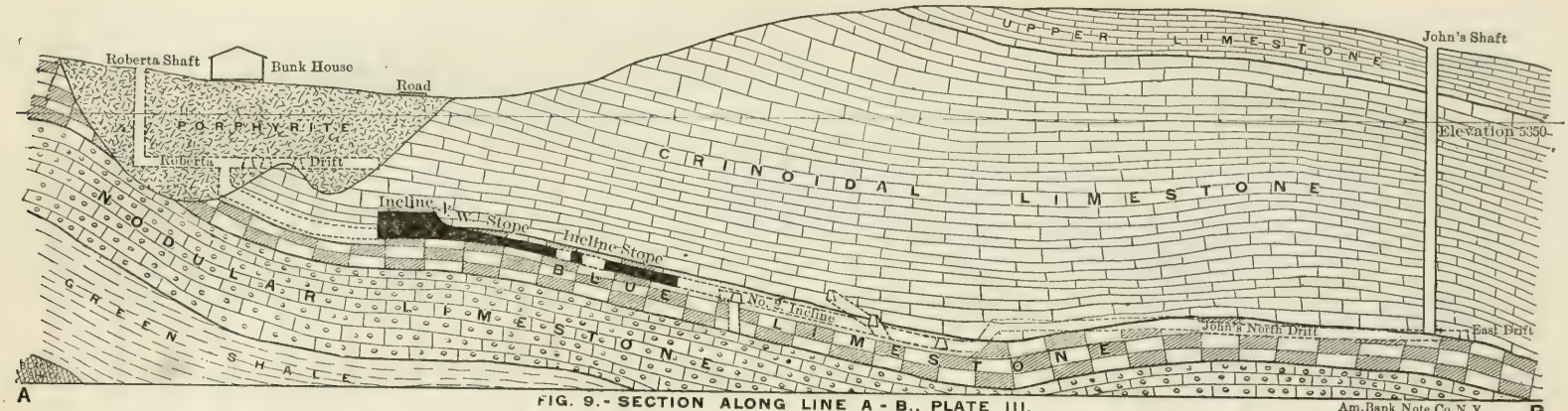


FIG. 9.- SECTION ALONG LINE A - B., PLATE III.

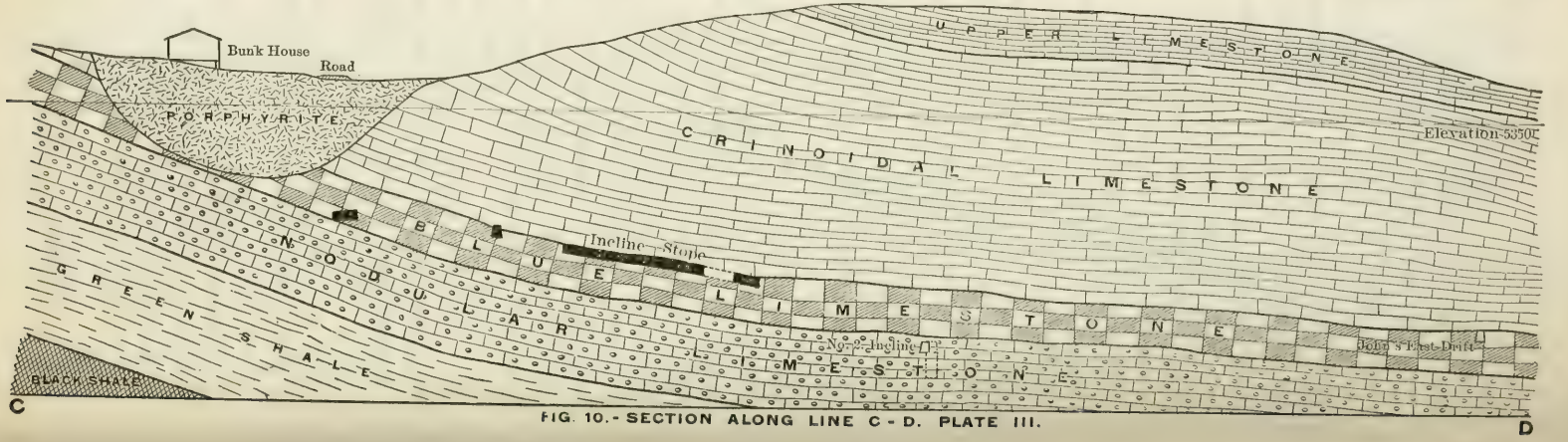


FIG. 10.- SECTION ALONG LINE C - D. PLATE III.

which the Roberta shaft has been sunk 54 feet, and a drift driven to the eastward, cutting the Crinoidal limestone in its course, and terminating in the same formation. An upraise was driven from the northwest stope along the contact of the limestones, connecting with the Roberta drift.

The Incline northwest stope was another large cylindrical trough, lying principally in the Blue limestone, but opening out to double size where it met the Crinoidal. The ore-body here was 15 feet high, averaging about the same width, and yielded a large quantity of neutral ore, considerably above the shipping standard in value. Seventy-five feet from the main stope of the Incline, this chute was cut off abruptly against a wall of limestone.

The width of the main ore-chute along this section is 87 feet, and on the southeastern side the contents in silver are sharply cut off, the iron-flint continuing in places along the contact on No. 2 incline for 165 feet, but too low in silver to be mined as ore. From the John's shaft, on the southeastern side of the section, which has a depth of 158 feet, a drift was driven close to the contact for 237 feet, connecting with the bottom of No. 2 incline. Two large chutes of ferruginous flint, each about 25 feet in width, were cut along this drift; but although all the iron contained traces of silver, no pay-ore was found.

The formation maintains a dip of 12° to the bottom of No. 2 incline, where it rises slightly, and continues nearly level to the foot of the John's shaft.

Section CD, Fig. 10, shows the porphyrite, and, directly under the wagon-road, a pipe of ore 13 feet wide and 6 feet high. This, as is shown on the map, connects the northwest stope of the Incline with the Bunkhouse workings. This pipe is remarkable as being entirely in the Blue limestone, close to the Nodular. For 75 feet after leaving the northwest stope it consists of a single pipe; but at that distance it separates into three, two paralleling the main chute of the Bunkhouse workings, and the other entering them at a right-angle. During a space of three years this was the only instance where the writer, in conducting mining operations, had both ends of an ore-body at the same time. This deposit was in the nature of a channel or water-course, the center of which formed a long cavern, and the roof, floor and sides were in ore.

The Incline stope on this section shows a gentle dip. No. 2 Incline shows at the base of the Blue limestone, and a winze from it was sunk 12 feet in the Nodular. From this point the contact lies

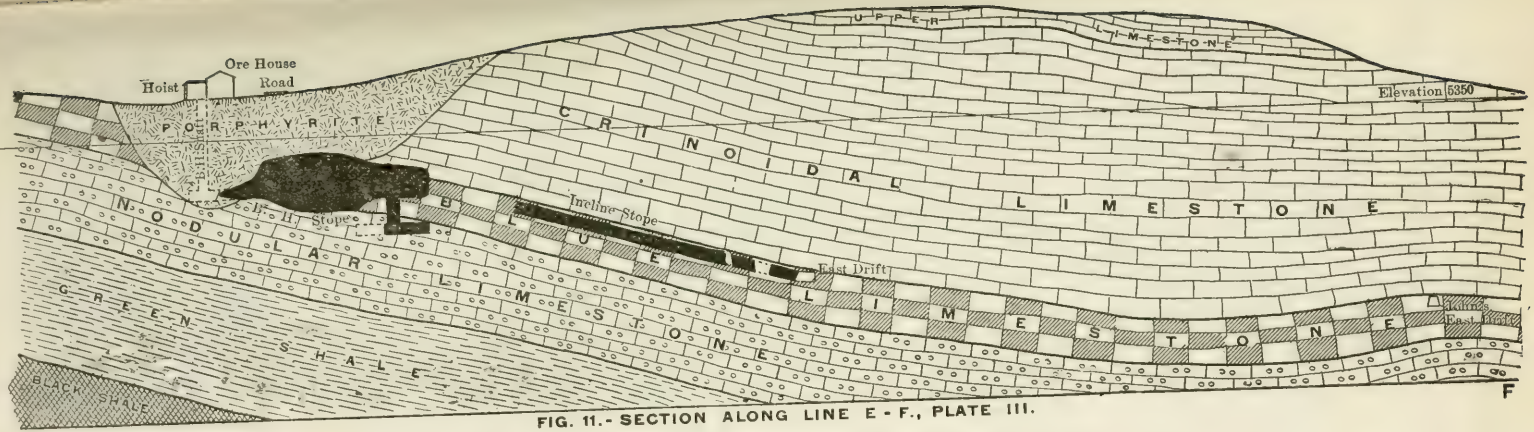


FIG. 11.- SECTION ALONG LINE E - F., PLATE III.

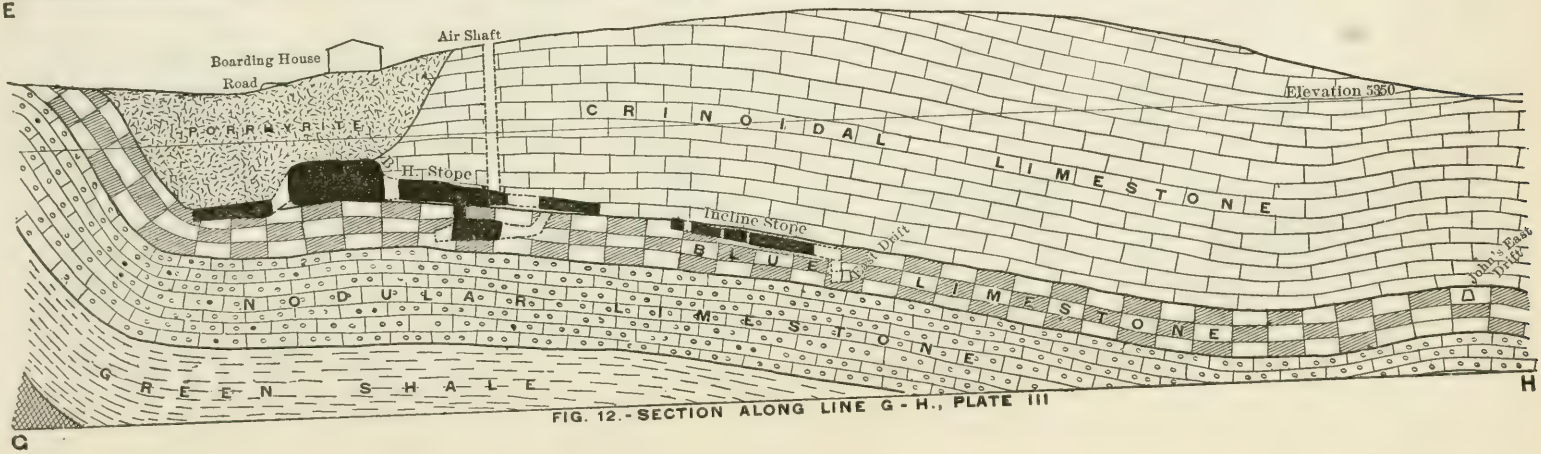


FIG. 12.- SECTION ALONG LINE G - H., PLATE III

almost level until the section terminates at the east drift of the John's shaft.

Section E F, Fig. 11, shows the Bunkhouse shaft, sunk through 54 feet of porphyrite. At this distance a short drift to the westward cut the Nodular. A drift to the southwestward opened the Bunkhouse workings, where, along this line of section, the ore-body was 100 feet long, with an average thickness of 20 feet and a maximum of 31 feet. Between the Bunkhouse workings and the Incline stope a barren pillar of Blue limestone was left, which was followed by 162 feet of high-grade ore. This has been mined during the last two years.

At the east drift the Incline chute turns to the northeast, forming the Harrison workings. The barren contact continues, with a slight dip, to the bottom of the basin, and is then nearly level until cut by the East drift of the John's shaft, at which point no ore was found.

Section GH, Fig. 12, begins in the Blue limestone, cuts through the overflow of porphyrite which covered the Bunkhouse stope, the workings of which extend, with lessened silver-contents, under the Crinoidal limestone, to the air-shaft, near which point a chute was followed well into the Blue limestone. This chute corresponds with the one mentioned under Section CD. Continuing, the ore along the Harrison chute overlies the contact in the Crinoidal limestone, but drops into the Blue limestone on reaching the Incline stope. The various workings around the east drift show the ore to be cut off in this direction, and nothing has been found to the southeast.

Section I J, Fig. 13, begins at the Morgan shaft, sunk in Blue limestone, passes by the Round shaft, which is sunk through the porphyrite and into the Blue limestone at a depth of 75 feet, and meets the workings of the Bunkhouse, which lie under both the Crinoidal limestone and the porphyrite. In the vicinity of the air-shaft the ore-body lies in the Crinoidal lime; but on reaching the Incline workings, it sinks under the contact. At the east drift the ore is cut off, and the various upraises and winzes failed to discover it. Nothing was found at the John's shaft east drift, at the end of the section.

Section KL (Fig. 14), shows but little ore. The porphyrite here widens to its maximum width. Comparatively small amounts of ore have been found in the Strieby. The porphyrite has been well explored by drifts, by the Strieby winze (which reached the Blue limestone at 125 feet from the surface), and by the north shaft, connected with the Bunkhouse workings.

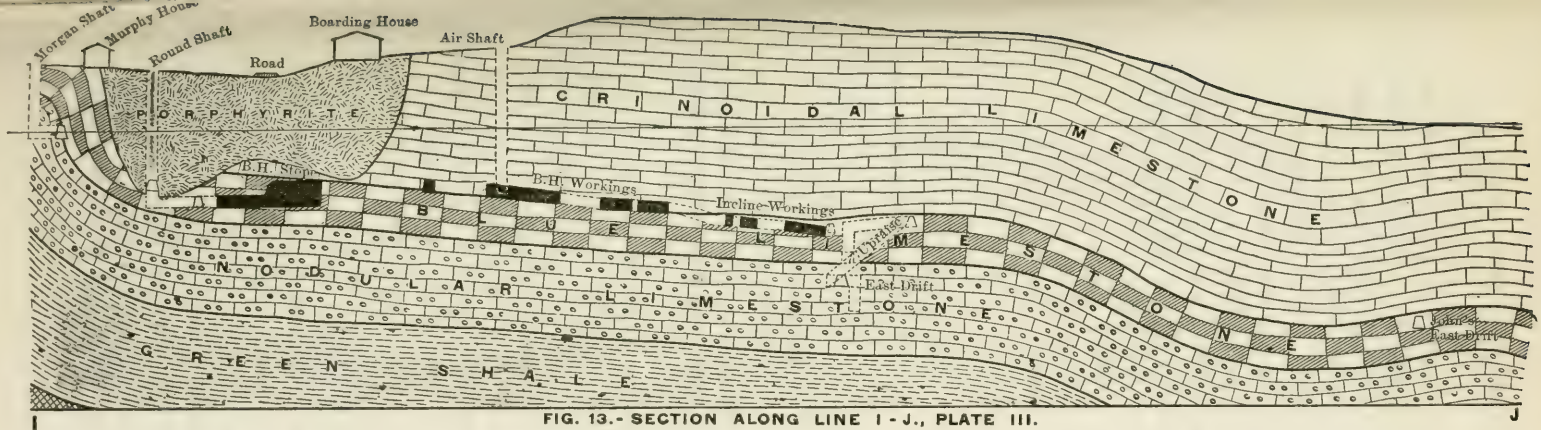


FIG. 13.- SECTION ALONG LINE I - J., PLATE III.

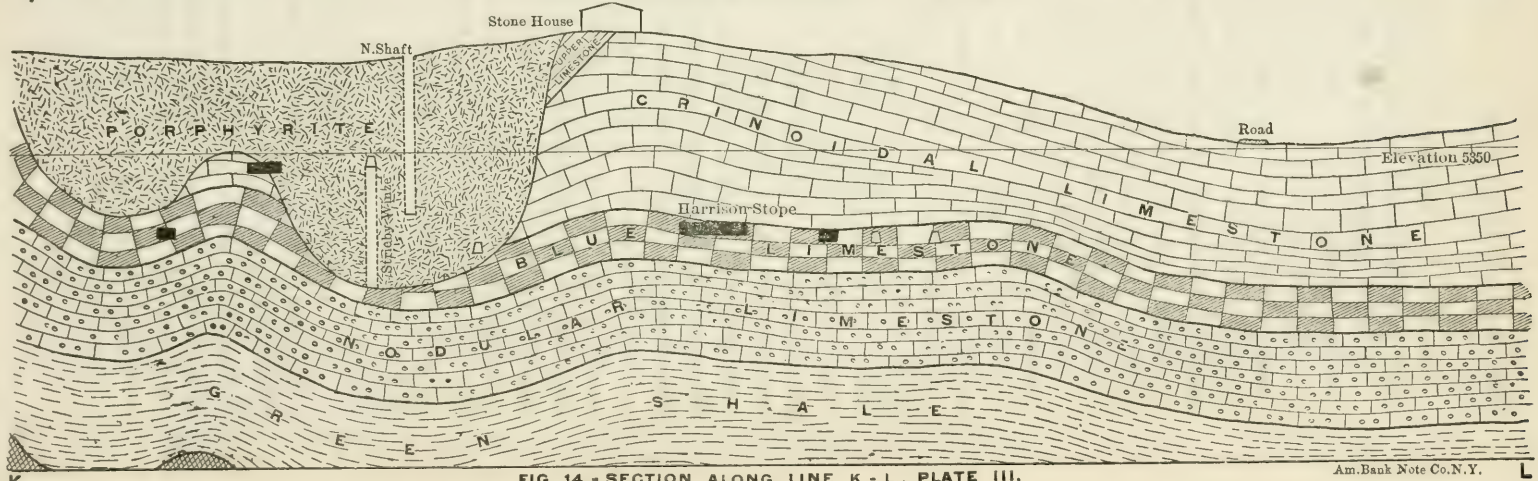


FIG. 14.- SECTION ALONG LINE K - L., PLATE III.

Am.Bank Note Co.N.Y.

Ore shows in the Harrison stope, and, to a small extent, in two other drifts along the section, in both instances of a rather low grade.

Plate IV. shows the complete underground workings. Those remaining undescribed are the Strieby, Columbia, Last Chance, Apache, and one joining the Bacon claim. The Strieby workings consist of a tangle of shafts, drifts and inclines, with but little stoping. They are in the limestones, underlie the porphyrite and have yielded ore in minor quantities. The Columbia workings lie at a distance of only 20 feet from the surface, and little is known of their production, except that tradition tells of a cart-load of ore which yielded $20,000.

The Last Chance workings are a quite recent development. The discovery-shaft is situated 125 feet north of the Bella Chute. In the shaft a body of manganese-ore was struck on the contact, at a depth of 14 feet. The workings showed a main chute running to the westward, and a subordinate one that left the main chute and curved back into it at a distance of 50 feet. The Last Chance ore-body was struck at the richest and most basic point. It contained at the shaft considerable horn-silver, little silica and a large percentage of manganese and iron; but at a distance of 50 feet from the shaft, the silver-, iron- and manganese-contents had decreased, and the silica-contents increased, so that it was not salable as a flux; and at 87 feet from the shaft, the silver-contents were so low and the silica so high, that it was no longer profitable to mine.

The Columbia and Last Chance claims show what is known as the Columbia slip or fault. This is an upthrow of the Blue limestone, which first shows at the dwellings on the wagon-road leading to the limestone-quarry, then curves to the southward, passing through the Columbia workings, where the Crinoidal is found abutting against the Blue limestone. It passes by the Last Chance workings, cutting off the extension of ore to the eastward, and continues to the south, cutting and terminating the Bella Chute in its progress.

3. *The Apache Workings.*

The workings on the Apache have been principally for iron flux, the silver-contents in general having been very low. The ore lies close to the surface, and has sometimes been quarried, and sometimes mined, leaving a shell of hanging-wall. Shafts sunk in the workings of this claim show considerable bodies of siliceous iron, containing but few ounces of silver to the ton.

Close to the Bacon claim, ore was taken out to the extent of

30,000 ounces. The ore was found in its usual place on the contact between the Blue and Crinoidal limestone, but the porphyrite which generally occurs near the ore, was lacking at this point.

The contact of the two limestones continues to the northeast from the Bacon.

Much money has been expended here in the sinking of prospect-shafts, and at places along this contact the iron-flint is very well developed; but with the exception of float taken from the surface, no ore-shipments have ever been made.

The average assay and analysis of 4000 tons of ore, a year's shipment by the company during the writer's administration, was:

Silver,	47.7 oz. per ton.
Lead,	Trace.
Silica,	28.5 per cent.
Iron,	13.9 "
Manganese,	18.2 "
Zinc,	4.0 "

This sample represents principally the Incline and Bunkhouse, with some ore from Thirty Stope and Twenty-Five Cut.

Theory of the Ore-Formation.

It has been held, almost from the time of their discovery, by those familiar with the deposits of silver-ore at Lake Valley, that the ore must have come up in solution from below; that it came along the "blanket" of iron-flint; and that it was in some way dammed up or stopped by the overflow of porphyrite, which may be said, in a general way, to overlie the outcrop of the "blanket." On the strength of this hypothesis, numerous diamond-drill-holes and shafts have been sunk, and those that were continued to a sufficient distance (seldom more than 150 feet) have encountered the iron-flint blanket, but invariably with its silver-contents lacking.

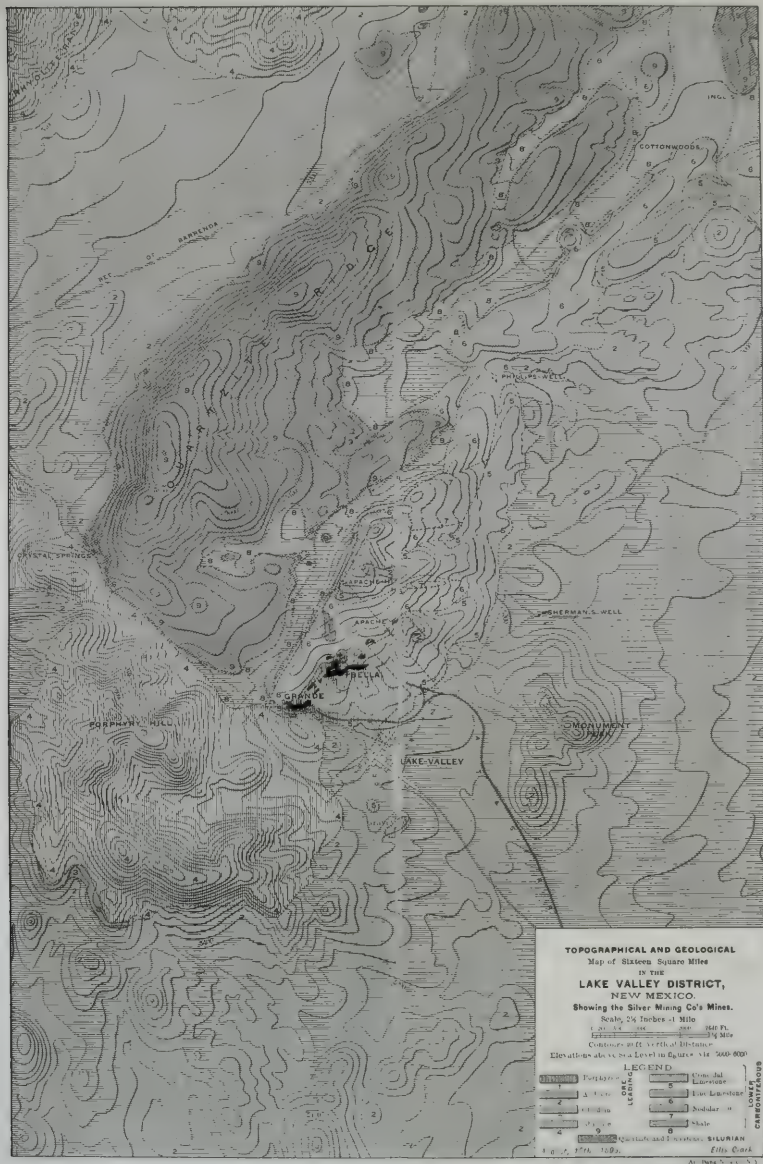
A later and more probable hypothesis is that the silver of the mines was originally contained in a great overflow of silver-bearing porphyrite, perhaps coming from Monument Peak, which covered a square mile or more in the immediate vicinity of the mines. In the erosion of this porphyrite, the silver in it was leached out, the greater portion segregating itself in the Bridal Chamber and the workings connected with it, and the remainder going to the Bunkhouse and the connected Incline and Bella workings. The greatest distance that any large body of ore has been found from the line of the porphyrite is 500 feet, and most of the workings are within 200 feet of that line.

The writer's own observations have shown him that at a distance of about 250 feet from the porphyrite, the ore rapidly decreases in grade, and that at a distance of 300 feet there is little that can be profitably shipped. The Bunkhouse workings appear to have been a cavern, in which the ore was deposited rapidly, and not by the slower process of a dissolution of the limestone and a synchronous substitution of the silver-bearing manganese. In many places in this working the manganese is pulverulent and non-adherent to the limestone walls; and when thoroughly cleaned off by brushing, the face of limestone has precisely the same weathered appearance as that of an outcrop, and looks as though it had been freely acted upon by the atmosphere, possibly assisted by the rays of the sun. Something of the same sort may be studied in the Last Chance workings at a depth of 20 feet from the surface, while the Bunkhouse workings lie at a depth of from 50 to 60 feet.

The evidences of a previous cavern or cavity in the Blue limestone at the Bridal Chamber are not so marked, but the indications are such that in the writer's opinion a comparatively rapid deposition appears more probable than a gradual substitution, such as was very likely the case in the Incline workings, the Bella Chute, the Thirty Stope, and Twenty-Five Cut workings.

In a property of the extent of the Lake Valley mines, which has yielded at least $5,000,000, there always remains the possibility of new finds through the expenditure of small amounts of money. The contact between the two limestones is an established fact; and there are but few places on the southeastern portion of the property where this contact cannot be reached at the moderate depth of 150 feet. Thus far, the explorations made at a distance from the porphyrite have been barren of commercial results; but from the occurrence of the ore in chutes, which, although constituting a part of the "blanket," vary in width (being generally narrow close to the surface and widening in depth), it is possible that large bodies, somewhat of the nature of the Incline or the Bella Chute, may still exist in the unexplored portions of the property.

The occurrence of new bonanzas, such as the Bridal Chamber and the Bunkhouse, is scarcely to be expected, as the conditions under which they appear to have been formed—viz., the triple contact of the Blue and Crinoidal limestones and the porphyrite—are not known to exist at any points as yet unexplored, and the overflow of porphyrite has been so thoroughly prospected as to leave but little unexplored ground of that class.


COTTONWOODS
BARRENDA
CRYSTAL SPRINGS
APACHE
SHERMAN'S WELL
SPRINGS
GRANDE
PORPHYRY HILL
LAKE-VALLEY
MONUMENT HILL
TOPOGRAPHICAL AND GEOLOGICAL
Map of Sixteen Square Miles
IN THE
LAKE VALLEY DISTRICT,
NEW MEXICO.
Showing the Silver Mining Co's Mines.
Scale, 2½ Inches = 1 Mile
Contours at 25 Vertical Distance
LEGEND.
Conglomerate
Limestone
Shale
Quartzite and Limestone, SILURIAN
Ellis Clark

The most promising quarter for further exploration would seem to be the extension of the Grande chute at some point south of the John's shaft workings, where, as before mentioned, large chutes of iron-flint, too low in silver for profitable working, were cut. Other points which should be prospected are the extension of the Incline Chute, south of the Bella Chute, and the extension of the Bella Chute beyond the point where it has been cut off by the Columbia fault.

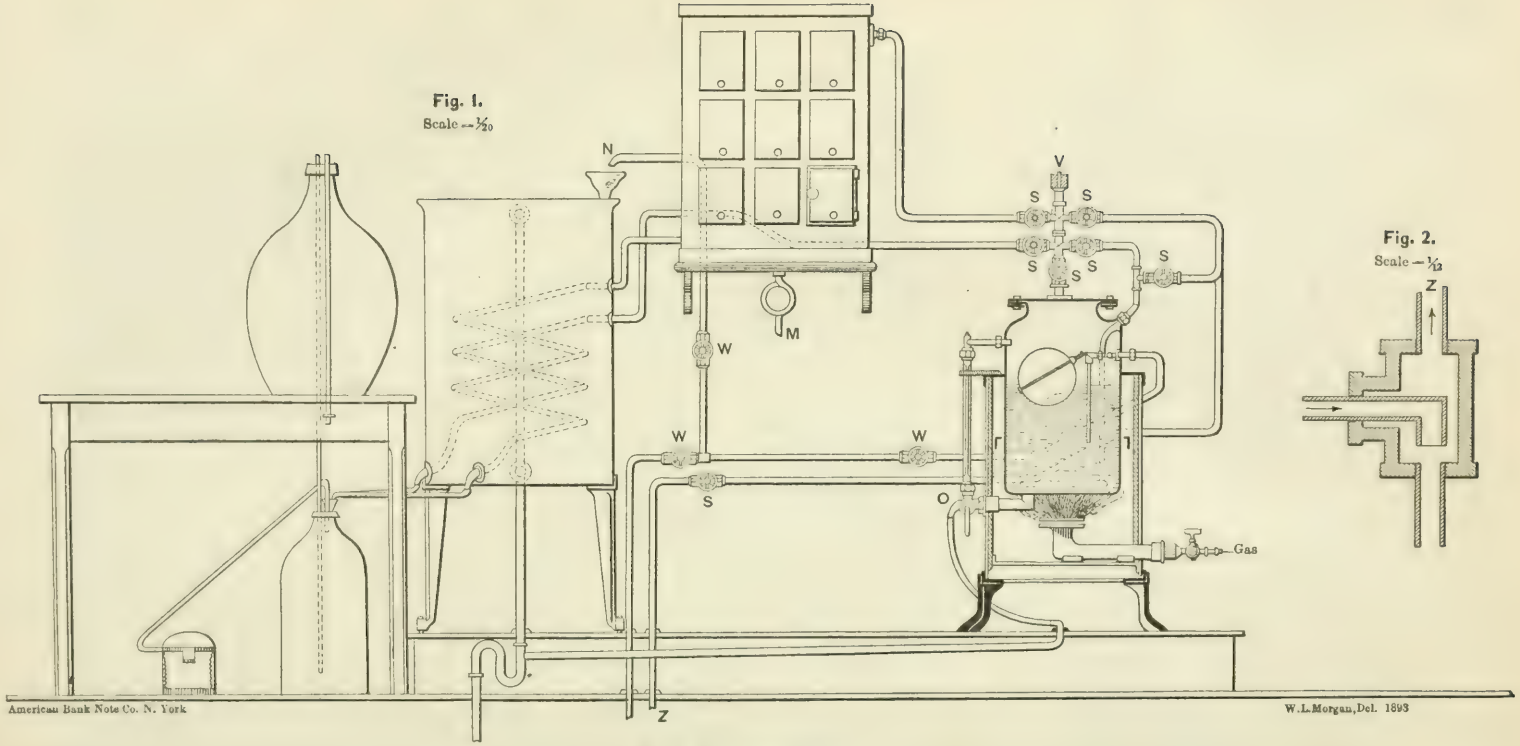
Company work by day's labor in the mines was stopped in August, 1893, on account of the low price of silver, and at the present writing has not been resumed.

A CONVENIENT STILL FOR THE LABORATORY.

BY CHARLES E. WAIT, PH.D., F.C.S., UNIVERSITY OF TENNESSEE, KNOXVILLE, TENN.

(Virginia Beach Meeting, February, 1894.)

In the use of the apparatus purchased for the new chemical laboratories of the university, no piece has given us more satisfaction, or has been a greater success, than a new still which is the subject of this paper. In the designing of this still I had two definite objects in view; one, the utilizing of steam from the large 60 horse-power boiler used in heating the building, and the other, a provision for making distilled water with gas, when steam from the heating-plant was not available. The still is encased in wrought-iron, with a lining of asbestos, and is provided with a float which automatically regulates the flow of water from the supply-pipe. It was made for me by G. J. Mürrle, Pforzheim, Germany, and was imported by Messrs. Eimer & Amend, of New York City. It is a beautiful piece of apparatus, and the workmanship is excellent in every detail. The essential parts of this apparatus will be understood by consulting the accompanying figures, but a few words of explanation may be appropriate. The still has a capacity of seven gallons, and is made of heavy copper, tin-lined. It has a water-gauge showing the height of the water in the still, also an exit for steam, and safety-valve, V; inlet for steam through a coiled perforated block-tin pipe; also an outlet, O, for cleaning out the still; and there is also a heavy



A Convenient Still.

disc gas-burner. There is a conveniently arranged drying-oven and a large condenser, carrying two block-tin worms, one direct from the still, the other from the oven, both delivering water to a carboy resting on the floor. When steam is taken from the large boiler, provision is made, by an arrangement seen in Fig. 2, located at Z, beneath the floor, to cleanse the steam, before it reaches the still, from the water mechanically carried into the pipes. The water returns to the boiler, while the steam passes to the still. By the arrangement of the steam-valves, S, it is possible to deliver the steam to the empty still, where it is again washed, and may then be passed either to the oven or direct to the condenser or to both. Or the steam from the boiler may be taken direct to the oven or to the condenser or to both. When a small quantity of steam is admitted to the still, most of the distilled water is condensed in the oven and is caught in a copper tin-lined vessel underneath, through the valve, M, a part passing to the condenser, and then to the glass vessel, from which it may be forced to the carboy above. When there is no steam in the large boiler, gas is used, the water being kept in the still at a constant level by the automatic arrangement before mentioned and seen in the sketch. In this case the steam may be passed to the drying oven or to the condenser direct or to both, as heretofore mentioned. In the case of gas alone, the still has a capacity of about 26 gallons of distilled water per day, while with steam from the boiler, at a pressure of 5 pounds, I have made distilled water at the rate of 112 gallons per day. A convenient system of piping has been arranged for supplying water to the still and condenser, to the latter at N, and under control of valves, W, as seen in the sketch. The still and condenser are both connected with the waste, and may be emptied and cleaned when necessary. Other details of this apparatus are shown in the drawings. As I have used this still constantly for a year, and fully appreciate its merits and good points, I have no hesitation in recommending the design to those who may wish a still for the purposes for which this one is used.

BENJAMIN HUNTSMAN, OF SHEFFIELD, THE INVENTOR OF CRUCIBLE STEEL.

BY R. A. HADFIELD, SHEFFIELD, ENGLAND.

(Virginia Beach Meeting, February, 1894.)

THE present tribute to a great pioneer in the steel industry, with the accompanying remarks upon the town of Sheffield, which has remained to this day what his invention made it, an important center of that industry, originally formed a part of the paper on "Iron Alloys," prepared, at the invitation of the Council of the Institute, for the Chicago meeting of August, 1893. That paper, begun with the intention of dealing with many alloys of iron, became, of necessity, through the limitations of time on the part of the writer and of space in the *Transactions* of the Institute, mainly a discussion of the principal modern commercial alloy of iron, manganese-steel; and much introductory and general matter was consequently omitted. The Secretary suggested, however, that this particular portion would be interesting and appropriate as a separate contribution, and I therefore present it as such, offering the foregoing statement simply as an explanation of its incomplete character. If I had undertaken to write a paper upon this subject alone, I should have felt myself bound to bestow upon it more elaborate research and to treat it with more abundant details.

It was in the immediate neighborhood of Sheffield that the first successful process for the fusion of steel on a commercial scale saw the light. The late Dr. Percy, a leading authority in general metallurgy, says:

"Formerly, so far as I am aware, steel was never melted and cast after its production; and in only one instance, viz., that of wootz was it ever molten during its production. . . . . Now, by the fusion and casting of steel after its production its heterogeneousness is remedied, and ingots of the metal may be procured of perfectly uniform composition throughout; and for the practical solution of this important problem we are indebted to Benjamin Huntsman, of Sheffield."

As a recent American journal appropriately remarks:

"Huntsman's patient efforts, at last rewarded with success, entitle him to an elevated niche among the heroes of industry. The invention of cast-steel was second

in importance to no previous event in the world's history, unless it may have been the invention of printing."

This citizen of Sheffield was born in 1704, his parents being natives of Holland, who came over and settled in England. He belonged to that sturdy religious persuasion, the Quaker body, which has done much for Great Britain, as it has for a large State in America, interested in the iron and steel manufacture. His character is shown by the fact that he would not allow any portrait to be taken of himself, and he refused an offer to be made a member of the Royal Society in 1750, when his fame had already begun to spread.

The writer has had the pleasure of knowing both the great-grandson (recently deceased) of Benjamin Huntsman and also the present head of the business and fourth lineal descendant, Mr. Frank Huntsman. The latter successfully carries on the manufacture of the highest qualities of Huntsman's Cast-Steel, the name of which still stands in the first rank for quality, a striking example of continuous success in a business which is founded on the production of an article of the highest and best character.* Not many of us can go back four generations to the origin of our business.

As some readers of this paper may not be aware why the first production of steel alloys occurred in the neighborhood of Sheffield, a brief account of this region and of Huntsman's early work may be of interest.

For several centuries Sheffield has been famous for its hardware productions, particularly in articles made of steel. Chaucer, about the end of the fourteenth century, spoke of Sheffield "thwytels" (from the Anglo-Saxon "thwyten," to cut or whittle), and, owing to the city's favorable surroundings, the trade grew to dimensions then considered large and important. It has been described by some as a city founded upon seven hills, and although the writer has never yet been able to see this resemblance to the world's former metropolis, certainly there are plenty of hills and valleys in Sheffield. The latter naturally afforded a cheap and easy supply of water-power which, in times past, contributed much to the growth of the town. To this day its "grinding-wheels," as they are called, gradually dying out with the introduction of steam-power, dot the streamlets here and there. Many an American visitor would find it interest-

* In the appendix is given a copy of an interesting report relative to the Huntsman process, published March 28, 1792.

ing to break his journey at the good old city, and see for himself these interesting reminiscences of the past. Black as it has been painted, and dirty as it may seem to one passing through in the cars, there is no city more picturesquely surrounded—"a black picture set in a golden frame," as Lord Palmerston said. There is certainly no better starting-place for a tour into the regions of the Peak or to such places of interest as Chatsworth, Haddon and Hardwick Halls, Bolsover Castle, Edale, Snake Inn, etc. A new branch of the Midland railroad, known as the Dore and Chinley Branch, will still further open up an undeveloped and interesting district not hitherto easily reached.

Although we cannot offer Colorado Cañons, Pike's Peaks, or Gardens of the Gods, or a Kansas prairie, and certainly no Niagara Falls, there are sights not unattractive in the High Peak district, with its curious folk-lore and its general historical associations; Dovedale, where Izaak Walton pursued the gentle sport, or the noble domains of Chatsworth or adjacent Haddon Hall, portions of which first saw daylight at least seven centuries ago. Southeast of us are the noble ducal estates of Welbeck and Clumber, from whence stretch the glades and woodlands of Sherwood Forest for many a mile, and where once rang the shouts of Robin Hood and his "merrie men." Most of these are almost within sight of Sheffield's smoky city. If you will not buy our hardware, we nevertheless freely offer the natural advantages we possess, and which are only conferred on a nation by Father Time.

In the early part of Huntsman's life, about 1740, there was one great drawback in connection with the development of Sheffield. All the materials used had to be imported either from Sweden or Germany. Importation, to an Englishman, does not at all mean destruction to English industries, but in this instance the quality of the imported article was objectionable, for it was variable.

Blister- or cement-steel was imported from Germany and Sweden, or, in some cases, the material obtained was a raw puddled or natural steel. A considerable trade was also done with Newcastle-on-Tyne, where several cementation-furnaces were worked, probably because the Swedish bar-iron more readily found its way there, owing to shipping-facilities. Whether these latter furnaces existed when Huntsman first commenced his experiments is not very clear; but, in 1774, M. Jars, a French expert, who visited England that year, observed in his interesting *Voyages Métallurgiques:* "There are many manufacturers of iron and steel (cemented) at Newcastle-

on-Tyne;" and it appears that a considerable quantity went to Sheffield.

Huntsman, being a maker of watches and clocks, often experienced much inconvenience from the irregular quality of the imported blister-steel. For fine work of this class the utmost attention is essential to ensure uniformity of production.

He was then settled in Doncaster, and from reported proofs of his ingenuity it appears that he was already known as the "wise man" of the neighborhood. It is, therefore, not surprising to find that his active brain set to work to master the problem from which we to-day are reaping so great a benefit, namely, the production by fusion of cast-steel.

From a recent excellent paper by Mr. L. H. Holland, F.G.S., Assistant Superintendent of the Geological Survey of India, it would appear that Indian wootz steel, usually found in conical ingots and made by the carburation of wrought-iron *crucibles* (so he states), has been made in India, and most probably for many centuries, especially in Trichinopoly.

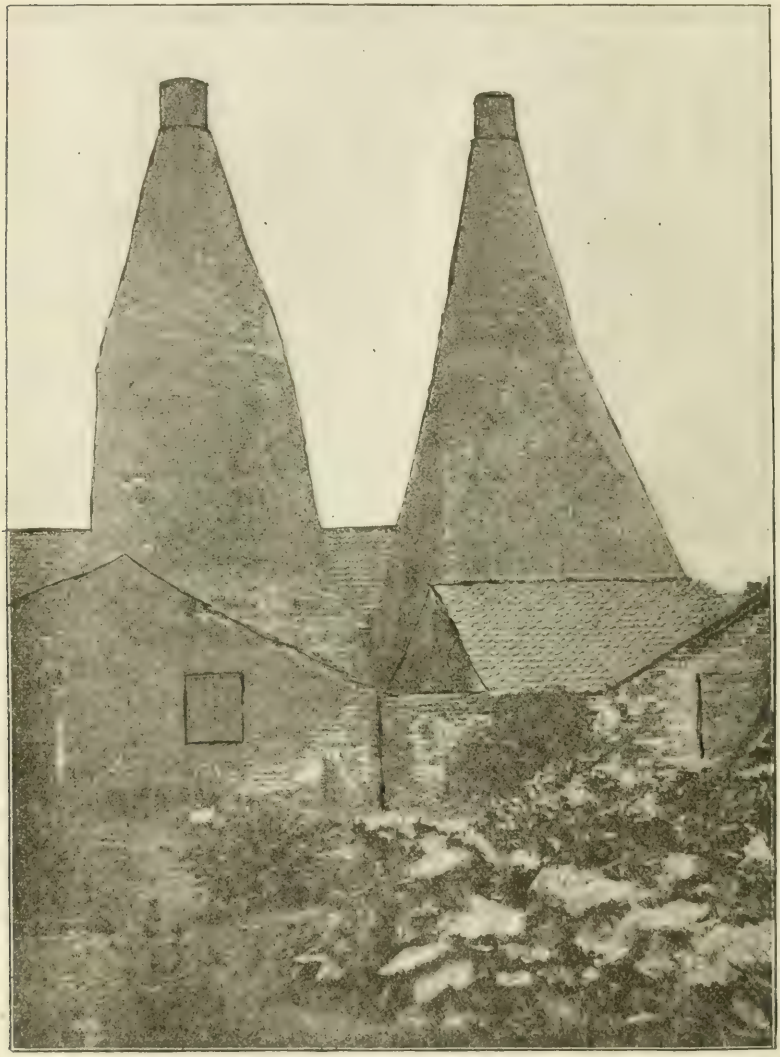
Nevertheless, Huntsman was very clearly the first to establish a fusion-process on something like scientific lines, and to make it a practical and commercial success. Smiles, in his *Industrial Biography*, gives an interesting account of the discoverer, giving him the full credit after very careful investigations. Moreover, M. Le Play,* Professor of Metallurgy in the School of Mines at Paris, after very carefully weighing all the evidence obtainable, stated that without doubt the credit of the invention belonged to Huntsman. Finally, a controversy was conducted in the London *Times*, some twenty-five years ago, and the discoverer, as we believe, was again fully vindicated.

Difficult as must have been the problem in the then crudely developed state of knowledge, having set his hand to the plough, Huntsman would not turn back. The crux of the difficulty lay in obtaining a fire-clay that would enable him to make a vessel or crucible in which the bar-iron or cement-steel could be made molten. At that time there was practically no knowledge as to the requisite chemical constituents of a fire-resisting material. There was uncertainty as to the character of the materials to be used in melting; melting-appliances were imperfect, and there was difficulty in obtain-

* "Mémoire sur la fabrication de l'acier en Yorkshire" and "Mémoire sur la fabrication et le commerce des fers à aciers dans le Nord de l'Europe."—*Annales des Mines*, fourth series, vols. iii., 1843, p. 583, and ix., 1846, pp. 113, 209.

ing the most suitable fuels. These and other obstacles would have appalled any but the stoutest heart.

Fig. 1.



Old Huntsman Furnaces, Sheffield.

Mr. Frank Huntsman has informed the writer that evidences in their works were formerly abundant, and even quite recently some have been discovered, of the large number of experiments which

had evidently been carried out in the early stage of the process. Buried salamanders are not unknown in the present history of metallurgy, and those found in the works of Huntsman are a proof that as now, so in the past, success usually comes after many trials.

Huntsman's first experiments were made at Doncaster, a town eighteen miles from Sheffield, to which city he removed about the year 1740. Here his further experimental work was carried out at Handsworth, a suburb of the town. Finally, he removed to Attercliffe (Otter-on-the-Cliff*), a manufacturing district forming part of the city, and his works are still in existence, considerably altered and enlarged, but situated in the street known to this day as "Huntsman's Row."

Through the courtesy of Mr. Huntsman and the kindness of Prof. Arnold, whose skill as an amateur photographer is famous in Sheffield, the writer is able to present a view of part of the original works. These are of special historic interest, showing, without doubt, as they do, where crucible cast-steel was first produced on a practical and commercial scale.

Fig. 1 represents the cementation-furnaces practically as built and used by Benjamin Huntsman. As will be seen, the design differs in several respects from those of more modern construction.

Within a few yards of the works is Benjamin Huntsman's house, where he lived until his death at the age of seventy-two, June 20, 1776. His remains lie in the family vault in Attercliffe cemetery.

Fig. 2 is from a photograph of Huntsman's clock, dating from about the year 1750. The works and face were made by Benjamin Huntsman. The steel employed in the works, including that of the pendulum-rod, is believed to be the result of his first successful experiments.

The following is a remarkable proof of the high quality of the Huntsman steel. Some manganese-steel, made by the writer's firm, was supposed to exhibit less hardness than usual. It was hard, yet it could be drilled, though very slowly, by a particular drill. On inquiry, it was found that this drill, which possessed such superior qualities, was made from Benjamin Huntsman's steel. The following was its analysis:

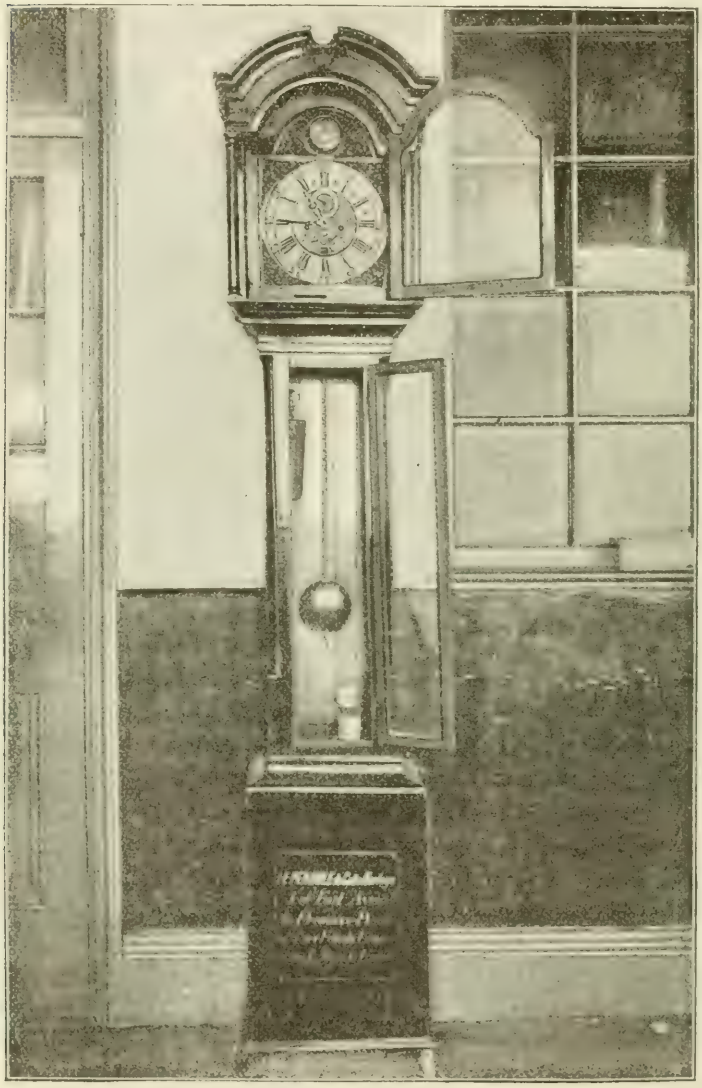
C, 1.40; Si, 0.17; S, 0.047; P, 0.017; Mn, 0.18 per cent.

Mr. Frank Huntsman has informed the writer that this analysis

* The otter, it is unnecessary to add, is now about as extinct as the dodo.

is typical of the kind of steel produced at the first inception of the system over a century ago, and that the steel above named is manu-

FIG. 2.



Huntsman's Clock.

factured practically in the same manner as then practiced. The analysis given confirms this statement, as it will be seen that the

manganese is exceedingly low, showing no signs of "physic" or other additions. It is not intended to convey the idea that the carbon-steel above named will tool manganese-steel in a manner applicable to commercial wants. A steel that will do that is still lacking.* Nevertheless, it is a remarkable proof of the value of crucible cast-steel and of the correctness of the lines worked out by Huntsman as regards the production of a special quality of high-class steel for tools and other special purposes.

The Attercliffe works supply to-day the world-renowned steel of the same kind and produced by the same methods as those employed more than a century ago. That is to say, cement-steel of the purest quality is made homogeneous by fusion and "refined," to use the old trade-term. No refining occurs in the true sense of the word; but carbonized bar, previously of variable and heterogeneous character (that is, as regards carbon-contents), is made by fusion homogeneous and applicable to a large number of purposes for which, previous to this treatment, it was unsuitable. An excellent account is given of the methods originally practiced in Sheffield about 1764 by M. Gabriel Jars, in his *Voyages Métallurgiques*, edited by his brother, and published in 1774.

"Blister steel is rendered more perfect by the following operation: Ordinarily, the scrap and cuttings from articles of steel are used. Furnaces of fire-clay (*fourneaux en terre*) are used, of similar design to those for brass castings. They are, however, much smaller, and receive the air by an underground passage. At the mouth, which is square, and at the surface of the ground, there is a hole through the wall, from which ascends the chimney-stack. These furnaces contain only one large crucible, 9 to 10 inches high and 6 to 7 inches in diameter. The steel is put into the crucible with a flux, which is kept secret, and the crucible is placed upon a round brick, set upon the fire-bars. Coal, which has been reduced to coke, is placed round the crucible, and the furnace is filled. Fire is then put to it, at the same time the upper opening of the furnace being entirely closed with a brick door surrounded by a circle of iron. The flame goes through the pipe into the chimney.

"The crucible is five hours in the furnace before the steel is perfectly melted. Several operations follow. Square or octagon moulds, made in two pieces of cast-iron, are put the one against the other, and the steel poured in at one extremity. I have seen ingots of this cast-steel which resemble pig-iron. This steel is worked under the hammer, as is done with blister-steel, but is heated less highly and with more precaution because of its liability to break.

"The object of this operation is to make the steel so homogeneous that there

* The difficulty in tooling manganese-steel does not arise from its hardness alone; it is the combined toughness and hardness of the steel which necessitate much greater force to move the surfaces operated upon and more than the nose of a tool of comparatively brittle material like hardened carbon-steel will face. Hence, with very low speeds only can any results at all be accomplished.

may be no flaw, as perceived in that which comes from Germany; and this, it is said, can only be done by fusion.

"This steel is not extensively used; it is used only for purposes requiring a fine polish. Of it are made the best razors, some knives, the finest steel chains, some watch-springs, and small watchmakers' files."

The following passage, in another part of the same work, shows that attempts were being made in Newcastle, as early as 1765, to imitate Huntsman's methods:

"We were told also that in the southern part of England, that is, south of Newcastle, old files, or other old steel articles, or blister-steel, are cut into pieces and put into a crucible with a flux, which is kept secret. It is said that each workman has his particular recipe. These crucibles are placed in a furnace to melt the steel. One person in particular has undertaken this process, two miles away from this town (Newcastle), but he has succeeded badly."

No doubt Huntsman's attention was at first confined to the supply of a material suitable for the purposes of his own immediate business, that is, clock-springs; but it could not have been long before he saw the further important applications and uses that awaited the eventual development of this process, of which the fusion-processes of Bessemer and Siemens are really the offspring. All these processes are dependent upon a knowledge of the properties of different steel-alloys, confined in those early days to carbon-steel. It is highly probable that future generations will be largely dependent in their material progress upon a thorough and more exact knowledge of this branch of metallurgy.

Huntsman exported his steel to France, a trade still held by his firm. Sheffield cutlers became jealous of the advantages they thought his steel offered to foreign competitors, and it is said that they tried to influence the government of the day (probably by "lobbying," a practice not unknown to modern politics), but, fortunately, they failed in their attempt to restrain trade.

That the process soon spread, is shown by a Sheffield directory, published by Gale & Martin in 1787, that is, about ten years after Huntsman's death. A list of steel-manufacturers is there given, from which it appears that in addition to Huntsman's firm, then trading under the name of Huntsman & Asline, as "steel-refiners and melters," there were several other firms already engaged in the same business. The directory states that five firms were engaged in melting or refining, and about a dozen in the converting or cementation-process.

That Sheffield can pre-eminently claim the title of "Steelopolis,"

not less from its modern development than from its long-standing and traditional associations with the early developments of the metallurgical industry of iron and steel, is shown in an interesting manner by the same directory. We find that there were then some half-dozen manufacturers of adzes and hammers; about 50 makers of edge-tools; not less than 40 engaged in file-making; over 300 in pen-, pocket-, and table-knife manufacture; at least 50 in razor-making; close upon 100 in scissors; and some 60 or 70 in the manufacture of scythes, sickles, and shears. Many of these were, no doubt, small workers rather than owners of large concerns; but it will be seen that here was the center for a considerable employment of steel. It was this, no doubt, that induced Huntsman to settle in Sheffield. The advantageous environments also proved to be of the greatest assistance in its rapid development. For example, its excellent supply of very pure water (also a source of cheap power) was believed by some to be of special quality and efficacy in the hardening of steel.

In these days of investigation, many of the old ideas on these subjects have been exploded, and probably there is nothing in Sheffield water that cannot be obtained elsewhere, at least from water showing upon analysis the same chemical composition. Yet, not very long ago, a considerable quantity of Sheffield water was exported to America for hardening purposes.

It is curious that the ancients not only thought that the quality of the water was the most important factor in obtaining the right quality of steel, but that the Latin and Greek word for steel, *Chalybs*, was believed to have been given to a Spanish river (known today as the Cabe) in the Royaume de Galice, which flowed into the Vélézar, and the water of which had the reputation of conferring the best kind of hardening upon steel. M. Duhannel, a clever metallurgist of the last century, pointed out the foregoing in his *Encyclopédie Méthodique de Chymie et Métallurgie*, published in 1786. It is, of course, well known that the original "Chalybians, a people of iron-workers," mentioned by Herodotus in the fifth century B.C., lived in Armenia, on the shores of the Black Sea.

Sheffield's proximity to supplies of excellent stone or millstone-grit, from which grinding and other stones, so largely used in the city's industries, are obtained, has been of considerable advantage. These grindstones have been in demand in many other countries.

In the same way, the well-known Sheffield gannister and fire-clay of excellent refractory quality appear to have been placed by nature

just where they were likely to be wanted. In fact, Sir Henry Bessemer owed to this fact much of his early success. Sheffield gannister is still exported world-wide.

Many of the names given in the directory of 1774, as connected with steel and hardware, for example, Spencer, Jessop, Peace, Wilkinson, Parkin, Turner, Rodgers, Tyzack, Sanderson, Hobson, and others, are to-day still household words in the world's markets. A proud record for these our Hallamshire men, who, after a century in the troublesome world of business careers, still continue, with the assistance of their sturdy Yorkshire workmen, to keep the name of England to the fore, both as regards quality of material and excellence of handicraft. Their aim has been truly represented by the motto of the City's Cutlers' Company, founded in 1624: *Pour y parvenir à bonne foi.*

In 1835 there were 56 converting furnaces and 534 melting-holes; in 1842 there were 97 converting furnaces and 777 melting-holes; in 1846 there were 105 converting furnaces and 974 melting-holes.

In 1848 the ratable value was £272,000; in 1893, £1,250,000.

The small beginnings of a century or more ago have expanded into a commerce of very large proportions. There are now over 250 different steel-manufacturers and suppliers in the city; also nearly 1000 representing the cutlery and edge-tool trades, and those engaged in other miscellaneous branches of manufacture, such as saws, files, etc. In times of ordinary trade, probably more than 1000 tons of the *best quality* of crucible cast-steel are melted per week in Sheffield. Huntsman would indeed be startled to find his child grown into such a giant.

America has developed the fusion of cast-steel by gas-melting-furnaces and, with the kindly help of nature, in the wonderful application of natural gas. But without such aid, Sheffield now uses weekly some 14,000 clay crucibles in which to fuse its steel. The quality of this steel still stands unsurpassed in the markets of the world; and the main principles in the production of it are those brought into practice by Huntsman.

APPENDIX.

Report on Huntsman's Cast-Steel.

BY

Fourness & Ashworth (1792),

Engineers to their Royal Highnesses the Prince of Wales and Duke of Clarence.

To the Public.

In justice to Mr. Huntsman, who makes the best Cast Steel in this, or perhaps any other country, we wish to present Society at large, with the following brief character of it, *which*, as persons who have for several years been in the practice of using it, we shall at all times be ready to confirm. We have made trial of different kinds of Cast Steel, but never met with any that would abide the same execution as Huntsman's.

The efficient properties of Mr. Huntsman's Cast Steel, are simply two, *namely*, extreme hardness, combined with great toughness and ductility. A point may be made of it that will cut glass, and at the same time, endure arduous work, as a Turning Tool for any kind of metal, without undergoing those frequent repairs necessary to tools made of other Steel. It is calculated also to take the highest polish; therefore, for Burnishing Tools, and Plates to beat or roll any kind of metal to a fine surface upon, it possesses a decided superiority; and, as to Dies, there is perhaps no Steel that can be made into a face of equal hardness and durability. For Buckles, Buttons, and other articles of the steel kind, to which great superficial brilliancy is requisite, there is, we believe, not another fabric of Steel so completely adequate. Indeed, as a hint to *Opticians*, it is probable, this Steel would admit of a polish sufficient for Speculums; for Mirrors it is particularly suitable. By a judicious workman, a plate of this Steel can be *laid to*, and united firmly *with* any malleable Iron or Steel, of even an ordinary kind.

There are many Smiths, within the compass of *our* knowledge, who have not been able to find out the real qualities of this Steel, on account of having no previous instruction relative to the working of it.—It has often been said, and amongst other incorrect statements it has been asserted that the Huntsman Cast Steel could not be united or welded to any other Steel or Iron; but the opinion is a mistaken one, because we can satisfactorily prove to any person, that Mr. Huntsman's Cast Steel may be securely united or welded to *any other* Steel or forged Iron. To elucidate this fact is one part of the design of this testimonial.

When Smiths use Cast Steel, they frequently imagine, that it requires the same heat commonly given to other Steel before it comes under the hammer; nay, some indeed think it is necessary to give the same heat to it they would give to Iron; whereas, if instead of this erroneous method, Huntsman's Cast Steel were treated with care in the fire by the smith who works it, it might be brought into any required congenial state. It might, as already observed, be laid to any piece of wrought Iron or Steel. In fine, two pieces of the same Steel, at a proper welding heat, will firmly unite under the hammer together. Steel of so fine a texture as *it* is, cannot bear excessive heat, since excessive heat undoubtedly destroys one of the two of its virtual properties, we mean its toughness.

For the facing of Anvils and Hammers, and the making of cold Chissels, no other Steel we have been able to select can bear any competition with it. It may be tem-

pered to any degree of hardness, and again meliorated to any degree of mildness or ductility. Needles of all denominations, Fish-hooks of every kind, we are enabled to certify, may be much best relied on, when made of this Steel. Likewise (to be concise) Edged Tools of every description made of the same; Screw Taps and Plates, Drills and Boring Bits, Points of Tools for the digging of ores, coal, &c. most of which we have some acquaintance with, are more to be confided in than any other we have tried.

It may not be unnecessary to observe (since the idea of interestedness generally produces bias) that *we have no connection whatever with Mr. Huntsman*, nor is it at all at his request or instance, we lay before the public this sketch of the qualities of *his* Steel; for, we ingenuously believe, were the nature and properties of it *better and more universally* understood, mechanics in general would be benefitted (since to all mechanical workmen, safe and durable tools are of great importance) and the ingenuity and industry of the man who prepares it, by an extension of his sale, might be better rewarded. We are still the more induced to present to the public this impartial, but imperfect character of Huntsman's Steel, as we understand that during the course of more than thirty years of time devoted to the manufacturing of it, he has so much neglected his own interest and credit, as never to give the public, thro' any general or circular medium, any account whatever of his Steel. It was by accident that *we* learnt there *was* such Steel, after being much put about to get such as would make Tools to perform services which we could get no other Steel to stand. All the orders we have since given Mr. Huntsman by pattern, have been executed *according to pattern.*

Springs of all sorts, scroll as well as others, may in general be depended on when made of *this* Steel and suitably tempered; Huntsman, however, has suffered much by some manufacturers, or *perhaps agents* unfairly making use of his stamp, therefore it would be prudent for those who wish to make trial of the genuine article, to purchase it from traders of known integrity, and such as do business with Huntsman himself, as there will probably always be some who would not scruple to deal surreptitiously in an article of high character.

The whole of this information we offer to the public, as friends to a man who we think ought in an advanced stage of life, as well for his own gratification, as his family's prosperity and comfort, to be repaid by the increase of trade, for his expenditure of time, and his sedulity in contributing to the convenience of the mechanical part of society.

FOURNESS & ASHWORTH,

Engineers to their Royal Highnesses the Prince of Wales and Duke of Clarence.

March 28, 1792.

THE ALLOTROPISM OF GOLD.

BY HENRY LOUIS, LONDON, ENGLAND.

(Virginia Beach Meeting, February, 1894.)

It can scarcely be considered a matter of doubt, in the present state of our knowledge, that the existence of, at any rate, two well-marked allotropic modifications of gold can be recognized, namely

(α), the ordinary, yellow variety, and (β) the red, brown or purple, non-lustrous, amorphous variety.

There are, indeed, not wanting indications that still other allotropic forms may be capable of existing. It is, for instance, possible that the green colors of gold obtained under certain conditions, or that the black powder produced when the alloy of gold with potassium is decomposed by water, may represent further allotropic modifications, although this proposition is open to doubt. It can scarcely be pretended that the two first-named varieties have been absolutely isolated, yet it is, perhaps, quite permissible to speak of the ordinary and the amorphous modifications as having a proved existence.

Ordinary gold is sometimes found crystallized in nature, although never in a state of purity. When gold is melted and cooled slowly, its surface shows crystalline markings, and the fact that it is capable of crystallizing in the cubic system may be looked upon as established. When gold is produced by precipitation, the form which it assumes is dependent on the conditions of precipitation. G. Rose * says that gold precipitated by ferrous sulphate from very dilute solutions is so finely divided that no regular form can be recognized, but in more concentrated solutions the precipitate consists of minute cubes. When oxalic acid is used as a precipitant, the gold is coarser and forms octahedral crystals. J. Thomsen † has obtained similar results. Working with dilute and with highly dilute solutions, I have myself been quite unable to recognize any crystalline structure, even under the highest powers of the microscope; nor did there seem to be even any tendency of the particles to group themselves into arborescent forms, such as might indicate incipient crystallization. Precipitates from solutions containing between 0.0001 and 10 per cent. of gold gave no indications of crystallization, even when magnified 800 diameters.‡

Thomsen (*loc. cit.*) has also pointed out that the physical characters of precipitated gold differ according as it has been precipitated from solutions of its chloride or its bromide. He also found that these different forms possessed different degrees of thermic energy, and hence deduced a strong argument in favor of their being allotropic varieties.

The specific gravities of various forms of gold differ considerably. G. Rose (*loc. cit.*) found that fused gold had a density of 19.3336

* *Pogg. Annal.*, 1848, vol. lxxiii., p. 8.

† *Journ. Prakt. Chem.*, Neue Folge, vol. xiii., 1876, p. 348.

‡ Private communication to the author from Mr. F. E. Lott.

after it had been compressed in a coining-press, it being a little lower before this mechanical treatment. The density of precipitated gold thrown down by ferrous sulphate he found to vary from 19.5419 to 20.6882, the highest figures being obtained from extremely dilute solutions, the precipitate from which showed no trace of crystalline form; when precipitated by oxalic acid its specific gravity was 19.4791. When such amorphous gold was struck in the coining-press, its density became *reduced* to 18.0194. I have found that the density of gold left on dissolving out various metals alloyed with it, when the gold remains behind in a brown, amorphous, lustreless condition, varies between 20.3 and 19.5.*

It is only fair to notice that Rose did not ascribe the differences in the densities of the different forms of gold to allotropism, but has suggested another explanation, which is hardly, to my mind, a sufficient one. It is probably safe to assume that there are two modifications of gold—one a light one, of density 19.3 or thereabouts, and the other a heavy one, the density of which approaches 20.7—whilst various combinations of these extreme forms are capable of occurring.

In this connection the curious divergencies in the densities of specimens of native gold, from different localities but of about the same composition, may also be referred to; allotropism may, at any rate, be suggested as a possible explanation of them. There are thus sufficiently well-marked differences in physical characteristics to support the hypothesis of allotropism.

As regards chemical properties, Thomsen has also pointed out that when amorphous, pulverulent gold is acted on by chlorine or bromine, aurylic compounds (Au_2Cl_4 or Au_2Br_4) are produced; whereas, these same substances produce auric compounds ($AuCl_3$ or $AuBr_3$) with ordinary gold.

I have found another point of difference, of far greater practical importance, in the behavior of these modifications towards mercury. Ordinary gold, of course, amalgamates readily, as is well known. I have found that gold precipitated from highly dilute solutions by ferrous sulphate is not attacked at all by mercury when freshly precipitated, and only slightly after drying on an air-bath. Near the boiling-point of mercury, partial amalgamation took place, but it was by no means complete. Mercury containing a large amount of sodium amalgam was equally without effect on the dry gold, although it readily and completely amalgamated it when moist.

* "Note on Experiments of the Specific Gravity of Gold," *Trans.*, xxii., 117.

In these observations I seem to have been partially anticipated by Ludwig Knaffl* who, however, appeared to attach little importance to his observations. In a brief note on the preparation of certain amalgams of gold, he says:

"Gold precipitated by green vitriol or mercurous nitrate is not suitable for amalgamation, as it is too finely divided and always *floats on the surface of the mercury*† as a black powder, whether heated mercury be poured upon heated gold or *vice versa*. I examined this floating black powder, and found it to contain gold and mercury. . . . Gold precipitated either by means of arsenious acid or by boiling a solution of the chloride in amylic alcohol, when it separates out in small, lustrous octahedra, is best suited for amalgamation."

It may also be added that the purple of Cassius, which probably contains an allotropic modification of gold, is not attacked by mercury. The black pulverulent form of gold resulting from the decomposition of the potassium-gold alloy likewise resists amalgamation. On the other hand, the coherent gold-sponge left on dissolving out the alloying metal from a gold-alloy amalgamates pretty readily, as does also the coherent pale brown powder produced by precipitating with sulphurous acid a strong solution of auric chloride.

All forms of gold are converted into the ordinary yellow, lustrous variety by the action of heat. A very high temperature is not required, but the exact point has not yet been determined; it is certainly well above 200° C., but probably under 600° C.‡ Powerful mechanical action, such as percussion, friction or compression, has the same effect.

I do not pretend that the above data form anything like a complete chain of evidence proving irrefragably the allotropism of gold, or that our knowledge of this subject is precise or definite; yet I venture to think that the facts do warrant us in looking upon the following deductions as probably correct:

1. Gold is capable of existing in allotropic modifications.

2. One of these modifications is capable of amalgamation only with great difficulty, if at all.

3. This modification is capable of being produced and of subsisting under conditions that may reasonably be supposed to exist in nature when gold is deposited in reefs.

Whatever may have been the nature of the solution by means of which gold has been introduced into the deposits in which we find

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* *Dingl. Polyt. Journ.*, 1863, vol. clxviii., p. 282.

† The italics are mine.—H. L.

‡ I am at present engaged in investigating this point, and hope to publish shortly the result of my research.—H. L.

it, whether as a soluble haloid salt, as is generally supposed, or as an alkaline aurate, as I venture to suggest,* it must have been precipitated from such solution in various ways and under varying conditions. We have but few indications of the cause of this precipitation, but it is reasonable to conjecture that such reagents as ferrous sulphate or sulphurous acid, both resulting, perhaps, from the slow oxidation of iron pyrites, may have found their way, in solution, into the fissures within which the gold solution was circulating, and may thus have caused the deposition of gold within the reefs. Now, if the gold, thus deposited from highly dilute solutions, happened never to be exposed to a particularly high temperature, or to violent mechanical action, the conditions would be favorable to the production of that allotropic modification of gold which is indifferent to the action of mercury. In other words, under the above conditions, an auriferous deposit will have been produced in which a greater or smaller part, or perhaps even the whole, of the gold is what gold-miners term "rusty." I have little doubt that the "rustiness" of gold is in different cases due to widely-different causes—that, in fact, there is more than one kind of "rustiness;" but I venture to think, also, that there is sufficient evidence to warrant us in classing allotropism amongst such causes of "rustiness." If this is correct, I need hardly point out either the great practical value or the application of this deduction. The gold which is thus allotropically indifferent to mercury is in a condition in which it is readily attacked by such reagents as chlorine and potassic cyanide. I have pointed out long ago that the gold of the Witwatersrand deposits of the Transvaal was probably deposited *in situ* under some such conditions as I have sketched above, and it is now notorious that a large proportion of the gold in them is not attacked by mercury, but readily by potassic cyanide solution. Again, in some cases it may be economically feasible to convert the non-amalgamable modification of gold into the common amalgamable variety by heating the ore to a moderate temperature, or the same end may be attained by mechanical means. In any case, the only really sound method of preventing losses of gold in the process of gold extraction is that of ascertaining, in the first place, the ultimate causes of such loss; and I venture to hope that it will be found that amongst such causes, the one here treated of—namely, allotropism of gold—will be found worthy of more consideration than it has hitherto received from scientific gold-miners.

* "On the Mode of Occurrence of Gold."—*Mineralogical Mag.*, vol. x., No. 47, p. 241.

THE ZINC-ORE-DEPOSITS OF SOUTHWESTERN NEW MEXICO.

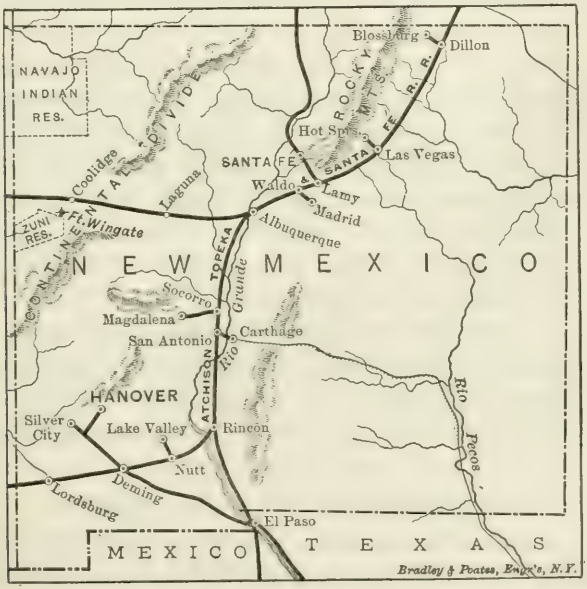
BY WILLIAM P. BLAKE, MILL ROCK, NEW HAVEN, CONN.

(Virginia Beach Meeting, February, 1894.)

Geographical Position.

In directing attention to the newly-opened zinc-ore region in Southwestern New Mexico, I adopt a suggestion made at the Engineering Congress last summer in Chicago by Prof. Le Neve Foster, who commended the indication of the geographical position of a district under description by means of an outline-map. Such a map

Fig. 1.



is the more important in this instance, inasmuch as the region to be described is remote and little known even in the United States. Until within a few years it has been infested by the savage Apaches, has not been often visited by mining engineers, and could not be

examined with safety. It is not far from the old stage-road to Silver City, but it is now reached by the Silver City and Northern Railroad, a branch line from the Silver City branch of the southern transcontinental road. It is between Silver City and Lake Valley, and not far from Fort Bayard and Georgetown, or from the famous copper-mines of Santa Rita. The railway terminates at Hanover, in the gulch of the same name. This is the shipping-point for both the zinc-ores and iron-ores of that district.

Geology.

It is a region of gray and white Palæozoic limestones, probably Lower Carboniferous and older, rarely showing any traces of organic remains, and resting, at low angles of inclination, upon a foundation of light-colored crystalline rocks of Archæan age. These ancient rocks are granitic, but have so much parallelism of structure in the arrangement of their component minerals as to suggest at once a mechanical origin, though by subsequent metamorphosing agencies they have become dense and solid, and, being hard and feldspathic, rather than micaceous, the rock cannot be described as gneiss. The structural planes are nearly vertical and trend northwesterly. Mineralogically, the rock is characterized by soda-feldspar, amphibole and an abundance of epidote of a light pistachio-green color, generally massive, rarely crystalline, and often associated with grains of magnetite. The abundance of this epidote, distributed over the hills and mingled with bowlders of iron-ore, is very striking and unusual. It may also occur in the limestones, especially at the contacts with intrusive dikes, as garnets are found.

The best exposures of the foundation-rocks are along the ravines, notably at Hanover, at the end of the railway. The rocks of the hills are generally limestones or dolomites, and both these and the granitic rocks are traversed by dikes of intrusive porphyry.

Beds of Smithsonite and Calamine.

The abundance of the massive iron-ore first led to the exploration of the region and to the construction of the railway. Copper-ores also claimed attention, and at last the oxidized zinc-ores were mined, and by reason of their exceptionally good quality and high grade, found a ready sale to the works in Wisconsin for the production of the white oxide of zinc. These ores comprised the carbonate (smithsonite), with some calamine, and were at first quarried at the surface,

and then followed downward in irregular pits and cave-like excavations, in some instances to a depth of 60 feet or more.

The ore occurs in irregularly concentric crusts or layers, or in cavernous masses made up of small layers or concretionary sheets of pure carbonate-ore, nearly white and cream-colored, but in some places colored gray, or even black, by manganese oxide. Crystals are rare. The ore is sometimes in close association with aggregations of small quartz-crystals, the presence of which reduces the percentage of zinc—and, consequently, the commercial value—so much as to prevent profitable shipment. The best carbonate-ores, assayed by the car-load, contain 35 to 38 per cent. metallic zinc.

The excavations upon these carbonate-ores show that they occupy cavernous spaces in the limestone strata, and irregular openings between the beds, gradually thinning out to mere seams. Considerable quantities are also found in the superficial soil, clays and ochers, derived, doubtless, in great part, from the corroded limestones and from the parent-sources of the zinc-ore. It is evident from a study of the phenomena, that the limestones have been corroded by zinc-bearing solutions, and that the zinc-carbonates replace the dissolved rock.

The sources of such zinc-solutions are in many cases obscure, but in other cases very plain. As a general rule, the largest and richest deposits of smithsonite give no direct indication of their origin. They do not have any obvious connection with any source or other ore, the original ore having undergone complete decomposition and recomposition.

Deposits of Zinc-Blende or "Black-Jack."

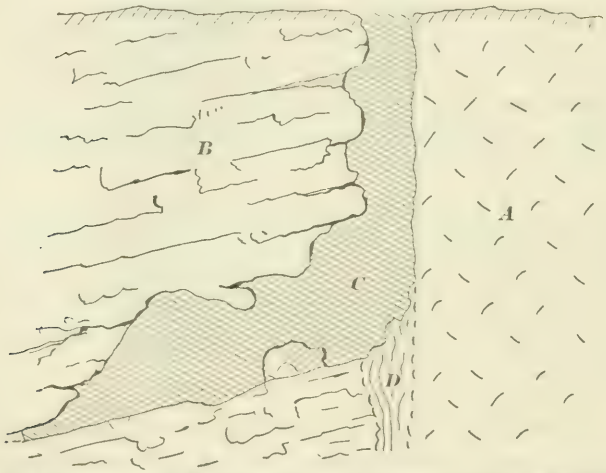
There is no reason to doubt that the carbonated ores existed originally as the sulphide of zinc (blende, black-jack or sphalerite of the mineralogist). This occurs in the same region in beds, in masses, and disseminated in other minerals, notably at the claims known as the Black Wolf, the Zinc Valley and the Thunderbolt, where excavations have revealed large deposits of black-jack, none of them directly associated with the carbonate-ore.

This blende is the dark reddish-brown variety, but is rich in zinc and free from arsenic and antimony. It does not appear to be highly argentiferous, and is an excellent ore for making spelter. It is granular-massive, not found in crystals, and exists in beds sometimes 20 feet or more in thickness. It is usually intermingled with iron pyrites in grains and bodies of irregular shape dispersed through

the mass; but close inspection shows that there is a rude structural distribution, in layers parallel with the sides of the bed or mass of ore. Some parts of the ore-bed yield clean massive jack, but the larger part is mixed with pyrite.

Any associated smithsonite is confined to the surface, or near to it, or to one side of the zinc-blende, and is not intermingled, being evidently in all cases a secondary product, resulting from the oxidation of the sulphide to sulphate, the solution of which, percolating to the adjacent limestone or dolomite, is immediately decomposed, with the formation of zinc carbonate and sulphate of lime and sulphate of magnesia, which flow away. The zinc carbonate thus replaces the lime and magnesia carbonate, and the extent or magnitude of

Fig. 2



A, Dike of Plutonic rock; B, Metamorphosed limestone strata; C, Cavernous excavation left by the extraction of zinc-ore; D, Blende and amphibole, "Green rock."

the deposit of zinc carbonate depends first upon the magnitude of the original bed of blende, and, second, upon the extent to which it has suffered decomposition.

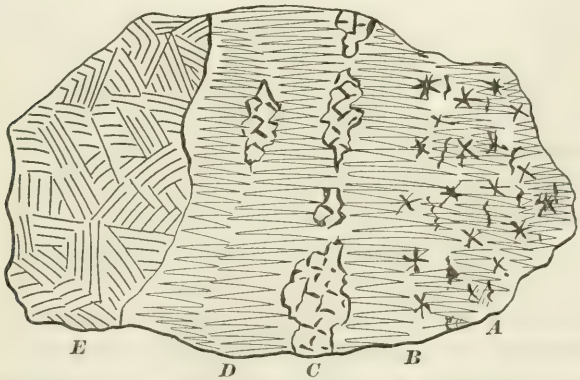
Fig. 2, a sketch-section of one of the openings, will serve to illustrate in a general way the form of the deposits, both of the blende and of the oxidized ore derived from it.

Descending a shaft about 35 feet deep, a hard wall of what appears to be a dike of intrusive rock is found on one side, and on the other, a softer, altered rock, apparently limestone colored by iron

oxide and corroded. At the bottom, an inclined floor of limestone leads to an irregular cavernous space from which about fifteen carloads of excellent smithsonite ("bone") and calamine have been taken. Some ore remains, running off in seams and sheets between the layers of limestone, and evidently thinning out as the distance from the source increases.

At the side of the excavation next to the dike, zinc-blende is found in patches and lenticular bunches, 3 to 4 feet thick. These are enclosed in an excessively hard and tough rock, locally known as "green rock," which is very difficult to break. It consists of fibrous amphibole, with garnet and probably epidote, and in places carries hematite iron-ore. The garnets are massive and amber-yellow, and sometimes show the faces of crystals. There is also some calcite. The whole is evidently the result of metamorphic action, doubtless in connection with the dike, the precise relation of which to the zinc-blende and its associated minerals could not be determined for want of sufficient exploration of the ground. The thickness of the dike is not known.

Fig. 3



E, Zinc-blende; D and B, Garnet; C, Pyrite; A, Hematite and Blende.

We have in this primary ore a combination of amphibole, garnet, epidote, specular iron, zinc-blende and pyrite. These minerals are disposed more or less in irregular layers, following in a general way the direction of the walls or sides of the bed. This structure gives the ore the appearance of a segregation. A hand-specimen presents the arrangement of minerals shown in the sketch, Fig. 3.

At a second opening upon the same claim an irregular chamber, 40 to 50 feet across, is excavated in similar zinc-ore. The walls are massive ore, and the full extent of the mass has not been determined.

There are, however, indications of the dike at one side, and, on the surface above, this dike appears to be about 40 feet wide. The full extent of this large bed of zinc-ore and its exact relation to the dike are not fully shown; but it appears to be an irregular lenticular mass, partially bedded in the limestone strata, and containing more of the garnet and actinolite on the side contiguous to the dike than upon the side next to the limestone beds.

The form of the deposits of zinc-blende seen at other places is generally lenticular; but the deposits being mostly, if not in all cases, along the planes of contact of intrusive dikes, or following the planes of faults, they have linear extension and sequence, so as to present the general appearance of lodes. This is especially well shown in the two contiguous claims known as the Black Wolf and the Zinc Valley, where a series of bunches of zinc-blende is found in one line for nearly the whole length of the two claims. The deposits may be classed as contact-deposits, or segregations following the dikes or the planes of metamorphism. The lenticular form is best displayed in the smaller bunches of ore; and these may be taken as typical of the form of the larger deposits. The form of the zinc-blende deposits, however, has not been much shown by explorations, very little having yet been done to develop the zinc-blende. For this reason it is not yet possible to delineate with certainty the exact relation of the deposits to the wall-rocks. So also the relation of the massive garnet to the limestones has not been clearly shown. If we regard the massive garnet and the massive actinolite as portions of the ore-deposit (as it seems we should do), the magnitude of the aggregations is much increased. At one end of the Zinc Valley claim the garnet-rock crops out in high cliffs, and has formed a heavy talus of blocks, from a few inches to many feet in diameter. This garnet-rock is composed almost wholly of garnets of a light color, generally wax-yellow, of the variety grossularite, and having a faint resemblance in color to the lighter resinous-looking varieties of zinc-blende, technically known as "resin-jack." It was for a time supposed to be a form of zinc-blende, a mistake which derived some support from the fact that tests of some of this garnet-rock proved the presence of a notable quantity of zinc-oxide. An examination shows that zinc-blende is closely associated with it, the blende being distributed in grains and layers, and sometimes in bunches of considerable size. It is similarly distributed in the actinolite. This actinolite is the light-green variety of amphibole, in radial coarsely fibrous masses, the divergent prisms closely aggre-

gated, and often two or three inches long. At other places it is a mass of closely interwoven fibers, making an exceedingly tough rock, but holding minute and sometimes almost microscopic grains of zinc-blende in its mass. This is true not only of the fibrous varieties, but of the coarsely radial masses, in which grains of sphalerite are distributed freely between the bladed crystals. These grains vary in size from a tenth of a millimeter or less to over a millimeter in diameter, being generally about the size of mustard-seed.

The zinc-blende, the actinolite and the garnet-rocks appear to have been formed contemporaneously, to be idiogenous, and to have been a part of the original magma from which these minerals were segregated, or deposited. Galenite, the usual associate of zinc-blende, appears to be generally absent from these deposits, except in very small quantity, though it occurs in workable quantity in the same district. The presence of manganese oxide in some of the secondary ores—the smithsonite and calamine—indicates its existence in the sphalerite.

The Deposits of New Mexico and New Jersey Compared.

This remarkable association of large beds of zinc-blende with pyrite, hematite, actinolite and grossularite in lenticular layers, and in disseminated particles in the substance of the actinolite and the garnet-rock, forming great contact-aggregations or segregated beds in limestone, appears to be unique. The literature of ore-deposits does not, to my knowledge, afford another example. The Åmmeberg (Sweden) zinc-blende deposits in granitic gneiss appear to have some resemblance to the New Mexican in this respect, that the zinc-blende has the granite minerals, amphibole, talc, chlorite, garnet and black tourmaline as associates. It is, however, without iron pyrites or iron ore, and is associated with galena.* Green garnet also occurs at Sala, in Sweden, with calc-spar, galena and zinc-blende.†

We have numerous examples of the association of garnet with magnetite and pyrite (notably with copper pyrites at the Nacosari mines in Sonora, Mexico, and in the Dragoon mountains of Arizona), but not with massive zinc-blende. We cannot, however, fail to find a striking analogy between these zinc-deposits of New Mexico and those of Sussex county, New Jersey, in that both are in connection

* Stapff, *Berg. und Hüttenm. Zeit.*, 1861, p. 252. Cited by Phillips, *Ore-Deposits*, p. 393.

† Bredberg, *Schweigger's Jour.*, xxxviii., 11. Cited by Bischof, *Geology*, ii., p. 285, Paul's Trans. Cavendish Society.

with ancient white crystalline limestone; that in both localities the component minerals are arranged in layers parallel to the walls; and that similar silicates, garnets and amphibole, abound in both places. The zinc- and iron-ores in New Jersey are in the condition of oxides and silicates; but in New Mexico they exist as sulphides, excepting the hematite. We can easily imagine that by some difference of temperature of solutions and oxidizing influences at the time of the consolidation of the beds in New Mexico, the zinc-ore and the pyrites might have lost their sulphur, and have been left in the condition of the New Jersey ores. In both regions we find the zinc-ores in juxtaposition with beds of magnetite and hematite. Both regions have underlying granitic rocks, and both are ferriferous and zinciferous. In New Jersey the oxidized ores abound, to the exclusion of the sulphides; and in New Mexico the sulphides abound to the exclusion of the oxides. Yet in New Jersey, further south, in the same limestones which carry the zincite and willemite, we find deposits of zinc-blende, which suggests a common origin of the zinc-impregnations, though finally formed under different conditions.

In the composition of the New Jersey ores manganese plays an important part, while in the New Mexican ores it is not so abundant, but exists in the amphibole, and to some extent in the iron-ores, and is found in the zinc carbonate proceeding from decomposition of the blende.

If the great masses of blende and pyrite in New Mexico could be desulphurized in place, we should have beds of oxide of zinc and oxide of iron, more or less intermingled, so as to exhibit much the same composition as we see in the zinc-deposits of Sussex county. The mechanical mixture of zinc-blende and actinolite, and zinc-blende and garnet, might also give us by alteration, a zinc silicate or willemite or zinciferous pyroxene, like jeffersonite, which is abundant in the New Jersey beds contiguous to intrusive dikes.

Metallurgical and Commercial.

From a commercial and metallurgical point of view, the New Mexican ores present great difficulties in the way of immediate utilization: first, in the matter of transportation to any of the established centers of production of spelter or of oxide; and second, in the objectionable presence, in at least a part of the ore, of the heavy garnet from which the blende cannot be mechanically separated. And in the portions of the beds free from garnet the pyrite impairs

the value of the zinc-blende for making either oxide or spelter. The pyrite can, however, be removed by the process I have before described,* but at present a supply of fuel cannot be obtained at figures sufficiently low to justify the undertaking. When coal can be obtained at a low price, either at Hanover or at the southern terminus of the railway, the higher grades of the mixed ore of blende and garnet, and of blende and actinolite, can be worked by either a preliminary conversion into a crude zinc oxide or by lixiviation, after proper roasting. The conversion of the smithsonite and the calamine into crude oxide is also desirable, to minimize the cost of transportation, and to work the lower-grade ores which cannot now be profitably sent away. The higher grades of smithsonite, carrying 35 per cent. of zinc and over, pay a fair profit when they command over $20 per ton, but are sent out at a loss at lower figures, the necessary cost of raising the second quality of ore being counted. The freight alone, aside from the cost of carting the ore to the railroad at Hanover, is about $12 per ton to Waukegan or La Salle, Illinois, or to Mineral Point, Wisconsin. The secondary ores at or near the surface are cheaply mined, and there is but little waste. Expensive hoisting works are not required; simple windlasses suffice; there is no water to contend with, and the claim-owners are generally content if they can realize wages by mining and shipping the ore.

The shipments of zinc-ore by rail from Hanover up to January, 1894, amounted to 1358 tons of 2000 pounds.

The greater portion has been sent to the oxide-works at Mineral Point, Wisconsin; a few tons have been shipped to Waukegan, Illinois. The financial depression of 1893 and the extremely low prices offered for zinc-ore have greatly repressed mining, and many of the zinc-claims have not been worked lately.

The district also furnished 22,285 tons of iron-ore from November 1, 1891, to April 1, 1893.

MINE-EXPLOSIONS GENERATED BY GRAHAMITE-DUST.

BY WILLIAM GLENN,† BALTIMORE, MD.

(Virginia Beach Meeting, February, 1894.)

THE Ritchie grahamite-mines of Ritchie county, West Virginia, were situated near the central part of the upper barren coal-measures

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* *Trans.* xxii., 569.

† The observations herein set forth were studied and discussed by the late P. G.

of the Appalachian coal-field. The rocks of the region are shales and sandstones, which lie almost horizontal. They show no evidence whatever of containing carbonaceous ingredients, except that they enclose, at long intervals, thin veins of exceedingly impure coal.

The vein of grahamite is a straight and vertical fissure, which cuts downward across the horizontal strata of the rocks mentioned. It will be sufficient here to state that the fissure is about 4 feet wide, and that it was compactly and completely filled with the asphalt-like mineral first described by Prof. Leslie in 1863.* He regarded it as a mineral pitch or inspissated petroleum, which he called asphaltum. The name grahamite was proposed in 1865 by H. Wurtz,† who more fully considered and described the mineral. Both these writers, together with Prof. Blake,‡ who studied the body in 1890, maintain that it is a form of asphalt.

An accurate and comparatively full study of the mine was made in 1873 by Prof. W. M. Fontaine, of the University of Virginia, who published his observations under the title, "Notes on the West Virginia Asphaltum Deposit,"§ thus further maintaining that grahamite is an asphalt.

Fig. 1 represents that part of the mine first to be considered. As no maps can now be had, the writer has been compelled to construct the sketch in part from material found in his letter copy-book and in part from memory; yet, so far as concerns the present purpose, it may be regarded as accurate. The figure represents a vertical section of the mine, and is in effect a view of the vertical vein with one side-wall removed, showing all the workings therein existing at the date presently to be mentioned.

The levels numbered 2, 4, and 6 represent workings made for removal of vein-matter when it was supposed that the proper way to mine the material was by means of a succession of such levels. The rooms lettered A, B, were the initial rooms, constructed when it was determined to mine by a method called by miners "standing breasts." In this method the miner stands upon the material he already has broken down, and attacks in turn that above his head. As all rocks occupy increased space when broken into smaller

Sauerwein, of Baltimore, who was the president of the Ritchie Company, together with the author, who was the manager of that company's mines and railway. As the more learned and able one of us cannot take part in this paper, the author alone must be held responsible.

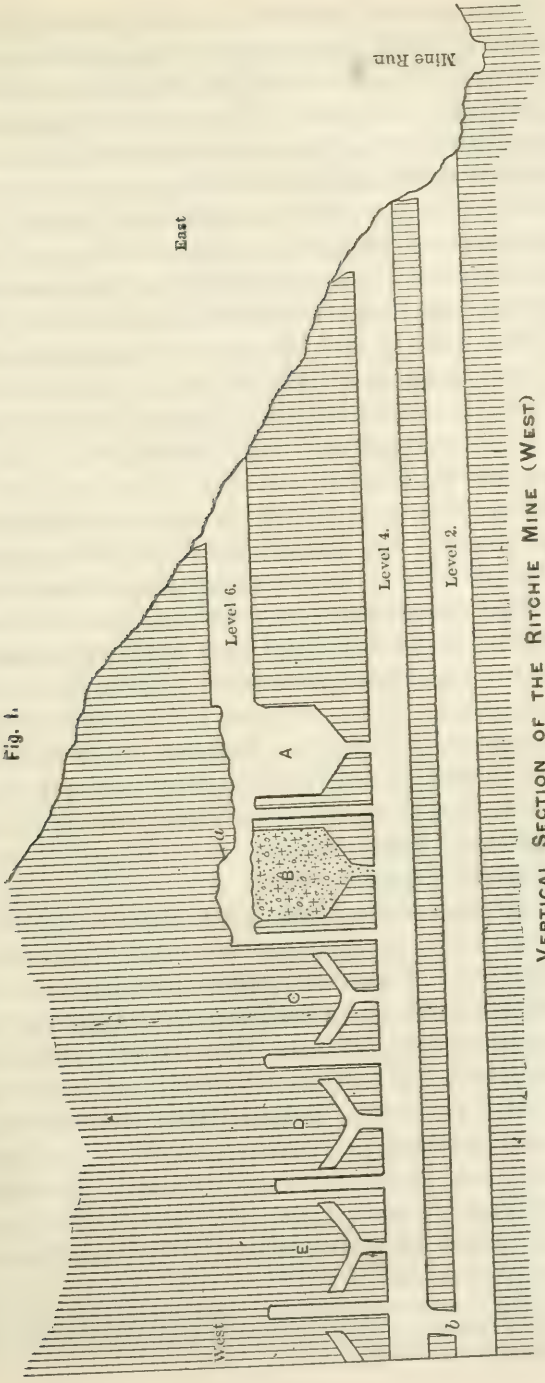
* *Proc. Am. Phil. Soc.*, ix., 185.

† *Am. J. Sci.*, 1866, ii., xlii., 420; *Proc. A. A. A. Sci.*, xviii., 124.

‡ *Trans.*, xviii., 563.

§ *Am. J. Sci.*, iii., vi., 409, 1873.

Fig. 1.



VERTICAL SECTION OF THE RITCHIE MINE (WEST)
AS IT EXISTED FEBRUARY 9TH., 1871.
Vertical Scale, $\frac{1}{375}$; Horizontal Scale, $\frac{1}{750}$

masses, it will be seen that the miner at work must soon nearly fill the space between the vein-matter over his head and that which he has mined already. To prevent this, the latter is removed at proper times and in necessary quantities. When a room is mined upward to its extreme height, then all its contents may be withdrawn. The details of the process do not pertain to this paper.

In the west mine (Fig. 1), on February 9, 1871, room A had been completed and the material had been removed from it. Also, the communication between its bottom and level 4 had been closed, so that air could not pass from the level through the room. Room B was being mined, and had attained about the condition indicated in the figure. Mining consisted simply in digging down the soft vein-matter by means of the exceedingly light pick used by bituminous coal-miners in Pennsylvania and West Virginia.

The contractors for room B, believing they could blast out the grahamite cheaper than they could dig it, received permission to try the effects of powder. But the attempt failed, either because the charge was insignificant or because the powder failed to explode. The Dupont mine-powder used in this hole was contained in a paper cylinder $1\frac{1}{4}$ inches in diameter, in which it occupied 3 inches height. Two days after, a second hole was prepared and charged with a similar cylinder containing 6 inches depth of powder. The position of this blast is shown at *a*, Fig. 1. The shot was fired at 3 P.M. of the date given, and immediately there occurred what was apparently a mine-explosion; such a disaster as is known among miners as a fire-damp explosion. For the size of the workings, it would have been judged unusually severe.

The first effect observed was, that so much of the pit-head structure (not shown in Fig. 1) as lay near the prolongation of level 4 had been demolished. A man who had been standing at that moment on the bridge leading from level 4 to the storage-bins had disappeared, together with his mine-car. The latter had been driven almost horizontally for 90 feet, and there had been wrecked against the east hillside of the ravine of Mine Run. Even the cast-iron car-wheels had been broken by impact against soft earth, while the wooden car-body was little more than splinters. The man was driven 60 feet, when, by impact against a heavy tool-chest, he received injuries almost immediately fatal. "He was burned and blackened past recognition."*

* Report of the manager upon this explosion. It was written February 10th, and contained about four thousand words.

The effect was as if level 4 had been a great gun, out of which the man and car had been projected. So much of timber-structures as lay near the line of fire had been swept away.

Before those near the pit-head had recovered from consternation, one of the men belonging in room B appeared at the portal of level 4. He was seriously burned, and could tell no connected story. Upon his clothing and on his bare arms there adhered more or less of what seemed half-burned coke, and some of this material was still aflame. His burns were fatal. His brother and partner was found under room D, or nearly so, in level 4. This man survived, and was subsequently able to give a clear account of the incident up to the moment of his injuries.

The third and only remaining man in this level was 140 feet inside of room B, and was engaged in constructing the initial work of a room, such as is indicated at C, D, E, Fig. 1. When the explosion occurred, he came down into the level and walked along it to the open air. He had seen the reflection of a flame in the level, was aware of what had occurred, but was in no wise injured.

Two other men were driving the end of level 2, then 640 feet long. They had heard the sound of the explosion; their lamp-flames had been nearly extinguished by an air-wave; but otherwise they had nothing to relate.

The ravine of Mine Run, less than 100 feet wide, contained all the pit-head structures. Upon so much of them as lay near the prolongation of level 4, and upon the east hillside, there adhered a coating of coke. This was not only peculiar but was striking and prominent. "On all parts of the old storehouse left standing, and upon the hillside near the portal of level 3, there remains adhering about a $\frac{1}{4}$-inch thick of cinder such as our mineral makes. The fire seems to have melted the mineral, thrown it from level 4 towards level 3, and left it a cinder sticking to every opposing thing. To use again the illustration of the cannon: Imagine it (level 4) to have been loaded with melted mineral with which its discharge coated all opposing objects; then imagine the mineral to have charred after sticking fast."* It may be well to mention that grahamite is plastic while hot; upon cooling it again solidifies.

In comparison with what was to be observed about the pit-head, relatively small quantities of the half-coked grahamite were found adhering to objects within the mine. The coarse-grained, whitish sandstone forming the two walls of the mine was quite free of it, ex-

* Report, already cited.

cept at two points, namely, about the top part of the airway next west of room B, and about the portal of level 4, where the walls were much blackened. The occurrence of this material within the mine was noticeable chiefly because of its exceeding peculiarity, and not because of its abundance.

We supposed that little vein-matter had been burned within the mine, because of lack of air there, and that the abundance of dust in level 4 had been swept along it to the portal, and there heated to the plastic state and thence projected by the explosion. As the material had been thrown forward along the projected axis of level 4, and not laterally as well, we were led to suppose that the explosion had occurred just without the portal, where the restraining side-walls were without a roof-covering.

The evidences were that the disturbance initiated in room B by the blast was propagated downward along the air-way next west of it, as shown by the coked vein-matter adhering along the air-way, chiefly about its upper part, as already stated. Having reached level 4 it extended itself westward along the level at least 30 feet, as proved by the severe burns received, as above narrated, by the two men there. But it did not extend 140 feet in that direction, because the flame was not evident to the miner working above and in plain view of the level there. He saw only the reflection of a light in the level, and heard the sound of an explosion. The principal extension of ignition was eastward, outward to the portal of level 4 where that explosion occurred.

We were not able to find evidences of any ignition or of violent force in level 2, which might have occurred because of the air-way *b*, Fig. 1, between it and the level next above. The two men working at its end were conscious of nothing beyond the sound of a violent explosion and of an air-wave which nearly extinguished the flames of their open lights.

Nor could we learn of a commotion in any part of level 6; it exhibited no evidences of heat. Room A exhibited blackened walls at top, at least; but otherwise there was detected in it nothing unusual.

The impression entertained first and finally was, that gunpowder had been responsible for the accident. It was for this reason that its agency was promptly examined into. The magazine, a primitive structure, had been erected 1400 feet from the pit-head, and it stood intact after the explosion. The guardian of it was able to state that all the powder in or near the mine at that time had been contained

in a tin tomato can which he described. We recovered this, and by his aid we determined that $18\frac{1}{2}$ cubic inches of powder had disappeared out of it. The quantity was ridiculous as compared to the results we had witnessed. The report already cited contains this passage: "I have said to the men that the burning of the powder generated an explosive gas, which was driven out, mixed with air, and ignited." A supplementary report further stated in explanation: "Explosion of the powder pulverized a certain quantity of mineral, and in that state it was easiest decomposed. The mineral lying in the room B was slack, and every adjacent wall contained dust to be acted upon. The indications are that gas burned along all the air-passages and exploded at the portal."

The Ritchie mine above water-level was dry beyond all mines of which the writer has any knowledge. The only water which entered the part now under consideration came after heavy rains through the roof of room A and through the adjoining part of the roof of room B. It was, in fact, surface-water, which found its way through the partly decomposed grahamite forming the backs of those workings.

It is also important to recall that the vein-matter was soft and friable, much reduced to dust in mining; and that it was pulverized in the levels by the traffic through them. Within the mine, all surfaces were abundantly coated with its dust, and the floors of the levels contained more than an inch in depth of it; and, of course, room B contained it in quantity, because it was nearly filled with the already-mined vein-matter.

It may be said at once that so far we had not observed any indications of fire-damp (chiefly CH_4) within the mines. To quote once more from the report: "No fire-damp or choke-damp (CO_2) ever was detected in these mines, even when there was no ventilation; and the explosion began where ventilation was excellent. We have a level (No. 2) 600 feet long and no ventilation for it." An inspection of Fig. 1 will disclose that the air-currents flowed in through levels 2 and 4, then up the air-way next west of room B, then across both rooms and onward to and out of level 6. As February 9, 1871, was comparatively a cold day, ventilation then was all that could be desired. Moreover, we could not suppose that so light a gas as fire-damp could lie in room B, when at times water dripped from the natural surface into the east end of it, as well as into room A. Fissures which can convey water must readily permit the passage of a gas. We supposed that if fire-damp were present at all,

necessarily it must have found lodgement in the small workings above the roof of level 4. These were ideal receptacles for it, and yet we had not found it there; we had not observed even so much as the lengthening of the naked lamp-flame of the miner who constructed them, or of that of the writer who visited them frequently. The tight end of level 2 was another good receptacle for the collection of fire-damp. If such had been present, we supposed it must have exhibited its presence any morning, after the quietude of a night in which to collect. But it had not done so.

Anticipating a little, it may here be said that fire-damp was first encountered in an inclined traffic-road (mine-slope) sunk subsequently, from the narrow ravine of Mine Run and under level 2. At about six fathoms vertically under water-level a blower was disclosed in the south wall of the slope. The gas was promptly ignited; but it soon ceased to burn. While the lower levels disclosed other gas-blowers, none of them were serious, and the gas was readily disposed of. It never became necessary to use safety-lamps.

It was rather a curious fact that gas was never observed to issue out of the vein-matter. Apparently, that was so compacted into the fissure that, practically, gas could not circulate through it.

In these damp lower levels, shots were frequently put into the side-walls because of the necessity of widening the roads. And even though fire-damp at times must have been present in some degree, yet no disaster ever followed.

Among the many surprises which grahamite offered, none were so striking as the peculiarity of combustion of the mineral and of its dust. When warmed over the flame of an open light, the mineral grew viscous, and then might be drawn out into a thread. Warmed yet more, it kindled into a dull and smoky flame, which burned until the mass became a smutty coke of slight tenacity. Dust which fell from one's hand about the flame of the lamp, created a halo of scintillations around the light. Yet more widely diffused and brilliant effects at times followed the falling of dust from one's clothing upon a flame.

Vein-matter mined in the rooms was permitted to slide from them into cars standing in the level below. More or less dense dust-clouds necessarily followed; and when one of these clouds was sufficiently dense, if it then enveloped an open light, a flash followed. Several men were in this way burned; but none seriously. Such flashes always produced watery blisters upon their victims. Glass lanterns offered a remedy; but dust soon coated the glasses. Following the

invariable ways of unrestrained miners, these car-loaders risked the danger of open lights. These they would place at a distance greater, as they supposed, than the dust-cloud would roll, and they would then open the chute and permit the grahamite to descend, almost flowing like water, from the room into the car below.

A few weeks after the explosion of February 9th, the writer stood near the car-loaders' lamps when a car was being loaded in level 4. He saw the dust-cloud extending unpleasantly near to the lamps, which stood upon the floor of the level, and observed it to roll onward until it reached a lamp full 30 feet distant from the column of falling grahamite. Immediately a flash followed, so brilliant and complete that one might have judged it due to gunpowder-dust disseminated through the air.

An unsuccessful attempt was made to recover the details of a grahamite dust-flash which occurred in the waters of New York harbor, perhaps in 1871. While a cargo was being discharged, a workman in the hold of the barge attempted to light his pipe by means of a match. Agitation of the grahamite had afforded the sufficiently dense dust-cloud, and a flash resulted. The flash was reported to be unusually vigorous; or more likely it was so regarded because the observers were unused to such occurrences.

The reader is now in possession of all information known to us as bearing upon the explosion of February 9th, and almost necessarily he must anticipate our conclusion as to its cause.

We are compelled to assume that the blast pulverized, and immediately decomposed into coke and inflammable gas, a great deal of the dry vein-matter; these assumptions are imperative. The remainder follows easily from what is known of the behavior of mixtures of marsh-gas and air.

The first effect was a burning of the air in room B, as proved by blackening of the side-walls and the adhesion to them of coke formed from the dust which had lain upon them. Expansion of heated gases could occur in two directions. Toward the open air there was but little fuel to feed the flame, and it ceased in level 6, because no dry dust was found there. Inward, every surface supplied its store of dust whereby ignition was led downward through the airway next west from room B. Once in level 4, the dust so abundant there was freely converted to gases which burned vigorously. After reaching and passing the two men 30 feet west of the airway, there was a cessation. We supposed it was due to resistance of the air-cushion offered in that direction to the widely expanding gases. The line of

least resistance was toward the open air, in which direction ignition was propagated to the portal of the level. The hot gases there encountered that abundance of air necessary to form with them mixtures which were explosive, and the result has been stated.

No explosion occurred within the mine, because the requisite volumes of air and gas were nowhere present. That is to say, there was at no point as much as eight volumes of air to one volume of explosive gas, if each had been measured at the same temperature.

Coke found within the mine was, of course, a product of destructive distillation, and it was the best of evidence as to what had occurred. Moreover, its abundance about the pit-head demonstrated that a great deal of grahamite-dust had been swept along the level to the open air; in a hot state it had been projected thence upon all opposing objects.

The Executive Committee of the Company's directory, of whom Mr. Enoch Pratt alone survives, attempted to find a remedy against the future occurrence of dust-explosions. They had the advantage of good advisers, among them, Dr. D. K. Tuttle, a chemist, now of the United States Mint, at Philadelphia. But they could suggest nothing better than to dampen the dust by means of small jets of water thrown from pipes secured within the mine, a method suggested by the fire-protection pipes in the cotton-duck mills of Mr. William E. Hooper, at Woodberry, near Baltimore.

The precaution actually taken was to put no more blasts in the vein-matter, and to fire none in the side-walls, except when the mine contained no men. Even these precautions at length failed; the east mine suffered an explosion which much injured it, and in connection with which four men assumed risks which cost them their lives.

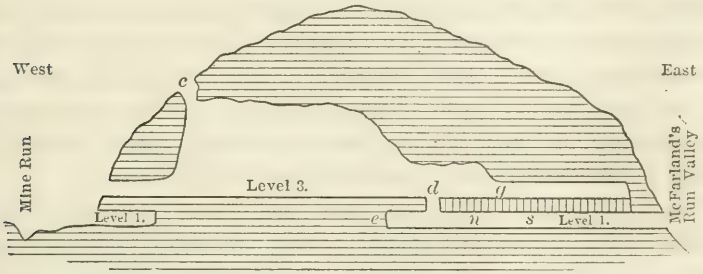
Fig. 2 is a distorted diagram of a vertical section of the east mine, at February 25, 1873. There are no existing records from which a scale-drawing might be constructed; and unfortunately, the writer has not clear memories of the distances involved. However, we shall not be seriously in error if we accept these assumptions: Distance between east and west portals of the two parts of level 1, 800 feet; length of cavern in level 3, 350 feet, and height of it, 40 feet; the distance from air-way, *d*, to the closed end of level 1 is important, perhaps, and yet the writer cannot recall what it was—it may have been 30 feet, or even somewhat more.

It should be stated, that the back of the east part of level 1 was mined-material, about 300 tons, which rested upon boards supported by timbers upheld by the two side-walls.

About the closed end of level 1, the roadway had to be widened by means of blasts in the side-walls. Shortly after 10 o'clock, in the morning of the date last mentioned, a shot was ready to be fired there, near *e* of the figure.

Blasts were invariably charged and fired by the mine-captain's helper, and nearly always they were discharged when the mine contained him only. But occasionally, as in this instance, when but few men were at work, noon or evening was not waited for, but those few men were notified to leave the mine while a shot was fired. So, in this instance, they were notified, and the notice was repeated. The two men who were mining down the back of level 3, about the point *g*, twice replied that they would take the risks of an accident. The car-loader in level 1 was ordered to leave, but instead of so doing concealed himself in some timbering (a battery) about the

Fig. 2.



Distorted Vertical Section of Ritchie Mine (East) as it existed February 25, 1873.

point *n* in the back of the level. All the above was learned later from the man who fired the shot. The four men mentioned were all who were at that time in the east mine.

After the captain's helper had ignited the fuse of the shot in the side-wall of the level near *e*, he walked east to about *s*, a point in the level at which the north side-wall had been cut away that cars might there pass each other. He passed under the car-loader who had concealed himself, and for whom he was on the lookout, but did not observe him.

A mine-explosion resulted, fully as violent as those which occur at fiery coal-mines. Cars which had stood in level 1 were shot in complete wreck out of its portal, and onwards far into the valley of McFarland's Run. Some timbers took a similar course, notably an oak board which was driven through an irregular track and which landed at last upon the opposite hills of the valley more than 500 feet distant from the level-portal.

In the ravine of Mine Run, at the portal of level 3, there was another explosion which did no little damage to the pit-head structures. At *c* a crater was formed, and from it were thrown what we judged to be 40 tons of earth and stone. A tree which had stood there was thrown at least 50 feet, and being a mountain hemlock of complete growth, it afforded some measure of the energy which had formed the crater. According to my present memories, the latter was fourteen feet deep; its figure was that of an inverted flat cone.

The helper who fired the shot was found wandering in the level, burned and mentally deranged, but otherwise uninjured. During lucid intervals, which preceded his death, he told us the story of the accident, as he alone knew it; of his warnings to the men, and of his having seen the level filled with bluish flame as it approached him.

After several hours of labor, the car-loader was dug out from under a pile of hot vein-matter which had fallen into the level when its supporting timbers were swept away. The two men who had remained at *g*, level 3, bore no marks of injury whatever, no burns and no abrasions; yet both of them had been killed. We judged they had died of asphyxia, or because of the pressure which had existed in the level at the moment the crater was formed.

The already-mined grahamite which had formed the back of level 1, had, in part, fallen into the level when its timber supports were swept away. It was ignited, and it afforded a troublesome fire to deal with. By throwing upon it water from pipes led into the level, we were able to shovel it into cars and thus remove it from the mine.

The writer is aware that this second explosion may be explained by use of the theories now held of coal-mine explosions—that it was a result of fire-damp and of dry-dust as well. There was a sort of gas-trap where ignition began, and, possibly, there was another in the roof of the cavern in level 3, even if surface-water did drip at times through it. But, as fire-damp had not been observed, and as it was not essential to the explosion, it seems more rational to regard this as having been a dust-explosion simply.

In the paper published in 1873, and already cited, while relating the characteristics of grahamite, Professor Fontaine says:

"The fine dust produced by handling the mineral is capable, when very dry, of inflaming from an open lamp. This has led to two accidents from explosions. The dry dust having caught fire in the lower levels, the gaseous products became mixed with air in the upper works and exploded."

This mention is but the briefest statement of bare fact; yet it has this additional interest—it seems to have been the first printed announcement of a purely mine-dust explosion.

Nineteen years after the conclusions reached by us in the early summer of 1871, while reading in the library of the British Museum, the writer saw, for the first time, the paper printed by Faraday and Lyell in January, 1845.* It was a report made by them to the British government upon the Haswell collieries explosion of the year previous; a report written in parts by each of them, as is clearly indicated in the text. Faraday wrote:

"In considering the extent of the fire for the moment of explosion, it is not to be supposed that fire-damp is its only fuel; the coal-dust swept by the rush of wind and flame from the floor, roof, and walls of the works would instantly take fire and burn, if there were oxygen enough in the air present to support its combustion."

They found upon the mine-timbering "coke gradually increasing in thickness" as they "neared the place of ignition;" coke which, they believed, had resulted from partial combustion of coal-dust so abundant in the mine. The report continues:

"There is every reason to believe that much coal-gas was made from this dust in the very air itself of the mine by the flame of the fire-damp, which raised and swept it along; and much of the carbon of this dust remained unburnt only for want of air."

Thus Faraday announced what has come to be our present belief regarding fire-damp explosions in coal-mines.

In a Royal Institution lecture,† Professor Abel took for his subject the dust-explosions which then had become so numerous in wheat- and rice-mills. The lecturer stated that such explosions had, "prior to 1872, appeared enveloped in mystery, until their probable cause was indicated by an Austrian observer." Referring to the paper of Faraday and Lyell, printed in 1845, the lecturer continues, "ten years later, M. de Souich, an eminent French mining-engineer, published as original" work which sustained the deductions of those writers. Professor Abel added, "Later on, M. de Souich extended his inquiries into the part played by coal-dust in mine-explosions."

After an ordinarily diligent search, made in the library of the British Museum, and in the Peabody Library at Baltimore, the present writer must say, that he failed to find records of any work done by the Austrian observer of Abel, or of M. de Souich. Several recent writers mention both of them, but nobody has cited the places of their communications.

* *Phil. Mag.*, iii., xxvi., 16. † *Nature*, xxvi., 19; lecture given, April, 1882.

GOLD-MILLING AT THE NORTH STAR MINE, GRASS VALLEY, NEVADA COUNTY, CAL.

BY EMILE RECTOR ABADIE, GRASS VALLEY, CAL.

(Virginia Beach Meeting, February, 1894.)

THE picturesque little mining town of Grass Valley, nestled in the foothills of the Sierra Nevada mountains, at an altitude of 2500 feet, has been for 43 years the scene of uninterrupted activity and prosperity, as the center of a mining district which was intimately associated with the pioneer days of California, and the discovery of gold by James W. Marshall, in El Dorado county, in January, 1848.

Although placer-mining was inaugurated in Nevada county as early as 1848, the first quartz ledge was not located until the summer of 1850. Discoveries made on Gold Hill and Massachusetts Hill increased the excitement in quartz-mining, and hastened the erection (during the same year) of the first mill operated in the State.

At the close of 1864, the district had produced $23,000,000 in gold, and all the well-known properties of to-day had been discovered and worked. The Eureka-Idaho, the North Star, and the Empire mines were in active operation; and to-day we still find them equipped with large crushing-plants, operating 80 per cent. of all the stamps in the immediate Grass Valley district. Of the mines just named, the North Star possesses the most recently constructed mill, a description of which, the writer believes, may prove of interest to members of the Institute engaged in the milling of gold-ores.

During the year 1886, operations at the North Star mine (the property of the North Star Mining Company) had reached such a stage of development that the necessity of a crushing-plant at the mine became imperative, and early in October the erection of a 30-stamp mill was in progress. The building designed to contain 40 stamps was speedily erected; and within sixty days the large structure was under cover. Plate A represents the appearance of the mill 40 days after work was commenced.

Late in February, 1887, the mill was in readiness, and, upon the

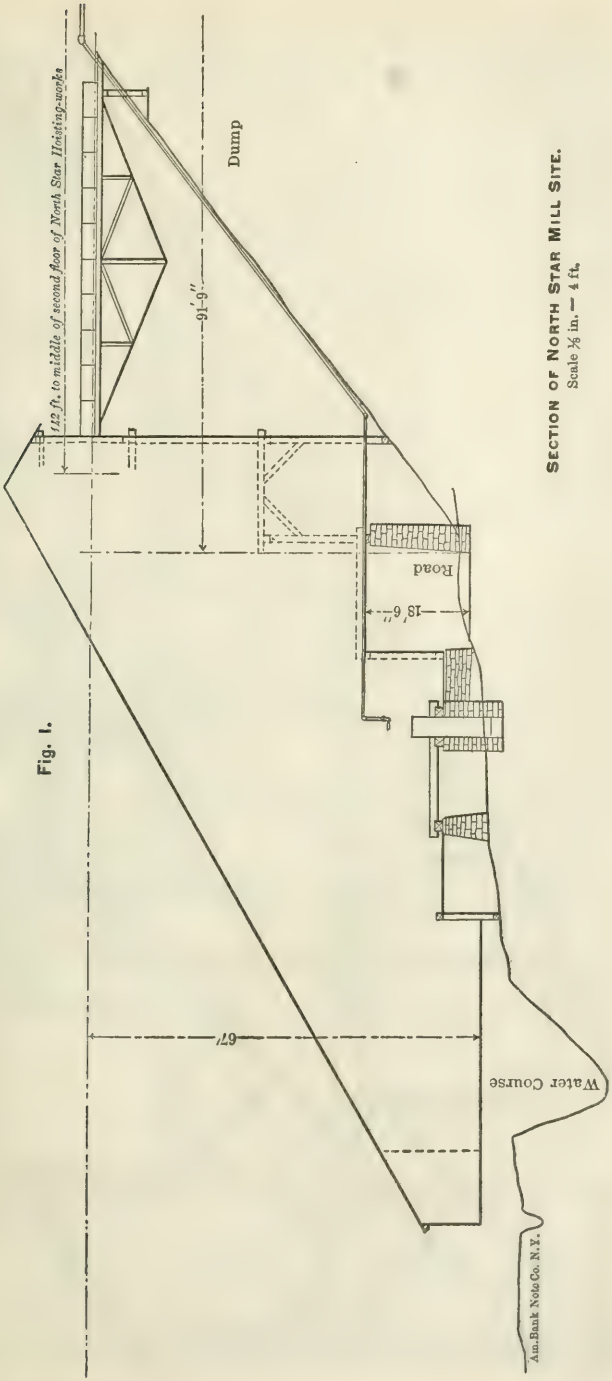


Fig. 1.

SECTION OF NORTH STAR MILL SITE.
Scale ⅛ in. = 4 ft.

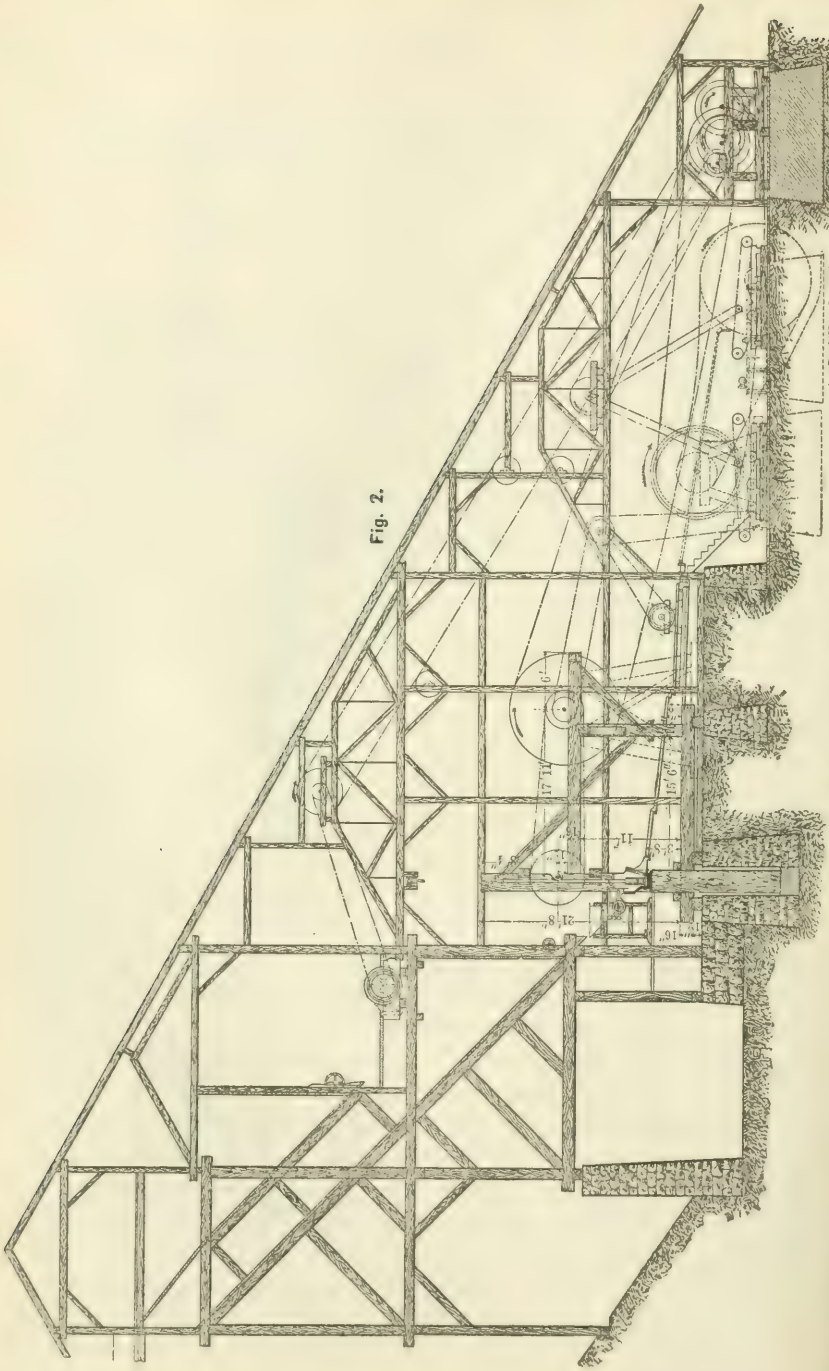


Fig. 2.

SECTION OF NORTH STAR MILL.

completion of the water-power system, then being introduced at both mine and mill, the crushing of ore was commenced March 15, 1887.

The local topography presented most favorable conditions for a mill-site. At a distance of only 142 feet from the landing-floor of the main incline of the mine, it was found possible to discharge the ore at a vertical height of 67 feet above the projected concentrator-floor. Fig. 1, showing a cross-section of the mill-site, illustrates the topographical features utilized in locating the present North Star mill. During the construction of the mill all mining operations continued uninterruptedly, the ore extracted being crushed in leased plants one mile distant.

From the numerous stopes, the ore is trammed to the main shaft, which has now reached a depth of 2400 feet, with an average incline of 20°. The shaft is well equipped with a double track-way, over which 140 cars can be delivered to the surface in 9 hours, actual running-time. One man on each shift delivers the quartz to the mill and waste to the dump.

The ore, as raised from the mine, is delivered in mine-cars, containing a little over 13 cubic feet each; three cars making 40 cubic feet, or two tons of ore. Sorting on the surface is not resorted to; underground, however, the custom of "stripping" the ledge *in situ* insures for the mill a clean product, generally free from diabase (the enclosing rock).

On passing into the mill the ore is discharged over grizzlies, placed at the top of long ore-bins. There are 8 fine-ore bins, one for each battery, of 5 stamps, each provided with a grizzly 4 by 12 feet in size, set at an angle of 40°. The 3-by $\frac{3}{4}$-inch bars are placed on edge, 2 inches apart. These grizzlies simply classify the mine-ore into fine and coarse. The former drops through the grizzly directly into the fine-ore bin, from which it passes through a gate, supplying the automatic feeder, which in turn supplies its own particular battery. Meanwhile, the coarse ore is delivered over the grizzly into the coarse-ore bin, from which it passes through a gate into one of the three rock-breakers. From the breakers it drops as "fine" into the fine-ore bin, mixing with the mine-fine, and passing on into the ore-feeders as above described.

The ore thus passes, by gravity alone, from the dumping-floor to the automatic feeders. One man on the day-shift operates the rock-breakers to crush the accumulated coarse ore delivered from the mine. The breakers (improved Blake, 9 by 15 inches) run intermittently, aggregating not over 7 hours' work during the 24.

From the automatic feeders the quartz is delivered into the batteries for stamping and amalgamation. The free-milling character of the material demands only the simplest methods of amalgamation and concentration for the recovery of its gold-contents. The pulp discharged through the battery-screens flows over silver-plated copper amalgamating-plates to concentrating-machines beyond, passing thence for further treatment in pans, or escaping from the mill direct as tailings.

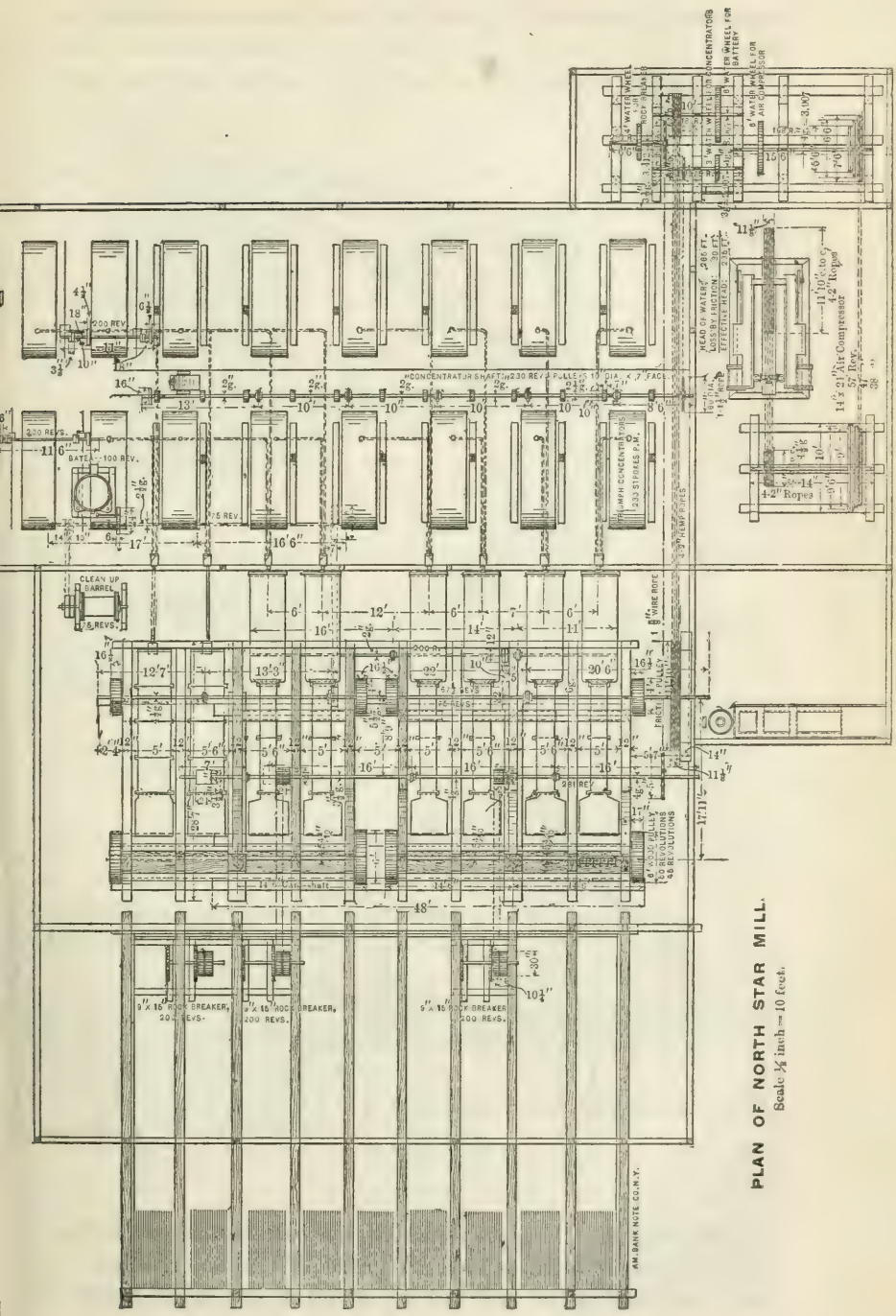
This brief outline of the course of the ore through the mill is illustrated by Figs. 2 and 3, which show a section and plan of the mill, and exhibit the very advantageous arrangement of the entire plant. Fig. 4 gives in detail the battery frame in elevation. For the several blue prints, from which these engravings have been made, I am indebted to Mr. R. S. Moore, Superintendent of the Risdon Iron Works, San Francisco.

The mill contains 40 stamps, weighing, when newly shod, 875 pounds each. The stamps drop 7 inches eighty-six times per minute. The shoes (steel exclusively used), weigh 159 pounds, and the remaining 716 pounds of the stamp are distributed as follows: stem, 358 pounds; stamp-head, 228 pounds; tappets, 130 pounds.

The life of a steel shoe averages 130 days, and it crushes during that period 260 tons of ore. The weight of the shoe, when removed, will average about 38 pounds. The very smooth and uniform wear observed on these shoes is due in great measure to the use of cast-iron dies.

The weight of the die when new is 100 pounds, and it loses during a life of 70 days one-half its original weight. The recent introduction of cast-iron plates 2 inches thick has increased the life of the die to 80 days, the die, when removed, weighing from 40 to 45 pounds. These plates, two in number to each battery, fit snugly in the mortar, forming a false bottom, upon which the dies are bedded as usual. They serve a double purpose, prolonging the life of the dies and decreasing the height of the discharge, which ranges from an initial of 4 inches to a maximum of 6 inches.

Brass wire No. 30 screens and perforated No. 0 tin screens are used exclusively. The latter screen is an experiment, and thus far has given good results. The life of a tin screen is about 30 days; the cost, one-fourth that of wire screens. At the North Star mill, in crushing 113,955½ tons of ore, the cost for screens has been $0.008 per ton. Dies have cost $0.026 and shoes $0.056 per ton,



PLAN OF NORTH STAR MILL.

Scale: ½ inch = 10 feet.

exclusive of a rebate of $1\frac{1}{2}$ cents per pound on removed shoes and dies.

The recovery of gold is first made in the mortar, where the amal-

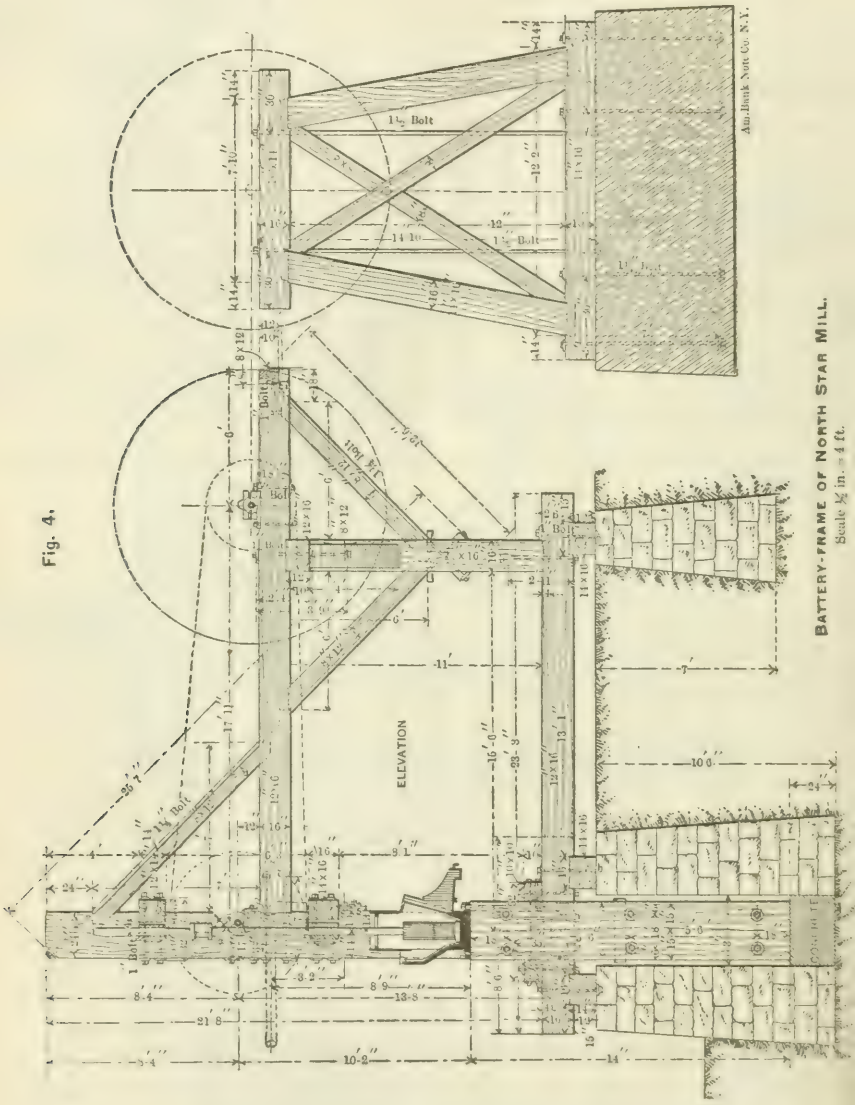


Fig. 4.

gamation begins. Mercury is fed to the batteries at regular intervals, the amount varying with the grade of the ore crushed. At times, this amount has exceeded 1 pound per battery, or a flask of

76½ pounds in 9 days. Of this amount, however, 35 per cent. is recovered at the fortnightly retorting of amalgam. There is, however, a considerable loss of quicksilver, which, in treating 113,955⅓ tons of ore, has amounted to $3680.05 or $0.032 per ton.

In the interior of the mortar, immediately under the screen-frame, are silver-plated copper plates 4½ inches wide by 52 inches long. These plates, one for each mortar, are bolted to chuck-blocks fitting tightly against the lip of the mortar, as shown in Fig. 5, a section of a single-discharge mortar used at this mill. As the pulp is discharged from the mortar through screens, it falls upon an outside battery- or splash-plate 18 inches in width, covering the iron mortar-apron, which is bolted to the mortar and forms part of it. The length of this plate is equal to that of the inside plate.

The apron-plate, 48 by 58 inches, lies below the splash-plate, tapering to 24 inches and connecting with the three sluice-plates which cover the sluice 12 feet long and 24 inches wide. From the sluice, the pulp passes over shaking-tables, which have a plated surface of 10 feet by 48 inches, made up of two plates 48 by 36 inches and two smaller ones, each 48 by 24 inches.

The grades of the above plates are as follows: battery- and apron-plates, 1½ inches to the foot; sluice-plates, 1¼ inches to the foot; shaking-tables, $1\frac{1}{16}$ inches to the foot. The natural tendency of the narrow sluice-plates to "scour" has always been objectionable, and in fitting the more recently erected batteries with a line of plates, the width of the sluice has been doubled. These plates, eight in number, are placed side by side, and overlap slightly, aggregating 15¾ feet by 4 feet wide, the total actual plate-surface being equal to that of the narrow sluice-plates and shaking-table plates combined. The grade of these new plates is approximately uniform at 1½ inches to the foot.

In Fig. 6 these improved plates (which cannot be too highly recommended) are shown in plan and elevation. The result of this introduction by the writer has strengthened the desire to remove the narrow sluices still remaining.

Plate B shows the old and new plates, in the immediate foreground, to the left and right respectively.

Two-thirds of all the gold recovered by amalgamation is found in the batteries. The yield is, however variable, frequently reaching 75 per cent. Of the amount recovered from the outside plates, the battery- and apron-plates will produce 70 per cent., the sluice-plates 23 per cent., and the shaking-tables 7 per cent. The average value

of amalgam from all sources is about $7.25 per ounce; the fineness of the gold-bars is 856.

The batteries are cleaned up fortnightly; the outside plates, every other day, frequently daily, and, on rare occasions, morning and evening.

In addition to the usual method of recovering the amalgam from the plates, the writer introduced, several years ago, the method of hot-water "sweating," which can be recommended for its simplicity, safety and excellent results. The effect of simply pouring boiling water upon the plates or immersing them (on the apron), is a source of as much pleasure as surprise when the amount secured from a poor-looking plate is weighed. Notwithstanding previous sweatings during the year, the December or annual sweating at the North Star mill produced 1703 ounces amalgam from which 600 ounces gold was obtained, valued at $10,524.82.

Leaving the battery-floor, the pulp is conveyed through 3-inch pipes to the concentrators on the floor below. Each battery of 5 stamps being provided with two machines, the full complement consists of 12 Triumph and 4 Frue vanners. The latter machines are preferred, and were introduced when the 10 additional stamps were installed. The machines are conveniently arranged on one floor and readily overlooked by one man on each shift. The appearance of the concentrator-floor is presented in Plate C.

The ore crushed at the North Star mill contains about 4 per cent. of "sulphurets." Their average value is $53.58 per ton; and they have yielded in seven years a total product of $236,756.63. The concentrates are sold to the local chlorination-works.

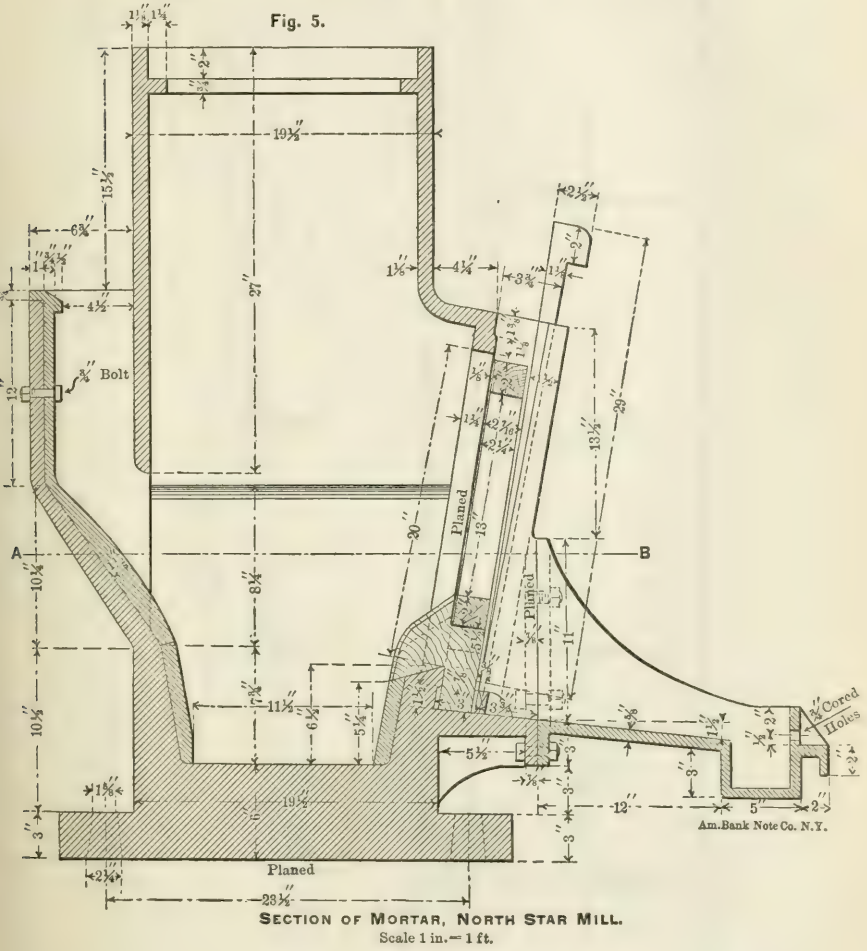
From the concentrating-machines the pulp escapes as tailings, containing more or less gold, notwithstanding a high percentage saved of the ore-value (reaching 94 per cent. under favorable conditions). At the present writing, there is in operation a simplex rotary amalgamator, treating 10 tons of tailings per 24 hours. Results obtained thus far have reduced the loss in the tailings 22 cents per ton.

The mill is operated entirely by water-power and notwithstanding a high rate per available horse-power, the cost of power per ton of ore crushed has not exceeded $0.32. The water used has previously been utilized by the Original Empire Mill and Mining Company under a head of 450 feet. At the North Star, the effective head is 275 feet at the mill.

In conclusion, to accompany the foregoing description the following

tables have been prepared, giving details of the cost of milling at the North Star, extending over several years. During this period the average value of the ore crushed has been high; and for the

Fig. 5.



SECTION OF MORTAR, NORTH STAR MILL.
Scale 1 in. = 1 ft.

year just concluded a gross production of $335,757.04 has been attained.

Plate D is a general view of the plant.

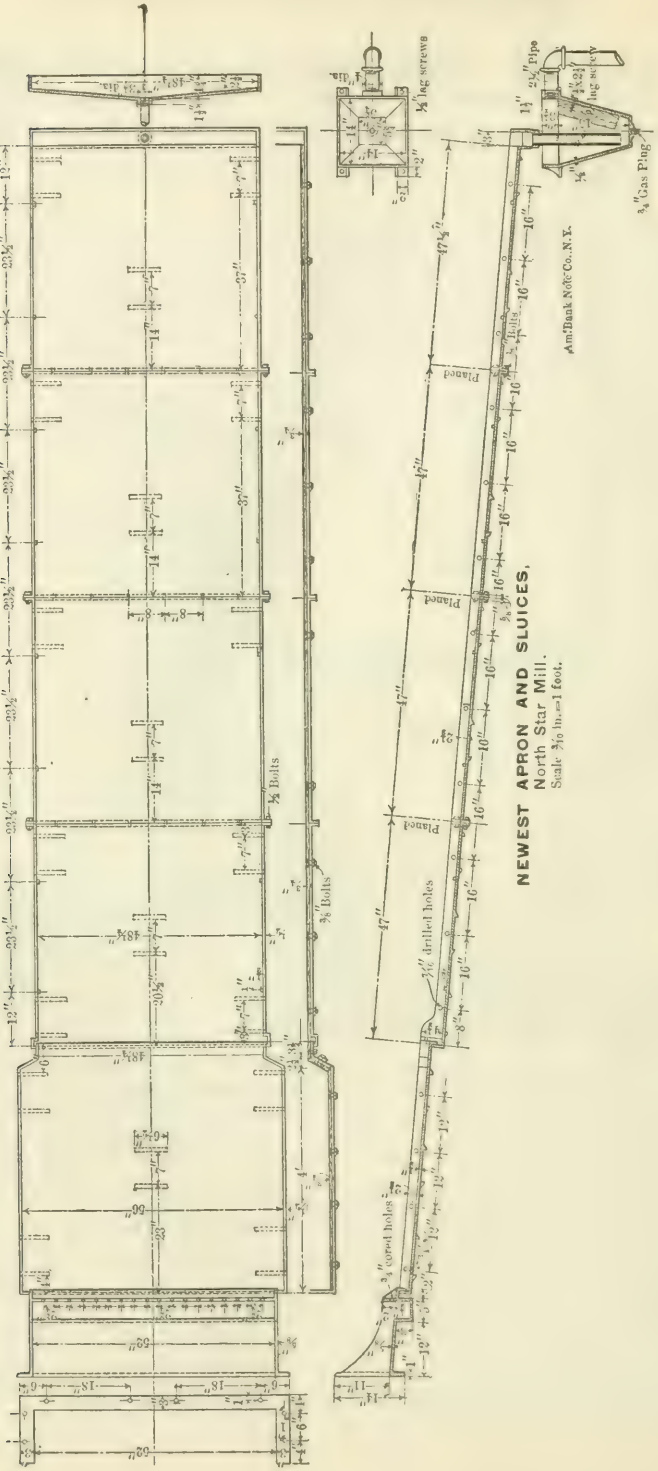


Fig. 6.
NEWEST APRON AND SLUICES,
North Star Mill.
Scale 3/16 in.=1 foot.
Am.Bank Note Co.N.Y.
Planed
drilled holes
countersunk holes
Bolts
Lag screws
Pipe
Gas Plug

Annual Tabulated Cost of Milling at the North Star Mine.

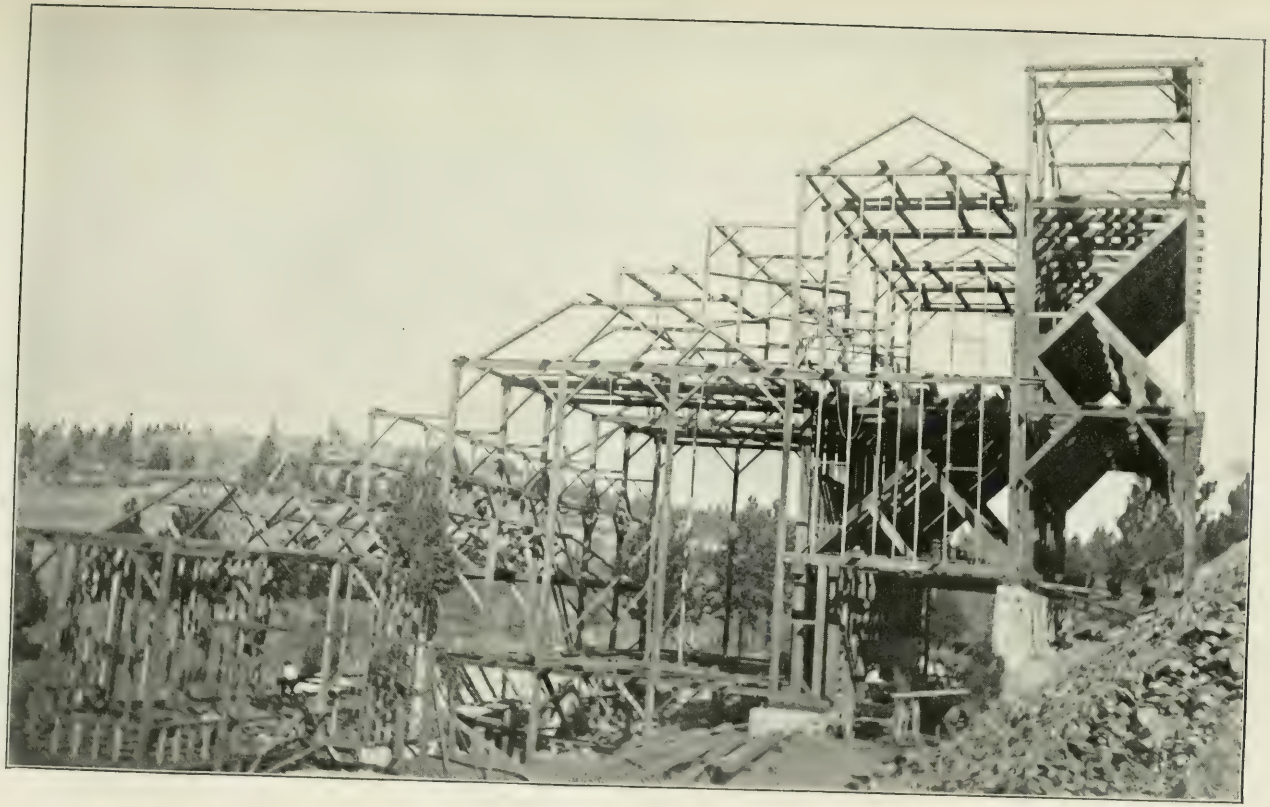
TABLE I.	SUPPLIES.							LABOR.					
Year.	Shoes.	Dies.	Quick-silver.	Screens.	Mill-Castings.	Concentrator, Feeder parts, Assay supplies, Sundries.	Total.	Amalgamation.	Concentration.	Rock Breaking Sundries.	Total.	Power Total.	Grand Total.
1887.....	$433.40	$307.70	$603 00	$150.00	$151.58	$232.10	$1877.78	$2363.00	$2515.25	$971.12	$5849 37	$3100.00	$10,827.15
1888.....	775.82	408 58	508.50	158.53	260.79	628.21	2740.43	2112.00	2527.25	716.15	5355.40	5230.36	13,326.19
1889.....	1700.19	862.09	649 80	167.32	543.97	470.10	4393.47	2559.17	2560.50	946.50	6036.17	6397.75	16,857 39
1890.....	644.75	224.86	607.25	173.52	300.83	655.19	2606.40	2238.80	2377.00	810 00	5425.80	4343.95	12,376.15
1891.....	740.85	379.89	549.50	161.63	291.25	830.70	2953.82	2653.00	2621.75	863.00	6137.75	5768.50	14,860.07
1892.....	1330.49	262.22	424.00	130.33	196.59	140.95	2484.58	2236.00	2101.00	831.25	5168.25	5787.70	13,440.53
1893.....	703.72	469.08	338.00	24.75	198.46	389.61	2123.62	1487.00	1172 75	723 00	3382.75	5787.70	11,294.07
Total.....	$6329.22	$2914.42	$3680.05	$966.08	$1943.47	$3346.86	$19,180.10	$15,648.97	$15,875.50	$5861.02	$37,385.49	$36,415.96	$92,981.55

Tabulated Milling Cost per Ton at the North Star Mine.

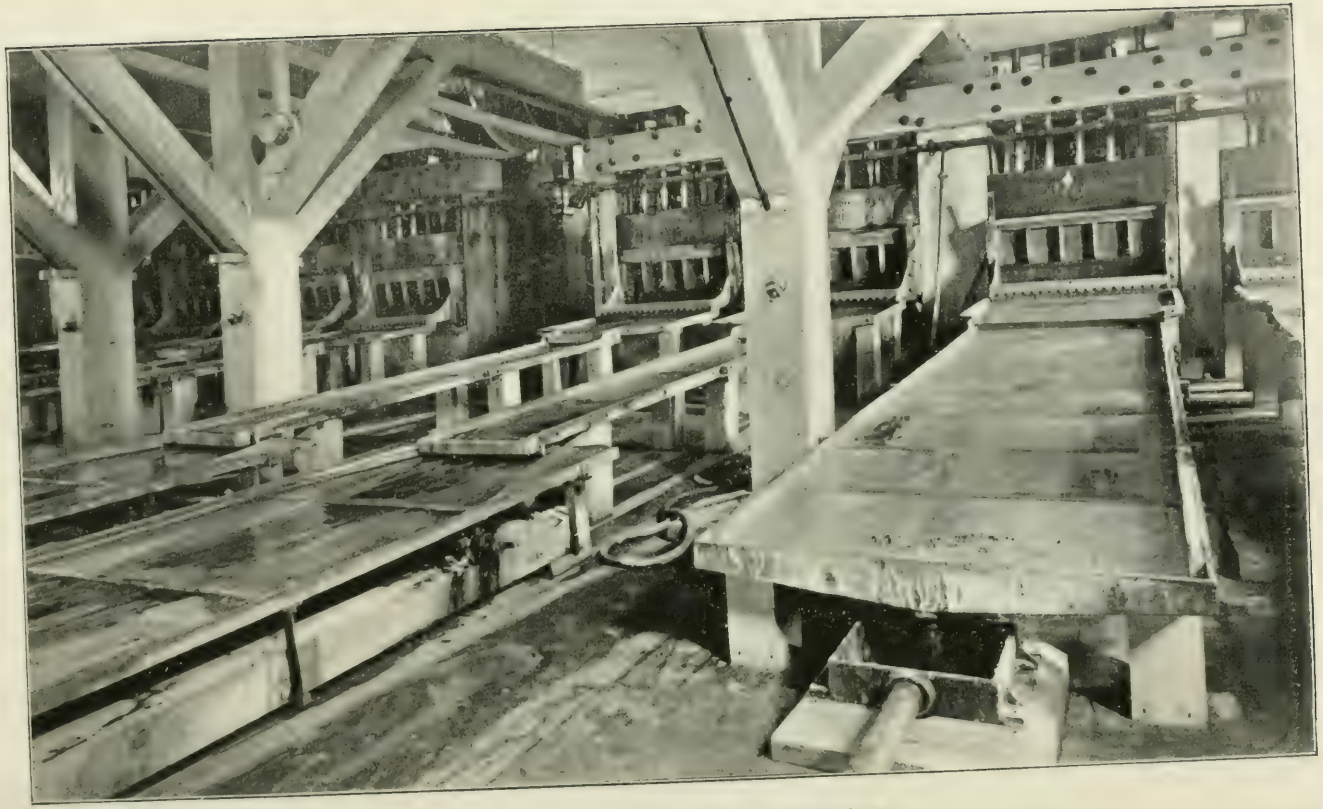
TABLE II.	1887–1893.													
	SUPPLIES.						LABOR.				POWER.		TONNAGE.	COST.
Shoes.	Dies.	Quicksilver.	Screens.	Mill-Castings.	Concentrator, Feeder parts, Assay supplies, Sundries.	Total.	Amalgamation.	Concentration.	Rock Breaking Sundries.	Total.	Total.	Grand Total.	Total Crushed.	Total Amount.
$0.056	$0.026	$0 032	$0.008	$0.017	$0.029	$0.168	$0.137	$0.139	$0.052	$0.328	$0.320	$0.816	113,955⅓	$92,981.55

Résumé.

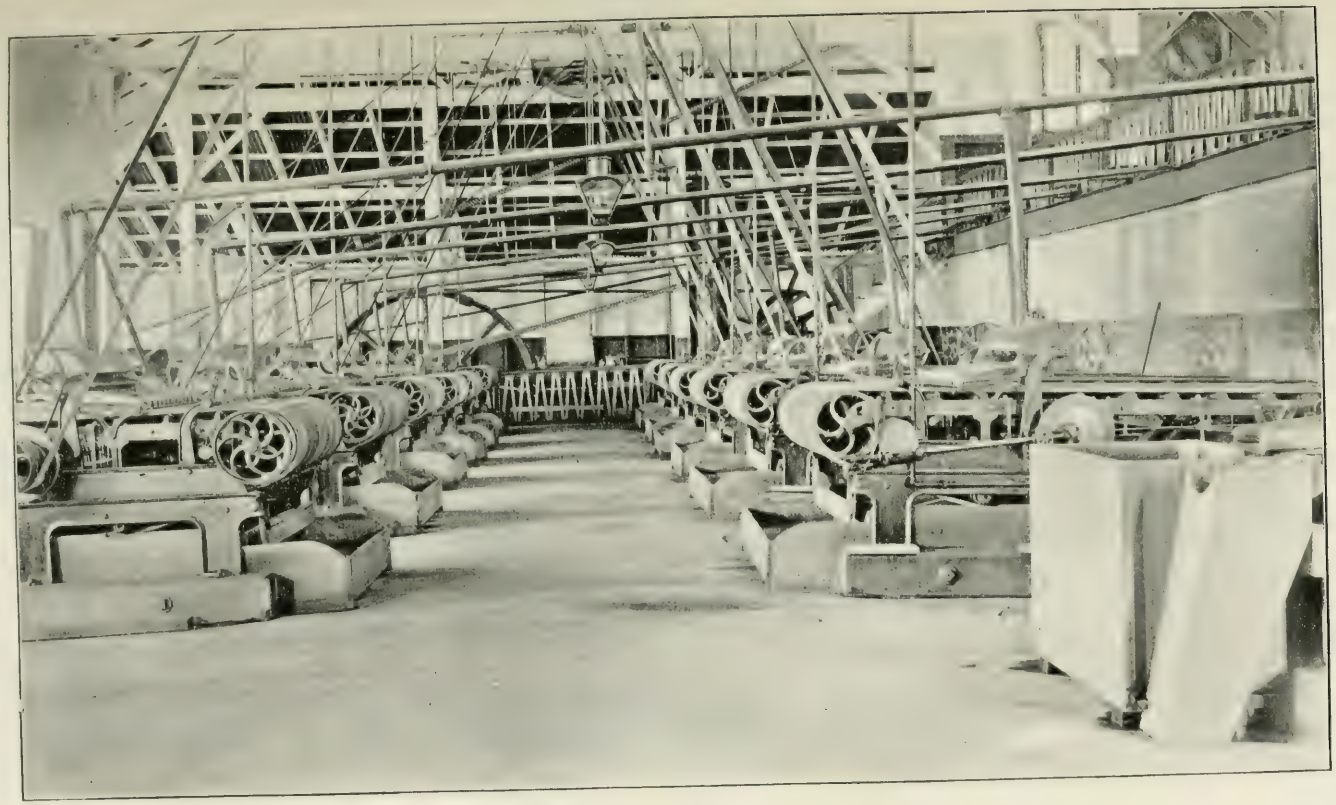
Labor	$0.328
Power	0.320
Supplies	0.168
Total	$0.816



North Star Mill in Process of Construction.



North Star Mill—Old and New Apron-Plates.



North Star Mill—Concentrator-Floor.

PLATE D.

North Star Mine, Grass Valley, Cal.

PRODUCT AND ECONOMICAL RESULTS OF THE MARSAC REFINERY FOR THE YEAR 1892.

BY C. A. STETEFELDT, OAKLAND, CAL.

(Virginia Beach Meeting, February, 1894.)

In a former paper (*Trans.*, xxi., 286) I described the plant of the Marsac Refinery, and the manipulations of the process, but could not, at the time, give reliable statistical results. To supply the latter is the object of the present paper. I shall give actual facts as correctly as possible, without any bias one way or the other. For all statistics I am indebted to Mr. W. G. Lamb, who has charge of the Marsac lixiviation-plant and refinery; to Mr. A. Luebeck, assayer at the Ontario mill; and to the Daly Office, Salt Lake City.*

Before starting on the work of the year 1892, a most careful clean-up was made, and any remnants were charged to the refinery. During 1892 the Marsac lixiviation-works produced 48.8 tons of so-called "regular" sulphides, which contained, on an average, 11,449 ounces of silver per ton; 11.77 ounces of gold per ton; and 569.8 pounds of copper per ton. To this must be added, 266 pounds of sulphides and by-products left over from the product of 1891, which contained 1380 ounces of silver, 1.41 ounces of gold, and 63 pounds of copper. Before giving the results of the clean-up, a general discussion of the items of expense, methods of assaying, etc., is desirable.

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* I had already sent the manuscript of this paper to our secretary when Mr. Lamb informed me that a silver-thief had been caught at the Marsac mill. Silver was stolen, not only recently, but also from the product of 1892. The quantity stolen in that year, thus far traced, and all shipped and sold to one firm of jewellers in Chicago, amounts to 1821.75 ounces of silver, and 1.87 ounces of gold, *i.e.*, it represents over three-tenths of one per cent. of the whole output. While thus the apparent loss of silver is considerably reduced, it is doubtful whether the remaining loss is not also in part due to stealing, so that the process may have done metallurgically and financially much better work than is represented in this paper. The stolen silver has been introduced in the statistical figures as a separate item, wherever necessary. It does not influence the expense account, except express and refining charges, and these are deducted from the value of this silver in figuring the net amount of cash realized. It is not necessary to introduce the item of reclamation—no doubt, the Chicago jewellers who buy stolen silver have already taken care of that.

Labor.—The 48.93 tons of sulphides (and remnants) were put through the refinery in 10 months and 20 days, requiring in a day-shift the labor of two men (one at $4.00, one at $3.50), and one boy (at $2.00). This is exclusive of the labor for melting bullion.

Coal.—Of the coal delivered at the refinery, a portion was consumed in the stove supplying hot air for the sulphide-dryer. Mr. Lamb estimates that this was fully one-quarter of the quantity delivered, and I have made a deduction accordingly.

Steam.—The cost of steam consumed by Koerting pumps and blowers, for running the ball-mill and cake-press and for heating solutions in the dissolving-tanks, is estimated by Mr. Lamb at $929, and I consider this rather too high than too low.

Cement-Copper.—It will be noticed that the refinery produced about as much cement-copper as it consumed; hence, the cement-copper does not enter as an item of expense. The iron consumed in the precipitation of copper costs nothing, there being always on hand at the mill an abundance of old wrought-iron. The cement-copper results by precipitation of wash-water from the silver filter-tanks, and of impure mother-solutions from the crystallizing-vats, *i.e.*, the latter solutions go to precipitation as soon as a mixed copper and iron vitriol commences to crystallize.

Bullion-Melting.—The melting of the silver-cakes was done in crucibles; it should have been done in a reverberatory-furnace. Such a furnace was introduced by the writer many years ago at the Ontario mill. Since at the Marsac mill no accurate account was kept of the cost of crucible-melting, I substitute here the cost of reverberatory-furnace melting.

According to Mr. A. D. Moffat, 1449 bars, weighing 2,152,523 ounces troy, or 147,635 pounds avoird., were melted at the Ontario mill, during 1892, consuming materials and labor as follows:

36 cords of wood, at $5.00,	$180 00
3240 pounds of borax, at 9 cents,	291 60
Labor for melting and weighing,	270 00
For 2 new hearths:	
1 mason for 5 days, at $5.00,	25 00
200 Denver fire-brick,	12 00
100 pounds of fire-clay,	1 00
Blacksmith's work, repairing tools,	10 00
Total,	$789 60

This would be 36.68 cents per 1000 ounces of bullion in bars.

The 36 cords of wood could be easily replaced by 24 tons of Coalville coal, at $4 per ton, reducing the cost of fuel to $96, and the total cost to $705.60, or, the melting of 1000 ounces of bullion to 32.78 cents.

About 2 tons or more of bullion are accumulated before melting is started, and this quantity is put through in three charges. The night-watchman starts the fire at 1 A.M., so that the first melt is ready to draw at 7 A.M., when the two melters go on shift. The furnace once hot, the two other melts follow quickly, and by 3 P.M. the whole work is done, including cleaning, dressing and weighing the bars. After drawing slag, the metal is well stirred and tapped into a row of moulds resting on a movable car. Only one assay is necessary for the bars belonging to one melt. The hearth of the furnace is made of the best Denver fire-brick, and lasts for about 54 melts before renewal becomes necessary. The arch over the hearth of the furnace is movable so that repairs can be easily made. I do not know of a more simple and economical method of melting bullion. The first furnace of this kind was built by the writer, in 1882, at the Lexington mill, Montana. The flue of the furnace should be connected with a dust-chamber for the deposition of fine silver-globules carried forward by the draught during the boiling of the metal.

Refinery-Cleanings.—The large quantity of refinery-cleanings (6.5 tons) consisted principally of ashes, unburnt coke, slags, and broken crucibles from bullion-melting, and some matte-soaked bricks from the muffle-furnace. The crucibles were of poor quality, an old lot being on hand in the store-house, originally made for melting antimony. With reverberatory-furnace melting of the silver-cakes these refinery-cleanings would be very small in weight and carry much less silver.

Assays.—The mill-assays of the sulphides represent the so-called "corrected" assays, *i.e.*, to the precious metals found by a careful scorification-assay, the silver and gold contained in the slag of the scorifiers and in the cupels are added, which increases the original result from 1.2 to 1.3 per cent.

It may not be out of place to make here á few general remarks about the assays of ores, lixiviation-sulphides, and bullion going to refineries. Our reduction-works, whether mills, smelters or refineries, keep only commercial but no exact metallurgical statistics. Even the famous government smelting-works at Freiberg and Clausthal have their *Hüttenremedien*. The assays of ores, as generally made in mills and smelting-works in this country, are far below the actual

contents in silver and gold; in extreme cases this shortage may reach 15 per cent. In making this statement I am far from accusing custom-mills and smelting-works of dishonest practice. If they were to make a settlement on the basis of actual values, they would have to increase the percentage of their deductions on the precious metals materially. There is a justification for these commercial assays, namely, the greater the loss in assaying, the greater in proportion will be the loss in treatment on a large scale.

I never heard of a refinery that would not make reclamations on bars stamped to their full value. In well-conducted parting-works, using the sulphuric acid process, the legitimate loss in silver is not less than 0.1 per cent. If the books of the refinery do not show this loss it is because their calculations are based on understamped bars. Hence, a moderate undervaluation of bars is excusable, or the refinery would be the loser. But some establishments carry this a little too far, and then we can call it only by a hard name.

While at Austin, Nevada, from 1865 to 1869, the writer melted and assayed a great deal of bullion, and the bars were shipped to various refineries in Europe and in the United States. The worst reclamations—which were, however, successfully contested—came from a firm in Paris; the next best experts in reclamations were a well-known establishment in San Francisco. Such unblushing robbery, I am glad to say, has ceased to exist. If silver-bars only $\frac{1}{1000}$, and below, fine in gold are parted and refined, the reclamations on gold are generally very heavy. I do not know—and all refineries are averse to giving information for publication—how great the actual loss of gold in such cases is; but I presume that it may be difficult to obtain a complete settling of the extremely fine particles of gold when it forms so small a fraction of the alloy. I am convinced that the Moebius electrolytic process is cheaper and better adapted to the parting and refining of such bullion than the sulphuric acid process. Wherever cheap water-power is available, and the output is large, reduction-works producing *doré* bullion will find it profitable to do their own refining by the electrolytic process. Several producers in one locality could easily combine and erect a plant for common use. I make the above remarks that the reader may better understand what follows.

Sale of Refinery-Cleanings.—The 6.5 tons of refinery-cleanings were sold to the Omaha-Grant smelter at sulphide-rates (these will be stated later on). A settlement was made on the basis of 7400.45 ounces of silver and 5.223 ounces of gold, according to mill-assays.

At the time of the sale the price of silver was lower than 84.864 cents; but having assumed this price throughout, this being the average price for which silver in bars was sold, I retain it here for the sake of uniformity in making comparisons. On this basis, the silver in refinery-cleanings brought $6091.91, and the gold, $104.46, total, $6196.37, less freight and charges of $30 per ton.

Returns of the Aurora Refinery.—The Marsac bars, on an average .927 fine in silver and .00096 fine in gold, were all shipped to the Aurora refinery. The latter first remelts the bars in large crucibles, and then charges 0.7 cent per gross ounce on their weights after remelting, for parting and refining. The refined bars were then shipped for sale to New York. The following table shows the differences between the mill-assays and the returns of the Aurora refinery.

*Table Showing Reclamations of Aurora Refinery.**

	Fine silver. Ounces.	Fine gold. Ounces.
Marsac assays,	545,044	569.752
Aurora returns,	543,003	536.563
Reclamations,	2,041	33.189
Or,	0.37 per cent.	5.82 per cent.

The average price at which the silver was sold in New York was 84.864 cents per fine ounce. The whole product brought:

For silver,	$460,814.80
For gold,	11,090.74
Total,	$471,905.54

The reclamations amounted to:

On silver,	$1732.07
On gold,	686.00
Total,	$2418.07

* If the figures in the annual report of the Daly Company be compared with those in this paper, a discrepancy will be found. This is due to the omission in the annual report of one bar which actually belonged to the clean-up.

The annual report also shows a much larger quantity of silver in sulphides sold to smelters than would be normally contained in base sulphides and carbonates which do not go to the refinery. This came about as follows: At the time, the Solvay soda used for precipitating lead separately had been all consumed, and the arrival of a new supply was delayed. Hence, the silver-solution had to be precipitated, for some time, at once by sodium sulphide producing precipitates rich in lead and not adapted to treatment in the refinery.

It will be seen that the Aurora reclamations on silver are not excessive, while those on gold are out of proportion. However, the Aurora refinery may not have recovered more gold than reported above. So far as the extraction of gold in the Marsac refinery is concerned, there is no reason why it should be below that of silver; on the contrary, it should be in excess, as shown by all the Marsac mill-assays. The Aurora returns, however, make the apparent loss in gold very large.

Statistics of the Marsac Refinery.

The following statement shows the quantities of metals and sulphuric acid sent to the refinery:

Sent to the Refinery.

	Ounces.
Silver, in sulphides,	558,715
" in remnants,	1,380
Total,	560,095
Gold, in sulphides,	574.42
" in remnants,	1.41
Total,	575.83
	Pounds.
Copper, in sulphides,	27,805
" in remnants,	63
" in cement-copper,	3,363
Total,	31,231
Sulphuric acid (66° B),	52,906

This gives a consumption of 0.54 pounds of sulphuric acid per pound of sulphides, or 1.74 pounds per pound of copper actually dissolved, or 1.96 pounds per pound of copper in blue-stone.*

The comparatively low consumption of sulphuric acid, considering that, besides the copper, some impurities in the roasted matte are also dissolved, is explained by the fact that the roasted matte contains more or less silver sulphate and cuprous oxide, which react upon each other in the dissolving-tanks, precipitating silver and forming copper sulphate. On account of the presence of cuprous oxide, the copper-plates suspended in the dissolving-tanks are not

* These figures are not absolutely correct, but as close as possible under the circumstances. A few hundred, or even a thousand pounds more or less of sulphuric acid would not influence the expense-account materially.

perceptibly attacked, and are kept there only as a matter of precaution.

Product of the Refinery.

(Mill-assays).

Silver:*	Ounces.	Per cent.	
In bars,	545,044	97.313	97.638
Stolen,	1,822	0.325	
Refinery-cleanings,	7,400	1.321	
Final cleanings,	169	0.030	
Not accounted for,	5,660	1.011	
Total,	560,095	100.000	

Gold:*	Ounces.	Per cent.	
In bars,	569.752	98.945	99.270
Stolen,	1.870	0.325	
Refinery-cleanings,	5.223	0.907	
Final cleanings,	0.071	0.012	
Total,	576.916	100.189	
Plus of gold,	1.086	0.189	

Copper:	Pounds of Copper.
In 108,011 pounds of blue-stone,	27,003
In cement-copper,	3,404
In refinery-cleanings,	509
Not accounted for,†	315
Total,	31,231

Of the blue-stone, there was sold:

To outsiders, 11,637 pounds, at 6¼ cents,	$726 30
To the mill, 96,011 pounds, at 5½ cents,	5,300 57
Total,	$6,026 87

* In figuring the product on the basis of Aurora returns, the silver in bars would be 96.948 per cent., and the loss 1.376 per cent.; the gold in bars would be only 93.180 per cent., and the loss as large as 5.576 per cent. As already remarked, there is no reason why the loss in gold should be so much in excess of the loss in silver, and the Aurora gold returns are manifestly too low. The assay of sulphides for gold is liable to be below the actual contents in gold; hence the plus of gold.

† The copper not accounted for is in the silver bars. The latter were not tested for their percentage in copper. If all the base metal in bars were copper there would be an excess of the latter in the clean-up. On the other hand, more mother-solution may have gone to precipitation than actually belonged to the clean-up for 1892. Since, however, it was found that the addition of cement-copper in matting sulphides is not essential to the success of the process (as had been assumed in the beginning), it is not necessary to buy cement-copper, but only to work in the cement-copper produced in the refinery. This copper always contains a small amount of silver, derived partly from the turbid solution coming from the Roessler tank, and

Expenses of the Refinery.

Labor,	$3,040 00
Coal, 170 tons, at $4.00,	680 00
Sulphuric acid, 52,906 pounds, at 2.48 cents,	1,312 07
Materials and supplies,	269 00
Melting bullion,	192 00
Steam and power,	929 00
Freight on bullion,*	5,314 77
Refining and commission,†	4,397 81
Sacking refinery-cleanings,	65 00
Freight and charges on the same,	195 00
Total,‡	$16,394 65

The products of the Marsac refinery were sold as follows:

Products Sold.

Bars,	$471,905 54
Value of stolen bullion,§ net,	1,543 92
Refinery-cleanings,	6,196 37
Blue-stone,	6,026 87
Value of final cleanings,	145 30
Total,	$485,818 00
Less expenses,	16,394 65
Realized net,	$469,423 35

Sale of Sulphides to Smelters.

Before the Marsac mill erected its own refinery the "regular" sulphides were sold to the Omaha-Grant Company on the following terms: freight and charges, $30 per ton; 97 per cent. of the silver

partly from silver-bearing scales on old wrought-iron used for the precipitation of copper.

* Freight on bars from Park City to New York, *via* Aurora, is 0.975 cents per fine ounce of silver, as reported by mill-assay.

† The broker's commission for selling bullion in New York is $\frac{1}{16}$ of one per cent. on value. This and the Aurora refining-charges make together a cost of 0.806 cents per fine ounce of silver, on the basis of mill-assay.

‡ In this expense-account two items have been omitted which might be properly introduced, namely, a charge for superintendence, and interest and amortisation on the capital invested in the refinery-plant.

§ 1821.75 ounces of silver at 84.86 cents,	$1,545 93
1.87 ounces of gold,	38 65
Total,	$1,584 58
Less express and refining charges,	40 66
Net value,	$1,543 92

paid for at New York quotations; $20 per fine ounce of gold. To this must be added the cost of sacking sulphides and hauling them to the Park City R. R. depot, at $10 per ton, and the reclamations on the mill-assays. It will be of interest and importance to investigate the differences between the mill-assays and the smelters' assays of sulphides.

The sulphides are first carefully sampled and assayed at the mill, and then put up in double sacks for shipment. After arriving at the smelter they are resampled and assayed, and a bottle is sent to the mill for assaying. The two samples and three assays form the basis of settlement between the mill and the smelter, but there are no fixed rules about it. In most cases a splitting of differences is resorted to.

The following abbreviations will here be used: M. S.—M. A., for mill-sample and mill-assay; O. S.—M. A., for Omaha and Grant sample and mill-assay; O. S.—O. A., for Omaha and Grant sample and assay.

On eight lots of "regular" sulphides, sold in 1890, weighing 17.5 tons, and containing, as per M. S.—M. A., 218,324 ounces of silver and 127.63 ounces of gold, the differences in assays were as follows:

Comparing O. S.—M. A. with O. S.—O. A., the latter was 0.587 per cent. lower in silver and 3.787 per cent. lower in gold than the former.

Comparing M. S.—M. A. with O. S.—O. A., the latter was 1.419 per cent. lower in silver and 5.469 per cent. lower in gold than the former.

In effecting a settlement, a deduction or reclamation of 0.564 per cent. in silver and of 1.802 per cent. in gold, based on M. S.—M. A., was accepted by the mill.

On five lots of "regular" sulphides, sold in 1891, weight 16.72 tons, and containing, as per M. S.—M. A., 156,091 ounces of silver and 134.28 ounces of gold, the differences in assays were as follows:

Comparing O. S.—M. A. with O. S.—O. A., the latter was 1.355 per cent. lower in silver, but 2.271 per cent. higher in gold than the former.

Comparing M. S.—M. A. with O. S.—O. A., the latter was 2.020 per cent. lower in silver, but 3.951 per cent. higher in gold than the former.

Separate terms of settlement for these lots I did not obtain; they are included in the twenty-six lots following.

On twenty-six lots of "regular" sulphides, base sulphides, and carbonates, sold in 1891, weight 76.69 tons, and containing, as per M. S.—M. A., 505,255 ounces of silver, and 427.28 ounces of gold, the difference in assays were as follows:

Comparing M. S.—M. A. with O. S.—O. A., the latter was 1.923 per cent. lower in silver, but 3.796 per cent. higher in gold than the former.

In effecting a settlement, a reclamation on silver of 0.931 per cent. was accepted by the mill, and a gain in gold of 1.633 per cent. was conceded by the smelter.

Outside of these twenty-six lots, twelve lots of sulphides were sold in 1891, on which no reclamations were made.

On the total production of sulphides, sold in 1891, there was a reclamation of 0.616 per cent. in silver, and a gain in gold of 1.078 per cent. on the basis of M. S.—M. A.

Finally, I received from the Daly office a statement of reclamations on all lots of "regular" sulphides sold in 1889, and up to 1892. These contained 1,774,776 ounces of silver and 1474.51 ounces of gold. The reclamations on silver were 5100 ounces, or 0.287 per cent., and on gold 4.60 ounces, or 0.312 per cent.

It will be seen from the above figures, that there is more or less, and rather variable, discrepancy between the respective samples and their assays. This is not surprising, in consideration of the richness of the material, the different methods of assaying, and the effort of the smelter not to be found on the losing side. It is not necessary to make further comments on these figures—they tell their own story.

Net Profit Realized by Selling Sulphides to Smelters.

It now remains to investigate how much the mill would have realized by selling the product of "regular" sulphides for 1892 to smelters. It is somewhat difficult to decide on which basis of reclamation this is to be done, and Mr. R. C. Chambers very properly remarks in this respect, "There are no figures that can be furnished giving a reliable guide as to what would be the loss through reclamations in the future, because there has been no regularity in the same."

Under the circumstances, I consider it best to use the last statement of reclamations, which is the most unfavorable to the refinery. In this statement, the reclamations on silver were 0.287 per cent., and on gold 0.312 per cent. Hence, we have to deduct from the

silver-product of 560,095 ounces for 1892, first, 0.287 per cent. reclamations, and then, 3 per cent. smelters' discount, which leaves 541,734 ounces of silver at 84.864 cents, or $459,727.14. From the gold product of 575.83 ounces we have to deduct 0.312 per cent. reclamations, which leaves 574.04 ounces at $20, or $11,480.80. From this we must deduct $1957.20 for sacking, for freight, and for smelters' charges on 48.93 tons of sulphides.

Realized by Selling to Smelters.

Silver sold for,	$459,727 14
Gold sold for,	11,480 80
Total,	$471,207 94
Less Expenses,	1,957 20
Realized net,	$469,250 74
Realized net by refinery,	469,423 35
Difference in favor of refinery,*	$172 61

* After the Marsac refinery had completed the clean-up for the product of 1892, the Dewey-Walter Refining Company took a contract to refine the "regular" sulphides by a new process (dissolving the sulphides in hot, concentrated sulphuric acid), and started operations in February, 1893. The terms of their contract are as follows: The Dewey-Walter Refining Company pays all expenses for labor, fuel, chemicals, and supplies (except steam and power which are furnished by the mill), keeps the by-product of blue-stone, and returns all the silver and gold, as per mill sample and "corrected" mill-assay, in the shape of fine, marketable bars. The mill pays for this performance 1¼ cents per fine ounce of silver and gold.

Should the Dewey-Walter Refining Company be able to continue this contract in the future (which, at the time of this writing—the end of June, 1893—is by no means certain), it will be of interest to calculate how much the Marsac mill would have realized on the product of 1892 by such a contract, as compared with selling to smelters:

Dewey-Walter Refining Company's Process—Expense Account.

Charges,	$7,008 00
Steam and power,	929 00
Freight on bullion,	5,462 00
Broker's commission,	304 00
Total,	$13,703 00

The product of bullion would have sold for:

Silver,	$475,319 00
Gold,	11,902 00
Total,	$487,221 00
Less expenses,	13,703 00
Realized net,	$473,518 00
Realized net by selling to smelters,	469,250 00
Difference,	$4,268 00

Using, however, as a basis for calculation, the reclamations on the total product of sulphides sold in 1891—which seems to me more just and equitable, because these reclamations refer to the last sales—the difference between selling to smelters and refining at the mill by the old process would have been $1520.28 in favor of the refinery.

From this it appears, that in order to make refining at the mill *decidedly* profitable, the process, as conducted in 1892, must be improved; and this point will be discussed in my concluding remarks. In making this statement it is assumed that the loss of 1 per cent. in silver is a *bona fide* loss, and was not caused by further stealing.

Finally, it can be easily seen that, with a lower price for silver, the profit in refining at the mill, as compared with selling to smelters, becomes less, provided the terms of the latter remain the same. The expenses of refining are independent of the price of silver, while the smelter's discount of 3 per cent. diminishes in dollars and cents with a drop in the price of silver. It is evident, however, that when a certain limit has been reached, the smelter will have to raise his deductions if he wants to make the same profit as formerly. It is, really, the deductions and not the freight and charges of $30 per ton, out of which his profit is derived. With each decline in the price of silver of 1 cent per fine ounce, the smelter's margin of 3 per cent. on sulphides containing 11,449 ounces of silver per ton, declines $3.43. Considering the recent drop in the price of silver, this lessening of the smelter's profits becomes quite serious.

Conclusion.*

The old process for refining sulphides, which worked very smoothly in a technical sense, should it be restored, can be made more economical in expense and less wasteful in silver. The plant, as now constructed, requires only an addition of crystallizing-vats for blue-stone to handle more than double the quantity of sulphides, which is important in view of the erection of lixiviation-works by the Ontario mill.

The muffle-furnace, which was running intermittently, proved,

* If the success of the Dewey-Walter process were an accomplished fact (the process has been abandoned at the Aspen mill, Colorado) it would not be worth while to continue this discussion. Such a contract, under the terms specified, would under all circumstances be of benefit to the Marsac mill, relieving the management of a great deal of trouble and responsibility. As it is, however, the discussion on the old process may be continued with profit.

in consequence, to be very wasteful in fuel. It took more coal to heat up the furnace in starting roasting, and to keep it hot for 16 hours, while no roasting was done, than the quantity consumed during roasting itself. Nor would it be necessary to employ twice the number of men in doubling the capacity of the refinery.

The selling of sulphides to smelters should be avoided, if possible. There is too much annoyance and uncertainty in effecting a just and equitable settlement. Every improvement in the present plant, and in the system of working, will materially increase the balance in favor of refining at the mill.

The most important point for consideration is, to reduce the loss of silver. Where does this loss take place? If the products are carefully handled, the mechanical loss by dusting is very slight. The gases from both the matting-furnace and the muffle-furnace pass through the Roessler converter, where dust and fumes are arrested. In boiling the roasted matte in the dissolving-tanks some fine silver may be carried forward by the steam; and these tanks should be covered with lead-lined hoods. If the asbestos filters in the silver-tanks are carefully watched, there is little danger of silver escaping with the copper-solution. I do not hesitate to say, that by far the largest part of the silver-loss was due to the present method of melting the silver-cakes in crucibles. The silver-cakes contain more or less impurities, which cause the metal to boil during and after fusion. In fact, the silver globules adhering to the sides of the melting-furnace, and accumulating in the flue leading to the chimney, and the large amount of silver found in the ashes, are sufficient evidence in support of this statement. Reverberatory-furnace melting would reduce these losses to a minimum.

It would be well to moisten the cement-silver before pressing into cakes with a concentrated solution of borax, perhaps with addition of nitre. A thorough drying of the cakes would not be necessary. The reverberatory-furnace could be charged with them, and a slow fire started to drive out all moisture first. This method of melting would also reduce the weight of refinery-cleanings to a minimum, thus causing less expense in their reduction.

A further saving could be made by the introduction of an electrolytic plant for parting and refining the *doré* bars. This would do away with the Aurora reclamations of $2418. The metal from the melting-furnace could be cast at once into plates for the electrolytic refinery. It is claimed that the cost of refining *doré* bars in the Moebius electrolytic plant at Mansfield, Pa., is only $\frac{1}{8}$ of 1 cent

per ounce of bullion. If this be true, I should think ½ cent would be a liberal charge at Park City, provided the dynamos were run by water-power from the Ontario tunnel.

I refrain from putting these estimated profits into dollars and cents, because this can be just as well done by the reader who feels interested in the subject.

In conclusion I may say, that Mr. W. G. Lamb, who ran the old refinery successfully after I left Park City, fully endorses all the statements and opinions expressed in this paper.

THE BAUXITES: A STUDY OF A NEW MINERALOGICAL FAMILY.

BY FRANCIS LAUR, ENGINEER OF MINES, DEPUTY OF THE SEINE, PARIS, FRANCE.

(Virginia Beach Meeting, February, 1894.)

BAUXITE, at first considered as a mineralogical curiosity without importance, now attracts daily increased attention from mineralogists, geologists, and manufacturers. The metallurgy of aluminum, which has entered with the advent of electrolysis upon a new phase within the last few years, requires every day larger quantities of pure alumina, and this can only be obtained from bauxite, which is, of known substances, the richest in free alumina.

Bauxite has been mined since 1872 in France, where about 200,000 tons have been produced, and where, as a natural consequence, its deposits, qualities, and applications have become known. Within a few years past, discoveries of important deposits of bauxite have been made in the United States and Canada, and this almost entirely new subject has thus acquired an additional interest for the mining engineers and metallurgists of the American continent. A brief summary of the available data concerning it appears likely, therefore, to be appropriate and valuable.*

I.—HISTORICAL.

It was in 1821 that the famous chemist Berthier discovered at Baux (Bouches du Rhône) a hydrate of alumina (varying from 66 to 79 per cent. in its contents of the oxide) mixed with silica and

* SECRETARY'S NOTE.—This paper, prepared for the International Congress at Chicago, was received too late to be accepted for that meeting.—R. W. R.

ferric oxide, to which he gave the name bauxite, thus establishing a new and vaguely defined mineral species.

This new mineral subsequently attracted the attention of M. Le Chatelier, a mining engineer, who attempted to utilize it in the manufacture of sulphate of alumina, but without great success, the quantity of ferric oxide present rendering the sulphate too impure. His endeavor to employ the mineral in the preparation of refractory materials was thwarted by its almost unlimited shrinkage.

The method of St. Claire Deville for the manufacture of aluminum having been brought forward, the works of Salyndres (Gard) manufactured from the bauxite of Baux the pure alumina required for that process. Unfortunately, the proportion of silica contained in this bauxite occasioned very considerable losses in the manufacture, which was finally abandoned about 1873.

All the applications of bauxite thus far appeared to have been unsuccessful, when, about 1872, M. Trouilloud, a prospector, and M. Augé, a distinguished Chief of Division of the *chemins de fer du Midi*, brought to the writer* a creamy-white, pisolitic rock, which soiled the fingers when handled, and which had been found in a bed-vein of considerable size in the tunnel of St. Pargoire, near Villeveyrac (Hérault, France).

The following analysis, which we have preserved, showed us immediately the importance of the discovery, and the existence of a new, namely, a white, bauxite, containing:

	Per cent.
Al_2O_3,	82.00
SiO_2,	2.00
Fe_2O_3,	0.10
Water,	14.20
Not determined,	1.70
	100.00

It was a native monohydrate of alumina, Al_2O_3, H_2O.

This bauxite possessed a remarkable property, namely, it was easily and energetically attacked by sulphuric acid. It occurred to us at once to resume the attempt of Le Chatelier in the direct manufacture of sulphate of alumina. The small proportion of ferric oxide contained in the Villeveyrac deposit favored this endeavor, and we were so fortunate as to establish, in 1875, the first works

* The two gentlemen named, together with the writer, founded the firm of Augé et Cie., which has exploited the French bauxites since 1873.

manufacturing sulphate of alumina from bauxite. The deposit was then exploited continuously and on a considerable scale.

The noted American manufacturers, Messrs. Harrison Brothers, of Philadelphia, having informed themselves concerning our bauxite process, introduced it at their works, and for more than a dozen years were consumers of the French white bauxites. But after the uses of the mineral had in this way become known in the United States, it was speedily discovered in Alabama, Georgia, etc. Thanks to our modest labors, therefore, but especially to those of Berthier and Le Chatelier, America has become independent of Europe for its supply of bauxite, except as to the non-siliceous variety, concerning which we shall speak in the sequel.

But one problem solved always leads to the solution of others.

The new variety of bauxite recognized by us, as above narrated, contains much more silica than the selected specimen which we analyzed in 1873. Certain German manufacturers, Messrs. Bergius, of Lissa-Schlesien, near Breslau; Messrs. Giulini Brothers, of Ludwigshafen; and Mr. Rademacher, of Carolinenthal, at Prague, in Bohemia, requested us to seek for them in France bauxites as nearly free as possible from silica. These manufacturers had, in fact, taken up again the Salyndres dry method of treating the red bauxites, by attacking them in the furnace with carbonate of soda. But, as had been the experience at Salyndres also, each unit per cent. of silica caused a considerable waste of alumina and silica, in the form of insoluble silico-aluminates of soda, during the treatment, the lixiviation, etc.

Our investigations, and those of our colleague, Mr. Augé, were crowned with success. We were able to bring to light, in the French Departments of Bouches du Rhône, Var, Alpes Maritimes, etc., deposits of a perfectly red bauxite, constituting a fine homogeneous paste, and containing only from 1 to 3 per cent. of silica. This was still another new species.

The success of this bauxite was considerable. The mineral of Baux was abandoned, and the shipments of red non-siliceous bauxite from the Var attained the figure of 20,000 tons per annum.

At last came the new method of the manufacture of aluminum by electrolysis, which America did so much to bring to light, and the demand became more imperious than ever for non-siliceous bauxites, which have been found, up to the present time, nowhere except in the south of France, but which may be hereafter discovered in the United States.

These bauxites, shipped to Germany, are transformed into pure alumina, which is then sent to all parts of America, notably to Pittsburgh, etc., for the manufacture of aluminum. It is scarcely necessary to point out, in passing, the advantage which would be derived by America from the manufacture upon its own soil of this alumina, which is now purchased at 700 francs or $140 per ton.

Such is the history, rapidly sketched, of the discovery of different varieties of bauxite, and the successive phases through which their commercial applications have passed.

II.—The Family of the Bauxites.

Although the industrial applications of this mineral have been thus developed in a few years, an exact account of its nature is still wanting. What are the bauxites?

They occur in the earth's crust in the form of veins, beds, or amorphous masses, not crystallized, and possessing no constant organoleptic characters. Hardness, color, texture, density—everything—changes, often in the same deposit. Hence the difficulty of establishing general characters by which these minerals can be recognized. Impressed with this difficulty of fixing, mineralogically, the general characters of the new family of bauxites, I was obliged to seek to avoid it by effecting, with the aid of thousands of analyses, a sort of molecular classification, in the absence of constant external peculiarities. The result of this endeavor will now be stated.

When these minerals are studied, not in isolated specimens but in mass, it is quickly noticed that there is in their composition one constant, so to speak, namely, the general proportion of anhydrous alumina, Al_2O_3, the average of which is about 66 to 69 per cent. This figure is given by the analyses of thousands of shipments. Representing this constant by A, we find three variable elements, *e*, besides, namely, water, silica, and ferric oxide; and it is a remarkable fact, that the sum of the weights of these is constant also at about 27 per cent. We will represent it by Pe.

Finally, the various accessory substances (titanium, vanadium, etc.) which occur even in the purest bauxites, present a constant total of about 3 to 4 per cent. These we represent by C.

Thus, the centesimal formula of the bauxites:

$$68 \text{ to } 70\ Al_2O_3 + 27\ (SiO_2, Fe_2O_3, H_2O) + 4 \text{ (sundry accessories)}$$

may be written in general form as A + Pe + C.

But the three variable elements of Pe have the singular prop-

erty of replacing one another, in whole or in part, separately increasing, diminishing, or totally disappearing, without change of the total of 27 per cent., and without altering the fixed mineral species, which is, according to our view, the bi-hydrate of alumina, forming the base of the mineral. These varying substitutions give rise to the different types of which we shall speak.

The Bauxite of Baux.—Before our discovery of the white bauxites of Villeveyrac (Hérault), there was only known the impure, more or less ferruginous and siliceous bauxite, reported from the village of Baux, by Berthier. Subsequently, the bauxites of Neustadt, Leoben (Austria), Ireland, etc., received mention. They constituted a vaguely characterized variety, containing the constant of alumina, A, with the elements of iron, silica, and water, often in equal thirds, constituting a variegated white and red (sometimes pink) bauxite of indefinite composition, but for which, in strictness, the formula

$$A + Pe\left[\tfrac{1}{3}H_2O, \tfrac{1}{3}SiO_2, \tfrac{1}{3}Fe_2O_3\right] + C.$$

could, perhaps, be employed. However, we have, in fact, to deal at Baux, not with a definite type of bauxite, but rather with a mixture of the types hereafter to be described.

Pale Bauxite.—As has been narrated above, Mr. Augé and the writer brought to light, at Villeveyrac, and introduced into industrial use, a variety which we have denominated, by reason of its peculiar appearance, pale bauxite. Its composition was practically novel. The ferric oxide has been almost entirely replaced with silica, giving a mineral with something of the aspect of a soft chalk, of creamy color, staining the fingers, often pisolitic, and having a composition which could be expressed by the formula:

$$A + Pe\left[\tfrac{1}{2}H_2O, \tfrac{1}{2}SiO_2\right] + C.$$

It will be seen that in this bauxite one part of water has been replaced with silica, while the ferric oxide has been relegated to the category C of accessory constituents.

It is upon this siliceous pale bauxite, which is attacked by acids with peculiar ease, that we inaugurated in France (at L'Oseraie, near Avignon), in 1875, the manufacture of neutral sulphate of alumina.

Red Bauxite.—Our investigations, under the circumstances above stated, and particularly the researches of Mr. Augé, led at last, about 1880, to the discovery in the departments of the south of France (Bouches du Rhône, Var, Alpes Maritimes, etc.), at Thoronet,

Cabasse, Luc, etc., of a third variety of bauxite, previously quite unknown. This type is still more characteristic than the preceding one.

Instead of being white, as at Villeveyrac, or striped with red, and pisolitic, as at Baux, it presents the appearance of a fine, homogeneous paste, breaking with splintery conchoidal fracture, and as dark-red as bloodstone; and it is so constant in its composition that we have been able to ship, during more than twelve years past, about 150,000 tons of it, upon a uniform guaranty of a minimum of alumina, which has always been fulfilled.

Here the silica has been almost wholly replaced with ferric oxide, and we have in this red type, so to speak, the inverse of the pale bauxite of Villeveyrac. It may be represented by the formula:

$$A + Pe\,[\tfrac{1}{2}H_2O, \tfrac{1}{2}Fe_2O_3] + C.$$

Here it will be seen that one part of water has been replaced with ferric oxide. The silica in the red variety, like the ferric oxide in the white variety, has been reduced to the position of an accessory constituent, under C, amounting, with vanadium, titanium, etc., to 2 to 4 per cent.

This bauxite, unsuited by reason of its contents in iron, for direct treatment with acids, was promptly demanded for manufacture in the dry way, namely, for the production of pure alumina by the sodium carbonate process of Salyndres; and it is the exploitation of this third type which has made, during the past ten years such astonishing progress.

Hyaline Bauxite.—Finally, there has been discovered in Alabama and Georgia, and in the Yellowstone National Park, a fourth variety of bauxite, in which silica and ferric oxide are not found, and two equivalents of water determine the type. It is this form which occurs as an easily discernible admixture in the siliceous bauxite of the Villeveyrac variety. As may there be observed, the mineralogical type remains the same, only the particles of bauxite have a more glaucous, translucent, slightly horny appearance, and are soft, easily scratched with the finger-nail. This is the bihydrate of alumina, and the formula is, therefore,

$$A + Pe\,[2H_2O] + C.$$

It is nothing else than amorphous hydrargyllite, nearly pure, with 3 to 4 per cent. of accessory constituents and 27 per cent. of water.

This variety has not yet been thoroughly tested by continuous exploitation and repeated analyses. It is yet in the beginning of its development, and what has been produced so far is a pale bauxite of the Villeveyrac type, but containing nodules of hydrargyllite in considerable abundance.

It will thus be seen that the four types of bauxite hitherto known (and which may occur sometimes more or less intermingled in the same deposit) are:

1. Mixed bauxite of Baux, . . $A + Pe \left\{ \begin{matrix} \frac{1}{3}H_2O \\ \frac{1}{3}SiO_2 \\ \frac{1}{3}Fe_2O_3 \end{matrix} \right\} + C.$

2. Pale bauxite of Villeveyrac, . . $A + Pe \left\{ \begin{matrix} \frac{1}{2}H_2O \\ \frac{1}{2}SiO_2 \end{matrix} \right\} + C.$

3. Red bauxite of the Var, . . $A + Pe \left\{ \begin{matrix} \frac{1}{2}H_2O \\ \frac{1}{2}Fe_2O_3 \end{matrix} \right\} + C.$

4. Pure bauxite of Alabama, . . $A + Pe\,[2H_2O] + C.$

And it may be affirmed that the basis of the bauxite is the natural bihydrate of alumina or hydrargyllite, with about 27 per cent. of water, which has not yet been, but may be some day, developed in workable deposits.

This natural bihydrate is the true mineralogical initial geyser-type, combining in definite proportions with two principal substances, ferric oxide and silica, by the substitution of these substances for its water of combination, in whole or in part.

This hydrated alumina has very manifold and energetic affinities, by virtue of the double part which it can play, as base and as acid.

In its combination with silica, to form the pale bauxites, it evidently takes the part of a base, uniting with silicic acid to produce a certain amount of basic silicate of alumina, if we may so term it, or, better, of white ultramarine. The silica is substituted for half the water of combination.

The probability of this theory is shown by the fact that we find bauxites containing about 27 per cent. of silica, and having the aspect of hard, opaque, polished lithographic limestone. In these, silica has totally replaced the water of combination, causing all the properties of hydrargyllite to disappear. This variety has never found commercial use; but it exists in large quantity, and may be called the siliceous type of bauxite, or natural white ultramarine.

The most curious variety is No. 3 of the above list, the inverse ferruginous variety, in which alumina plays the part of an acid

towards the ferric oxide which has completely replaced silica. Evidently, we have here a true ferric aluminate, in which the metallic oxide has been substituted for about half the water of hydration, forming the red type of the Var by a process precisely similar to that by which the white type of Villeveyrac has originated.

Here, also, we can find what I shall call the maximal ferruginous type (*le type ferrugineux-limite*), in which all the water of hydration has been thus replaced with ferric oxide. This occurs in abundant pisolitic concretions, presenting strong analogies with those of Berry, which latter are said to be themselves, perhaps, aluminates of iron.

Thus the bauxites find their place in the geological category of known types as geyser-products, between the clays proper and the ores of iron.

Eliminating the mixed type of Baux, included among the commercial bauxites on a preceding page, and adding the two extreme types already mentioned, we have five definite typical classes, as follows:

1. Hydrargyllite (the pure type of bauxite), $A + Pe[2H_2O] + C.$

Siliceous Bauxites.

2. Pale bauxite of Villeveyrac (an intermediary type: hydrargyllite, with basic silicate of alumina), ... $A + Pe\left\{\begin{matrix}\frac{1}{2}H_2O\\ \frac{1}{2}SiO_2\end{matrix}\right\} + C.$
3. Maximal siliceous bauxite (basic silicate of alumina), ... $A + Pe[SiO_2] + C.$

Ferruginous Bauxites.

4. Red bauxite of the Var (hydrargyllite with aluminate of iron), . . . $A + Pe\left\{\begin{matrix}\frac{1}{2}H_2O\\ \frac{1}{2}Fe_2O_3\end{matrix}\right\} + C.$
5. Maximal ferruginous bauxite (aluminate of iron), ... $A + Pe[Fe_2O_3] + C.$

We must not forget to mention that there exists, notably at Villeveyrac, a violet manganiferous bauxite, which may give room for a new type; and there is no reason why titaniferous, vanadic, barytic, calcic and other bauxites should not also be discovered. They certainly exist, but they have not yet attracted the attention of scientists or manufacturers. There are some in Alabama bauxites, the alumina of which is combined with organic matter.

In short, to resume our argument, the bauxites are nothing other than the inorganic *lacs* or *lakes*, more or less defined in proportions (but with a certain maximum in combination); and all substances capable of uniting with alumina hydrate may constitute, some day, mineralogically, a family of aluminous compounds in the humid way, as numerous as the aluminous family found in the dry way. We are only at the threshold of discoveries in this respect; for alumina combining in the dry way constitutes but one substance, Al_2O_3, whereas alumina combining in the humid way has three or four hydrates, possessing powerful and diverse chemical affinities. Hitherto, it is the bauxite family derived from the bihydrate which has become somewhat known; but it would be of great interest to discover the family derived from the monohydrate, and containing up to 85 per cent. of alumina. Bauxites from the higher hydrates, on the other hand (such as gibbsite, for example, which has three equivalents of water), will evidently present less interest, and will be lost among the ordinary clays and the various silicates, iron-ores and limes already known.

If, now, we would form, by way of suggestion, some idea of the entrance of the bauxites among our sedimentary rocks, we need only consider hot springs as depositing gelatinous alumina (absolutely as they do gelatinous silica), and the different substances, ferric oxide, silica, titanic acid, etc., as forming, with this deposit, silico-aluminates, definite aluminates or inorganic *lacs*, such as have recently become known. In the Yellowstone National Park this hydrargyllite is still deposited to-day, the substance which is the primary basis of all the bauxites over the globe, in quantity much greater than has been suspected heretofore. According to Hayden, this mineral can be seen combining with iron, silica and lime.

Geologically speaking, the phenomenon which gave rise to the bauxites in Europe occurred with great intensity towards the end of the Cretaceous epoch, and has never been repeated. As a result, these minerals have a definite geological age. They take their place between the Urgonian and the Cenomanian, where they form to-day a very useful geological horizon among the Upper Cretaceous terranes. But this branch of the subject should be treated in a separate paper.

Such is the conception which we have formed concerning the bauxites after twenty years spent in their exploitation. General experience will, perhaps, some day present complete confirmation of our views.

THE GEOLOGICAL RELATIONS OF THE SOUTHERN APPALACHIAN BAUXITE-DEPOSITS.

BY C. WILLARD HAYES, PH.D., WASHINGTON, D. C.*

(Virginia Beach Meeting, February, 1894.)

Introduction.—The recent developments in the metallurgy of aluminum and its consequent rapidly growing use in the arts are at present attracting considerable attention to its ores. The success of the Hall electrolytic process, by which the metal is obtained directly from the oxide, has removed bauxite from the position of a rather uninteresting mineral to that of an important ore. Its production on a commercial scale is so recent, particularly in this country, that little attention has yet been paid to its mode of occurrence and origin. Any investigation, therefore, which throws light upon these points should be of interest to mining engineers at the present time, especially if the conclusions afford a basis for estimating the probable extent and value of deposits already known and a means of directing search for others.

Two localities in the United States have so far yielded bauxite in commercial quantities. These are in Arkansas and in the Coosa valley of Georgia and Alabama. According to descriptions of the Arkansas deposits by Branner, they bear little resemblance to those in the Southern Appalachians and their geological relations appear to be entirely different. Only the latter deposits are considered in the following paper. These have been briefly described by McCalley† and Spencer.‡ They were visited by the writer during the past summer and somewhat carefully examined in connection with the study of the areal geology of the region.

Location of the Deposits.—The ore is found irregularly distributed within a narrow belt of country extending from Adairsville, Ga.,

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* Published by permission of the Director of the United States Geological Survey.

† "Alabama Bauxite," by Henry McCalley.—*Proc. Ala. Industrial and Sci. Soc.*, 1893.

‡ "The Geology of Northwestern Georgia," by J. W. Spencer.—*Geol. Sur. of Georgia, Atlanta*, 1893.

southwestward, a distance of 60 miles, to the vicinity of Jacksonville, Ala. The only points at which it has been worked on a commercial scale are at Hermitage furnace, 5 miles north of Rome, Ga., near Six Mile station, south of Rome and in the Dyke district near Rock Run, Ala. The two last-named localities were most carefully studied, but it is believed that the relations of the ore in these will hold throughout the entire belt.

In order to explain the conditions under which the ore occurs, a brief account of the geology of the region will be given. The areal distribution of the formations which range in age from Cambrian to Carboniferous is shown upon the accompanying map, Fig. 1, which embraces the portions of the ore-bearing belt at present productive.

Stratigraphy of the Region.—The oldest rocks of the region are of Cambrian age and are subdivided on lithologic grounds into two formations, the Rome sandstone below and the Connasauga shale above. The former consists of 700 to 1000 feet of thin-bedded purple, yellow and white sandstones and sandy shales. In the southern portion of the region the Rome sandstone is replaced by the Weisner quartzite, which consists of a series of interbedded lenticular masses of conglomerate, quartzite and sandy shale. It apparently represents delta deposits contemporaneous with a part or the whole of the Rome sandstone. These rocks form Weisner and Indian mountains, and in the latter they attain a thickness of 10,000 feet or more.

The Connasauga is between 2000 and 3000 feet in thickness. It consists at the base of fine aluminous shales; the upper portion is more calcareous, and locally passes into heavy beds of blue seamy limestone.

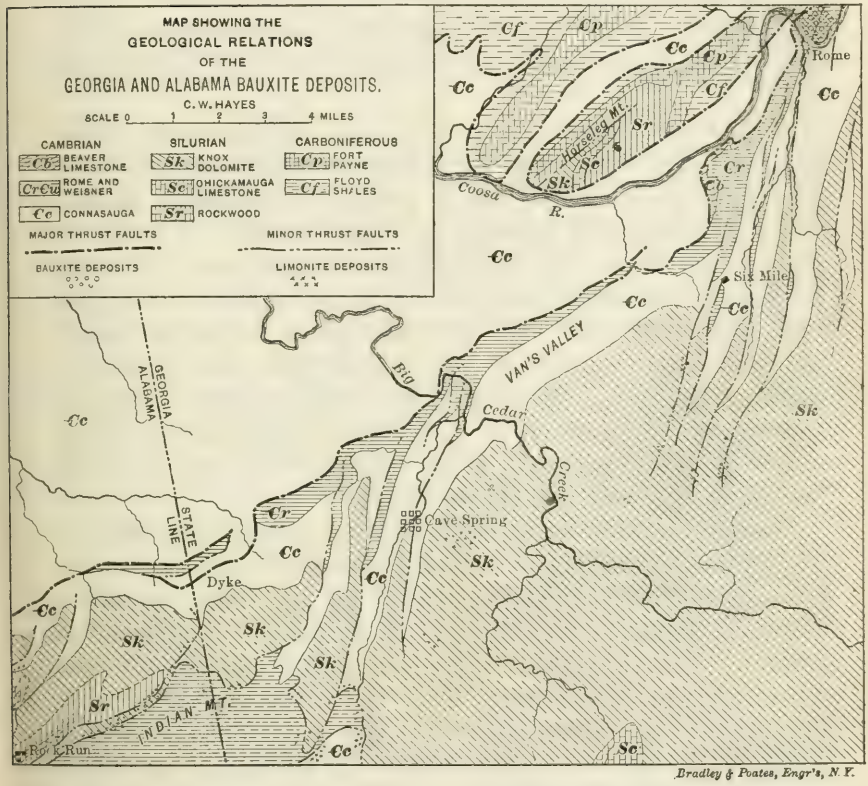
Above the Connasauga shale is the Knox dolomite, the most uniform and persistent formation of the southern Appalachian region. It consists of from 3000 to 4000 feet of gray, semi-crystalline, siliceous dolomite. The silica is usually segregated in nodules and beds of chert. These remain upon the surface, and with the other insoluble constituents form a heavy residual mantle covering all the outcrops of the formation. It is associated with these residual materials that the extensive deposits of limonite and bauxite are found.

Over a considerable portion of this region, a stratigraphic break occurs above the Knox dolomite, which is separated from the succeeding formations by an erosion-interval. Next above the dolomite, where the succession is unbroken, is the Chickamauga limestone which, in turn, is followed by the Rockmart slate. For the purposes

of the present paper, these and succeeding formations have little interest, and the description of them will be omitted.

Geologic Structure.—The geologic structure of the region under consideration is exceptionally complicated. In addition to the folds which characterize the entire Appalachian province and whose form is familiar to all, the region is intersected by two series of faults.

Fig. 1



Its very intimate connection with the bauxite-deposits makes a somewhat detailed description of the structure necessary.

In the northern portion of the ore-bearing belt the structure is quite simple; the folds are broad and but little faulted. In the central portion, between Rome and Cave Spring, it is more complicated and the numerous narrow folds are commonly faulted. In the southern portion, particularly between Rome and Mt. Weisner, occurs the most complicated structure known in the southern Appalachians.

A line of hills borders the Coosa-Oostanaula valley upon its south-

eastern side from Calhoun, Ga., to Mt. Weisner. These hills are composed of Rome sandstone dipping toward the southeast and they form the northwestern limit of the ore-bearing belt. East of this line of hills is a valley underlain by Connasauga shales, also dipping toward the southeast, under the Knox dolomite. From Rome southward a number of narrow shale valleys penetrate a few miles within the border of the dolomite. Each of these valleys corresponds in position with a narrow anticlinal fold, in every case faulted upon its western side. Beyond these narrow folds a broad syncline of dolomite extends eastward 15 or 20 miles to the limit of the metamorphic rocks.

In the region between Cave Spring and Rock Run the original folds have been almost entirely obliterated by subsequent faulting. Also in this region folding was in progress during middle Silurian time. The folds then formed were deeply eroded, and across the edges of the tilted strata subsequent formations were laid down. During this and later periods of folding, the massive beds of Cambrian quartzite formed rigid buttresses which themselves resisted folding and against which the less resistant strata were crushed by horizontal pressure. The effects of great compression were thus concentrated within narrow belts. It is in such a belt, along the western base of the Indian mountain, in the vicinity of Rock Run, Ala., that the largest deposits of bauxite are found.

The faults thus far mentioned are of the ordinary type found in the southern Appalachians. Having been developed from steep or overturned folds by a continuation of the horizontal pressure to which the latter are due, they are all thrust-faults hading to the upthrow. The inclination of the fault-plane is steep, usually from 40° to 60° with the horizon. They are in this region only from three or four to a dozen miles in length, and the displacement of corresponding beds on opposite sides of the fault-plane varies from a few feet to several thousand.

Other faults are found in this region, however, which have certain less common features. These have been described elsewhere* and only the main characteristics will be given here. They constitute the principal dislocations of the region, and while they resemble the former class in being the result of horizontal compression they differ in the angle of hade and the amount of displacement. The fault-

* "The Overthrust Faults of the Southern Appalachians," by C. W. Hayes.—*Bull. Geol. Soc. Am.*, vol. ii., 1891, pp. 141–154.

plane is usually nearly horizontal, and in some cases it has been distorted by subsequent folding together with the underlying and overlying strata. When this subsequent folding has placed portions of the overthrust strata in a position where they would be protected from erosion a minimum measure of the horizontal displacement is afforded. In one case in this region the visible displacement is about four and a half miles, and the total displacement is probably very much greater. Two faults of this class are shown upon the accompanying map, Fig. 1. The one with which the present paper is chiefly concerned extends from Rome southwestward to Mt. Weisner along the western base of the Rome sandstone hills above mentioned. Following the southeastern border of the Coosa valley, it has been named the *Coosa fault.* Although its horizontal displacement cannot be directly proved to be as great as that of the Rome fault, the inference from observed differences in contemporaneous deposits upon opposite sides is that the displacement is certainly very great. There is also evidence that the minor faults with steep hade are older than the broad major thrusts, and that a considerable period of erosion separated the two systems.

Thus it appears that the region has been the seat of orogenic activity from very early geologic time. In the middle Silurian, folds were formed and their crests were eroded. In late Carboniferous time, during the great Appalachian revolution, a second generation of folds was born and minor faults were developed, their distribution being determined largely by variations in the strata, particularly the resistant masses of Cambrian rocks. And finally, after another long period of erosion, the folded and faulted mass was displaced upon a nearly horizontal thrust-plane.

Rock-Weathering.—The region shows evidence of having been subjected for a very long time to conditions favoring sub-ærial rock-decay, and only moderately active degradation. Hence the surface is deeply covered with a mantle of residual material, consisting of the insoluble portions of the sub-terranes. This residual mantle is thinnest over areas of shale and slate, thicker over areas of limestone in which the insoluble matter makes up a small portion only of the rock-mass, and thickest over areas of the Knox dolomite. The insoluble constituents of the latter are small quantities of iron and alumina and much larger quantities of silica in the form of chert. Hence the residual mantle is composed of ferruginous clay in which large amounts of chert are imbedded. The deposit often attains a thickness of more than 50 feet, and in some cases reaches 100

feet or more; so that the dolomite itself is rarely seen, except in the stream-channels.

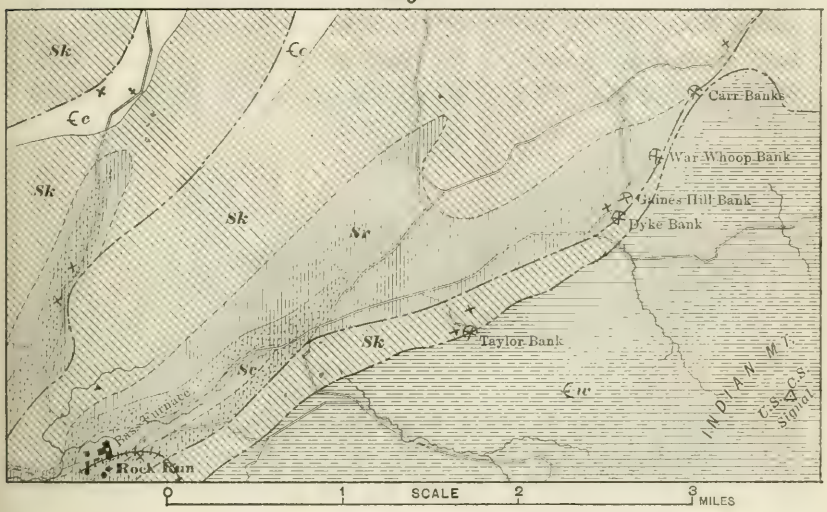
Mingled with the residual deposits which characterize each terrane, especially in the vicinity of the high quartzite ridges, are greater or less quantities of foreign *débris*. Near the base of Indian Mountain this frequently masks the characteristic residual deposits to such an extent that it is extremely difficult to determine the areal distribution of the several formations.

A further source of obscurity, in the same region, is the incipient metamorphism which has affected some of the rocks, particularly the chert of the Knox dolomite. The effect generally produced has been a change of the amorphous or chalcedonic silica, composing the chert, into finely granular quartz. The chert loses its coherence and forms a white chalk-like substance, which readily disintegrates at the surface. When the metamorphism has been carried a step further, it results in the secondary deposition of silica, forming a peculiar jaspery rock, in which the original character of the chert is wholly lost. In many cases it is impossible to determine whether this jasper has been derived from chert, sandstone or quartzite. The metamorphism is intimately associated with the faulting, and was doubtless produced by thermal water, so that it belongs to a class of changes no longer taking place at the surface in this region.

Form of the Ore-Bodies.—The deposits in the Rock Run district are typical of the entire region and will be described in some detail. Their location is shown on the map of the district, Fig. 2. Four bodies of the ore were being worked in 1893 on a considerable scale, and all show practically the same form. The southernmost of the four, called the Taylor bank, is located $3\frac{1}{2}$ miles northeast of Rock Run, near the western base of Indian Mountain. Although the heavy mantle of residual material effectually conceals the underlying rocks, the ore appears to be exactly upon the faulted contact between the narrow belt of Knox dolomite on the northwest and the sandy shales and quartzites of Indian Mountain on the southeast. The ore is covered by 3 or 4 feet of red sandy clay in which numerous fragments of quartzite are imbedded. The ore-body is an irregularly oval mass, about 40 by 80 feet in size. Its contact with the surrounding residual clay, wherever it could be observed, appeared to be sharp and distinct, and, about the greater portion of its circumference, very nearly vertical. A certain amount of bedding is observable in the ore-body, although no trace of bedding can be detected in the surrounding residual material. Upon the northwestern or

down-hill side of the ore-body, this bedding is very distinct. Layers of differently colored and differently textured ore alternate in regular beds, a few inches in thickness, and above these are thinner beds of chocolate and red material, probably containing considerable kaolin. These beds have a steep dip, somewhat greater than the slope of the hill-side, but in the same direction. They are not simply inclined planes, however, but are curved, so as to form a steeply-pitching trough. With increasing distance from the ore-

Fig. 2



GEOLOGIC MAP OF THE
ROCK RUN DISTRICT,
ALABAMA
SHOWING THE LOCATION OF BAUXITE AND LIMONITE DEPOSITS

CONNASAUGA SHALE Ce | WEISNER QUARTZITE Ciw | KNOX DOLOMITE Sk | ROCKWOOD SHALE Sr | BAUXITE | LIMONITE | FAULTS

Bradley & Foates, Engr's, N.Y.

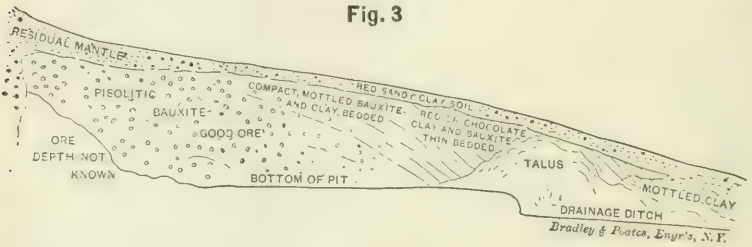
body, the lamination becomes less distinct, and the beds pass gradually into a homogeneous mottled clay. The accompanying section, Fig. 3, shows these relations of the ore and residual mantle.

At the Dyke bank, Fig. 2, about a mile northeast of the one above described, the stratification is well shown in portions of the deposit. Beds of yellow and gray, fine-grained material, alternate with others of pisolitic ore. The beds dip at an angle of about 40°, and are curved so as to form a steep trough. The compact material also shows distinct cross-bedding; both primary and secondary planes dipping in the same direction.

In the Gaines Hill bank, about 250 yards north of the Dyke bank, the ore-body shows a more regularly oval form than in most of the other deposits, and is also somewhat dome-shaped, swelling out laterally from the surface downward, as far as the working has progressed.

Although some of the workings have gone to a considerable depth (in a few cases 50 feet or more), the bottom of the ore-body has not been reached in any case. The ore varies in composition with depth, but not in a uniform manner, nor more than do different portions at the same depth. The deepest pits have not gone below the base of the surrounding residual mantle, so that no observations have yet been made with regard to the relations between the ore and the country-rock; and nothing has yet been observed which warrants

Fig. 3



the conclusion that the ore if followed to sufficient depth, will be found inter-bedded with the underlying formations, or even that it will be found occupying cavities in the limestone—although the latter is quite possible.

Structure of the Bauxite.—The ore shows considerable variety in physical appearance, though it generally has a very characteristic pisolitic structure. The individual pisolites vary in size from a fraction of a millimeter to 3 or 4 centimeters in diameter, although most commonly the diameter is from 3 to 5 millimeters. The matrix in which they are imbedded is generally more compact and also lighter in color. The larger pisolites are composed of numerous concentric shells, separated by less compact substance or even open cavities, and their interior portions readily crumble to a soft powder.

In thin sections the ore is seen to be made up of amorphous flocculent grains, and the various structures which it exhibits are produced by the arrangement and degree of compactness of these grains. The matrix in which the pisolites are imbedded may be composed of this flocculent material segregated in an irregularly globular form

or in compact oölites, with sharply-defined outlines. Or both forms may be present, the compact oölites being imbedded in a matrix composed of the less definite bodies. In some cases the interstices between the oölites are filled either wholly or in part with silica, apparently a secondary deposition.

The pisolites also show considerable diversity in structure. In some cases they are composed of exactly the same flocculent grains as the surrounding matrix, from which they are separated by a thin shell of slightly denser material. This sometimes shows a number of sharply-defined concentric rings, and is then distinctly separated from the matrix and the interior portion of the pisolite. The latter is also sometimes composed of imperfectly-defined globular masses, and in other cases of compact, uniform, and but slightly granular substance. It is always filled with cracks, which are regularly radial and concentric, in proportion as the interior substance has a uniform texture. Branching from the larger cracks, which, as a rule, are partially filled with quartz, very minute cracks penetrate the intervening portions. Thus the pisolites appear to have lost a portion of their substance, so that it no longer fills the space within the outer shell, but has shrunk and formed the radial cracks. No analyses have been made of the different portions of the pisolites or of the pisolites and matrix separately, and it is impossible to say whether any differences in chemical composition exist. It may be that some soluble constituent has been removed from the interior of the pisolites, but it is more probable that the shrinking observed is due wholly to desiccation.

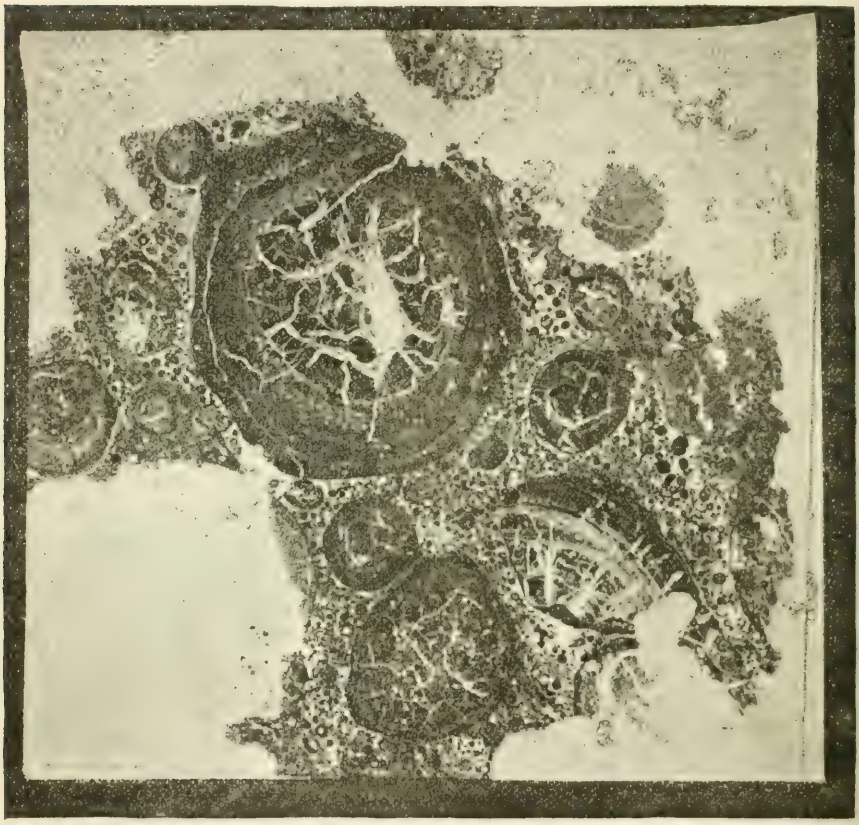
Scattered throughout the ground-mass are occasional fragments of pisolites, whose irregular outlines have been covered to varying depths by a deposit of the same material as forms the concentric shells, and thus have been restored to spherical or oval forms.

These various features in the structure of the ore are shown in Fig. 4, which is from a photograph of a thin section, enlarged 5 diameters.

Origin of the Deposits.—The bauxites of France are apparently residual deposits from the decay of basalt. Remains of the constituent minerals of the parent rock appear in the ore, as well as traces of the original rock-structure. The ore also occurs disseminated throughout the residual material, and not, as in the case above described, in compact bodies with well-defined limits. The Arkansas deposits, on the other hand, occur as regularly stratified beds in rocks of Tertiary age. They are found only near the contact with

certain eruptive rocks, and their origin seems to be closely connected with the latter. In both localities, therefore, the relations of the ore differ so widely from those of the Georgia-Alabama deposits that their origin must be explained on a different theory.

FIG. 4.



Thin section of bauxite, enlarged five diameters, showing pisolites with concentric shells and shrunken interiors; also oölites imbedded in the matrix.

No eruptive rocks, either ancient or modern, are found in the vicinity of the latter, nor are there any rocks in this region which, by weathering, could yield bauxite as a residual product. Hence, any satisfactory explanation of the origin of these deposits must give the source from which the material was derived, the means by which it was transported, and the process of its local accumulation.

As already stated in describing the stratigraphy of the region, the ore is associated with the Knox dolomite or with calcareous sandy shales immediately overlying the dolomite. The Connasauga, consisting of 2000 feet or more of aluminous shales, invariably underlies the dolomite at greater or less distance beneath the ore-bearing regions, and is probably the source from which the alumina was derived.

The faults of the region have been briefly described. Undoubtedly such enormous dislocations of the strata generated a large amount of heat. The fractures facilitated the circulation of water, and for considerable periods the region was probably the seat of many thermal springs. These heated waters appear to have been the agent by which the bauxite was brought to the surface in some soluble form and there precipitated.

The chemical reactions by which the precipitation was effected are not well understood, and the conditions were not such as can be readily reproduced in the laboratory. Of the few soluble compounds of aluminum which occur in nature, only the sulphate and the double sulphate of potash and alumina need be considered.

The oxygen contained in the meteoric waters percolating at great depths through the fractured strata would readily oxidize the sulphides disseminated in the aluminous shales. Sulphates would thus be formed by a process strictly analogous to that commonly employed in the manufacture of alum. Probably the most abundant product of the process in nature was ferrous sulphate. Some sulphate of aluminum must also have been formed together with the double sulphate of potassium and aluminum, especially in the absence of sufficient potash to form alum with the whole.

In its passage from the underlying shales through several thousand feet of dolomite the heated water must have become highly charged with lime, in addition to the ferrous and aluminous salts already in solution. But calcium carbonate reacts upon aluminum sulphate and to some extent also on alum, forming a gelatinous or flocculent precipitate which consists of aluminum hydroxide and the basic sulphate. This reaction may have taken place at great depth and the resulting flocculent precipitate may have been brought to the surface in suspension. From analogy with pisolitic sinter and travertine now forming, such conditions would appear to be highly favorable for the production of the structures actually found in the bauxite. The precipitate was apparently collected in globular masses by the motion of the ascending water, and constant changes

in position permitted these to be coated with successive layers of more compact material. Finally, after having received many such coatings, the pisolites were deposited on the borders of the basin, and the interstices were filled by minute oölites formed in a similar manner or by the flocculent precipitate itself. Slight differences in the conditions prevailing in the several springs, such as concentration and relative proportion of the various salts in solution, also temperature and flow of the water, would produce the variation in the character of the ore observed at different points.

The bedding observed in the bauxite-deposits may have been produced by the successive layers deposited on the steeply inclined outlet of the basin. After the cessation of the spring-action, surface-creep of the residual mantle from the higher portions of the ridges covered the deposits to varying depths, as they are found at present.

A small portion of the ferrous sulphate was oxidized and precipitated along with the bauxite, but the greater part was carried some distance from the springs and slowly oxidized, forming the widespread deposits of limonite in this region.

Conclusion.—This explanation of the origin of these deposits is certainly in accord (1) with the arrangement of the larger deposits along the fault lines; (2) with the reactions, so far as they are known, of the aluminum compounds in nature; and (3) with the process now developing analogous pisolitic structures. If subsequent investigations prove it to be correct, it should enable the engineer to estimate the probable extent and value of the deposits; it should suggest economic methods of working those already known and direct the search for others; and, finally, it should emphasize the importance of a knowledge of the geologic structure of the region in the future development of these important deposits.

CORRELATIONS IN THE COAL-ROCKS WEST OF POCAHONTAS, FLAT TOP, VIRGINIA.

BY C. R. BOYD, WYTHEVILLE, VA.

(Virginia Beach Meeting, February, 1894.)

In 1874 I received an appointment in the corps organized by Col. William P. Craighill, U. S. Engineers, to survey the New or Upper Kanawha river for the purpose of ascertaining the practicability and

cost of the improvement of that stream by the government for purposes of navigation.

Being placed in charge of the slack-water party, under the immediate supervision of civil assistant N. H. Hutton, C.E., I had peculiar facilities and advantages for the study of all rock-formations presented to view in that river and some of its tributaries, such as Greenbrier and Bluestone rivers.

Previously, in 1872, I had been called upon by Col. Craighill to make a report upon the ores and coals of Upper New river, in Virginia; and it was one of my chief duties in these surveys to note carefully the position and character of all rock-formations, with a view to their possible use, either as natural firm foundations for dams and locks, or as building-materials for construction.

In this way I really began my studies of the correlations of the West Virginia, Virginia, and Kentucky coal-series; and I believe I began at the right end of it, where the whole series is cut in one long cañon. At that time it was a study rather of physical and structural features; since then the fauna and flora, as well as the economic relations of the coal-seams in those massive stratifications, have been, from time to time, the objects of earnest study over this field, 200 miles in length by about 60 miles in width.

It was in 1872 that I first visited the Flat Top coal-field, and found in the entire series there 46 feet of coal; and in 1874 I saw, for the first time, the Quinnimont series at or near Nuttallsburg, on New river, dipping northwest, and bringing in, lower down the river, the massive ledges of conglomerate on top of these coals.

During Mr. Lathrop's administration of the Pocahontas mine I found a confirmation of my report, made to Col. Craighill in 1872, of the quantity of coal in that series. But it was not until October 31, 1885, that I became fully convinced of the identity of the Flat Top and Quinnimont coals.

In the distance across from the river to Pocahontas the conglomerate pebbles had changed from larger to smaller size. In later years I have been able to study all these changes and correlations throughout the great coal-field comprised in West Virginia, Virginia, Kentucky, and eastern Tennessee, particularly through McDowell county, West Virginia; Buchanan, Tazewell, Russell, Dickenson, Wise, Scott, and Lee counties, Virginia; Bell, Harlan, and Pike counties, Kentucky; and Claiborne county, Tennessee.

As is now well known, the conglomerate I have mentioned as overlying the Flat Top coal-series comes down below the tops of the

hills as we proceed west from Flat Top, both on Tug and Dry forks of Sandy river, in McDowell county, West Virginia. It then passes through the divide, with very little change of level, westwardly and southwest, between the Dry and Louisa forks of Sandy river, several hundred feet below the tops of the highest ridges, and shows itself high up on the sides of the hills on Dismal creek and its affluent, Hurricane fork. The same ledges, traced through with care, may be certainly recognized on the Clinch Valley slope of Sandy ridge, throwing the coal-seams mined at Richlands, Big creek, Town Hill, and Coal creek into the New River Flat Top series.

The same conglomerate, coming west from Dismal creek with greater and less undulations, appears on the head-streams of the main fork of Louisa river, in Buchanan county, passing on westward through the ridges bounding the Garden fork of Louisa river, and through the divide between the waters of the Louisa and Russell's forks of Sandy river, and comes out again fairly to view at Hurt's, on that stream, several hundred feet below the tops of the hills. (With reference to particular hills and streams, exact elevations are taken.)

These identical ledges of conglomerate then pass west and southwest, without interruption, behind or northward of the Big Ax upthrow, toward the waters of Frying Pan creek and the head-waters of Dumps creek (the latter an affluent of Clinch river), and litter nearly all those streams with pebbles.

On Dumps creek this conglomerate is nearer the crests of the hills, and begins to inspire the hope that the full thousand feet of the Pocahontas Flat Top series will be seen, so that the famous No. 3 seam will again emerge in full view; but the bottoms of these streams stop us fully 150 feet above the level at which we may reasonably expect to find Pocahontas No. 3. On this stream the conglomerate is fully 40 feet thick, with about 30 feet of thin-bedded alternate limestones and shales above it, the limestones showing *Chonetes variolata* and *ornata*, *Streptorynchus umbraculum*, and *Productus longispinus*.

Passing then westwardly along Sandy ridge to the head-waters of Lick creek, Russell creek, and Tom's creek, tributaries of Clinch river, we find that Prof. J. P. Lesley's Sheep Rock conglomerate is our identical friend that we have followed assiduously for so many miles from New River and Pocahontas.

The declaration of Prof. Lesley that this great conglomerate plate overlies all the coals of this region was either made to the writer when he saw Prof. Lesley at his house in 1881, or was contained in

a memoir then handed him. My observations there seem to be confirmed by distinguished authority.

On the crests of the hills of Tom's creek, near Coeburn and overlooking the Clinch Valley branch of the Norfolk and Western railway, we might leave this conglomerate for the present reposing above the Big Tom creek series, holding the so-called Imboden seam and others, such as the Jawbone, the Kennedy, and the two Banner seams; but we feel constrained to add a few more words. From Tom's creek westward through Tacoma, about 8 miles by air-line to Norton, there is a territory in which further and very close identification is quite necessary to prove that the so-called Imboden seam of Tom's creek is the real Imboden seam of Norton and the "Pocket," nearer Big Stone Gap. I doubt it. There is, indeed, some probability that the Big Tom's creek coals and rocks sink, and, carrying the overlying conglomerate with them, west of Tacoma, possibly pass entirely beneath the Norton and "Pocket" series, rising again quite abruptly to the south of Norton and the "Pocket." Witness the dip of the rocks as we go westward from the top of Sandy ridge, overlooking Tacoma on one side and Gladeville on the other. These rocks soon begin to dip as we go toward Norton from there, and descend at the rate of 300 feet per mile. This explanation may appear to conflict with some views already expressed, I believe, with reference to the northeastward continuation of the Stone Mountain axis, as we proceed from the south northeastward across Gess river, between Tacoma and the mouth of Tom's creek. But those differences of opinion which now exist, or may hereafter arise, with all the interesting questions involved in these correlations, it seems to me, can only be solved satisfactorily by systematic and laborious research, particularly along faulted and eroded lines, together with a thorough study of the lithology and palæontology of each individual member of this entire series of rocks.

In closing, it may be pertinent to remark that, on Bennett's fork of Yellow creek, three miles westward from Middlesborough, Ky., near the level of that creek, these sub-conglomerate coals are probably now being mined, from which seams we can readily trace the well-known series up through the Middle to the Upper Coal-Measures, almost to the crest of the highest coal-rocks below the Permian.

IRON-ORES OF EAST TEXAS.

BY W. KENNEDY, AUSTIN, TEXAS,

(Late Assistant Geologist, Geological Survey of Texas).

(Virginia Beach Meeting, February, 1894.)

Introductory.

Throughout northeastern Texas we have an extensive series of iron-ore deposits, occupying portions of nineteen counties and having approximately an actual ore-covered area of 1000 square miles.

The existence of these ores has long been known. Kennedy* says: "Iron-ore is distributed in profusion throughout Texas, being found almost everywhere except in the level region of the coast." Before this, C. Newell† had mentioned the existence of these ores thus: "Iron is found in abundance in the eastern, northern and middle portions of Texas;" and in 1849 Edward Smith‡ said: "We met with immense hills of iron which were said to be, and appeared to be, of excellent quality, in Cass and Titus counties. . . . . It is well known that Texas abounds with iron of first-rate quality." In 1859, Dr. Shumard‖ reports that detailed surveys had been made in several counties, and says: "Our labors have demonstrated the important fact that we have a vast iron-region in the eastern part of the state, embracing considerable areas in Cass, Harrison, Rusk, Panola, Smith, San Augustine and Shelby counties. The ore-deposits belong to the Tertiary era and consist chiefly of hematites and limonites, of which there are several varieties. We have also found in this district extensive beds of carbonate of iron." Dr. Shumard gives no very exaggerated description of Cass county§ as it then existed when he says: "Cass county alone is capable of supplying a number of furnaces with an abundance of excellent iron-ore for many years." Of Rusk county he says: "Our de-

* *Texas*, by W. Kennedy, 1842, vol. i., p. 117.

† C. Newell, *History of the Revolution in Texas*, 1838, p. 172.

‡ *Account of a Journey Through Northeast Texas*, 1849, pp. 36-37.

‖ Appendix to *Partial Rep. on Geol. of Western Tex.* Reprint 1886, p. 142.

§ *Op cit.* Cass county then included Marion.

tailed examinations in Rusk county have developed the occurence of almost inexhaustible deposits of workable hematite, similar to that found in Cass, while our general surveys in Cherokee, Nacogdoches and the other counties above enumerated have convinced us that farther explorations will reveal there also the existence of equally extensive accumulations of this important element of state wealth." S. B. Buckley, who succeeded Dr. B. F. Shumard as State Geologist, gives, in his first annual report, published in 1874,* a short account of the ores of Cherokee, Nacogdoches, Bowie, Cass and Marion counties, and in the same year there was published, by A. R. Roessler, a map of the state purporting to show the mineral localities.

The next information we have regarding the iron-ores of this district is contained in Executive Document No. 195, First Session of the Fiftieth Congress, entitled *Report on the Iron Regions of Northern Louisiana and Eastern Texas*, by Lawrence C. Johnson. From this report very little beyond the most cursory information is obtainable.

These are all the authorities we have regarding the quantity or quality of the east Texas iron-ores prior to the establishment of the present Geological Survey in 1888.

On the establishment of the present Survey, Dr. Penrose made a reconnoissance of east Texas, and a general summary of his examinations of the iron-ores of the region appears in the first annual report of the Survey.† The summary, although correct in its general features, does not deal with the ore-question in detail. In 1890, the writer, with his associates, Messrs. J. B. Walker and J. H. Herndon, took up the whole question in detail, and made careful and accurate detailed surveys of the whole area, many of which appear in the second annual report of the Survey. Since then the writer has alone worked out many of the details of these surveys, as well as others made since. During the course of these examinations many specimens were collected and analyses made, the ore-deposits were located and mapped, and their thickness and relative geological positions determined.

The general public, and even some of the best authorities on iron manufacture, appear to have had little or no knowledge of the history of the iron-business in Texas. Even such a generally well-

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* *First Ann. Rep. Geol. Surv. of Tex.*, 1874, pp. 17–21.

† *First Ann. Rep. Geol. Surv. of Tex.*, 1889, pp. 65–84.

informed writer as Mr. Swank says, on page 111 of his Statistics of the Iron and Steel Production, in the Tenth Census, vol. ii.

"Texas does not appear to have had any iron enterprises of any kind before the civil war; but three small furnaces are reported to have been abandoned at the close of the war. They were probably built during its continuance to meet the necessities of the Confederate government. In 1869, a charcoal-furnace was built at Jefferson, in Marion county, which was rebuilt in 1874. It was in operation in 1880, and was then the only furnace in the state. It is called Kelly furnace, after Mr. G. A. Kelly, the president of the Jefferson Iron Company, by which it is owned. It uses brown-hematite ore found in the neighborhood."

The first iron-furnace of which we have any positive knowledge was built in Cass county by Mr. J. S. Nash about the year 1855, and was in full operation, making "an excellent quality of iron," when visited by Dr. Shumard in 1859. In the same year (1859) Mr. Reese Hughes built a furnace near Hughes Springs in Cass county, but did not commence operations until 1861.

Several small furnaces or bloomeries, of which we have no record of any value, appear to have been in operation about 1860. At an extra session of the Legislature a joint resolution was passed recounting that inexhaustible supplies of iron-ore existed in Marion and Cass counties, and foundries were in successful operation, and inviting the government of the Confederate States "to consider the propriety and importance of establishing a foundry and manufactory for the manufacture of ordnance and arms for the Confederate States."

The Confederate government accepted the invitation given in this resolution and took charge of and operated some of these furnaces, amongst others Hughes', Young's and Philleo's furnaces and Nechesville, Montalbo and McLain bloomeries for the manufacture of gun-barrels and other munitions of war.

In 1863, the Ninth Legislature reorganized the Military Board and passed an act making it the duty of the Board "to erect and put into operation one or more furnaces or forges and other works for the manufacture of iron," and providing "that all iron manufactured by said Board or under its direction not needed for the defence of the State or in performance of their duties shall be sold to the people." Two acts incorporating the Texas Iron Company and the Sulphur Fork Iron Company were passed by this Legislature, together with another important act entitled "An Act to encourage and promote the introduction and operation of manufacturing industries within the State of Texas." In this act the State offered a

section of land for each one thousand dollars invested in the erection of such manufactories.

The first and, so far as we know, the only company to take advantage of the provisions of this act was the Sulphur Fork Iron Company. This concern had been incorporated by the same Legislature, December 4, 1863, and on June 15, 1864, began operations with a furnace 34 feet square and 36 feet high, having a daily capacity of about 8 tons. The products were chiefly pig-iron and hollow-ware. The ore used was a limonite obtained in the immediate vicinity of the furnace, and the fuel was charcoal. After a troubled existence of nearly a year it closed down about April 1, 1865. In July of the same year, B. W. Musgrove, sent by General C. E. Hovey to report on the condition of the works, recommended their operation by the United States government. This was not done, however, and land claims under the act of 1863 being presented to the State government, Governor Throgmorton, in 1866, sent W. E. Estis to examine and report on the condition of the works and at the same time make an estimate of their value. Mr. Estis valued the plant, machinery, buildings and such other materials as he could find at $97,500. Shortly after this the machinery was removed and the buildings were torn down. At the time of the writer's visit in 1890, the whole place was covered with a dense timber-growth and nothing but a small heap of cinder was left to show that it had ever been anything else. Many brick chimneys, however, throughout the county stand to-day as mute witnesses of the destruction of the last furnace of a troubled time.

With the destruction of the Sulphur Fork furnace closed the initial period of the attempts to manufacture iron from east Texas ore. With the exception of the Nash furnace, which had long before gone out of blast, these furnaces and bloomeries were all of abnormal growth, and their existence was due solely to the exigencies of war. The closing of the war and the return of peace with the more stable conditions of trade, the want of a local market for their products and the great cost of transportation to the more distant trade centers operated against them, and, one by one, they fell into disuse and eventual destruction.

The want of a good local demand is forcibly illustrated in the history of the Loo Ellen furnace. This furnace was erected in 1869 and put into blast in 1870 by Mr. G. A. Kelly, who operated it until 1882, when, owing to business difficulties, it passed into the hands of the Marshall Car Wheel and Foundry Company. Al-

though the capacity of the furnace was only ten tons daily, the local demand was so small that in 1886, four years later, the company had accumulated a stock far beyond their immediate necessities, and the furnace, being in need of repairs, was closed down, and is now only a ruin.

A new era began in 1885. The state government built a new penitentiary at Rusk and erected the "Old Alcalde" furnace in the midst of one of the extensive ore-fields. This furnace was designed chiefly to provide employment for the state convicts and incidentally to demonstrate the availability of the iron-ores of that portion of the state for manufacturing purposes. This furnace passed through a number of vicissitudes of various kinds until it was reconstructed by Mr. John Birkinbine, and is now successfully producing pig-iron of various grades and cast-iron water-pipes, as well as many other castings, direct from the furnace.

The ultimate apparent success of the "Old Alcalde" gave rise to considerable speculation in iron-ore lands; many of the smaller places throughout the ore-regions started on a "boom;" and, as usual, each claimed to have the best ore-land in the State, and all the facilities for the successful operation of furnaces. Some of the places formed companies and offered large bonuses to any one who would deal with them. Private individuals spent both time and money buying options on land supposed or reported to be ore-bearing. Furnace companies also began operations in several places. Of these, however, only three erected plants. These are the Cherokee Land and Iron Company and the Cherokee Iron Manufacturing Company, located at New Birmingham in Cherokee county, and the Lone Star Iron Company at Jefferson, in Marion county. These have a combined daily capacity of 205 tons, distributed as follows:

Furnaces.	Daily Product.	Ore.
Old Alcalde,	25 tons,	Laminated.
Tassie Belle,	50 "	" out of blast.
Star and Crescent,	50 "	" "
Lone Star,	80 "	Nodular.

Geology of the Ore-Region.

Although iron-ores occur in many portions of east Texas the great ore-belt may be described as a triangular area, bounded on the north by an irregular line drawn from the Sulphur Fork in Cass county, extending west and south through Daingerfield in Morris county to a little south of Quitman in Wood county. The western

limitation of the belt extends irregularly southward through the west side of Smith, east side of Van Zandt and Henderson, and the center of Anderson county, a short distance west of Palestine, and in a southwesterly direction to the Brazos river near Hearne. The southern side is limited to a line crossing the Trinity river near Crockett, the Neches at Augusta and the Sabine river near the northeast corner of Sabine county. The total area is approximately 10,000 square miles of which about 1000 square miles are covered with iron-ore.

While there may be small detached or isolated deposits of iron-ore lying without the limits of the territory embraced within these lines, yet it may be said that this area includes the whole of the workable deposits of ore within this portion of the State. It must not, however, be inferred from this that the whole or even a very large proportion of these deposits are of such extent, quality and quantity, or located in such positions, as to warrant their being worked. Of the 1000 square miles of ore-covered territory probably a little over one-half, or 550 square miles, can be classified as workable deposits.

The topography of the ore-region will be best understood when it is compared to a great plain sloping gently to the southeast, cut into many flat-topped, steep-sided ridges and small table-lands by the numerous water courses which have their sources within the region, or find their way through it. These flat-topped areas are usually denominated mountains, although but few of them reach an altitude of over 700 feet above sea-level, and their existence is due solely to the presence of an iron capping found either lying upon the surface or beneath a thin covering of yellowish-brown sand. This condition obtains mostly through the central and western portions. In the northeastern and along the eastern boundary of the area, the hills forming the overlying beds of the central and western portions have been almost entirely eroded and the country lies at a generally lower level. In this region the hills rarely reach an elevation of 350 feet, and water-courses and larger streams are bordered with wide margins of marshy lands and extensive areas subjected to repeated and long-continued overflow.

The few higher elevations found in this area are either the remnants of the overlying beds which form the highlands of the central area, or are formed of deposits of gray and brownish-yellow sands and brown and yellowish-brown sandstone, and contain deposits or irregular beds of nodular ore. It is to the presence of

these sandstones and ore-deposits that many of the hills owe their existence, and it is more than probable that these hills represent a disintegrated and destroyed continuation of the glauconitic or greensand beds forming the mountains of Cherokee and Rusk counties. No fossils have been found in these deposits, and therefore their geological position cannot be accurately determined. It may be noted, however, that, wherever found, these hills and associated sands overlie the upper stratified division of the lignitic beds and therefore cannot be of lignitic age.

The geology of the ore-region is extremely simple in its greater features. Only two of the great geological divisions are represented —the Quaternary or Pleistocene and the Eocene stage of the Tertiary. The former of these consists of irregularly deposited soft ferruginous sands and ferruginous sandstones of a brown and brownish-yellow color, lying in irregularly lenticular shape and intermixed or interstratified with the sand from which they have been derived or of which these sandstones form the source. It is impossible to determine exactly the relations between the two, they look so much alike. Light gray and yellow sands and brown clays occur in many places; and to this division belongs the great series of what have been denominated conglomerate iron-ores.

The Quarternary deposits occur only as scattered remnants of a great blanket-like sheet, which at one time apparently covered the whole of this area and extended many miles farther to the southward, but which has long since been eroded from all but the higher ridges, and is even now rapidly disappearing.

The Tertiary is represented throughout the ore-regions by two members of the Eocene, as that stage has been divided in Texas: the lignitic, and the greensand or marine fossiliferous beds. The lignitic division consists of an extensive series of variously-colored sands, sandy clays, clays and brown coal and lignite. The sands are colored white, yellow, brown, red, gray, blue and black, the colors often shading into one another and, with the exception of the dark blue and black and occasionally white beds, presenting no uniformity of coloration for any distance. In structure these beds are laminated or thinly stratified, massive or cross-bedded and frequently interlaminated with clay. The clays occur in massive and stratified beds and interlaminated and interstratified with the sands. Many of the beds occur comparatively free from sand; but the greater portion exist as sandy or micaceous clays and are generally dark blue, gray or black. Occasional deposits of red and yellow clays occur, and

frequently beds of white clay are found, many of which are of considerable value as refractory material. The uppermost division of these deposits consists of a series of laminated or thinly stratified white and red sands and sandy clays, frequently merging into one another, and forming a mottled sandy clay or clayey sand. The laminæ generally do not exceed one-fourth to one-half inch in thickness; but the white clay frequently expands to six feet or more. These sands form a base line, beneath which no deposits of ore occur that may be considered to have more than theoretical interest.

The lignitic division is extensively developed along the northern border of the ore-regions and throughout Bowie, Cass, Marion, Panola, Shelby and other counties along the eastern boundary of the State. It has an ascertained thickness in Marion and Harrison counties of over 800 feet, and may probably be correlated with the lignitic of Alabama and Mississippi, with which it appears to be connected through Arkansas and Louisiana.

It is to this division that the great series of brown coals and lignites belong. Small quantities of nodular ore also occur throughout the area occupied by these deposits, but these have more of a theoretical than an actual value.

The marine Tertiary or greensand deposits overlie the lignitic beds, and consist of a series of brown and yellow sands, green sands, altered greensands and clays, with extensive deposits of ferruginous sandstones and laminated iron-ores. The upper division of these beds comprises brown fossiliferous sand, greensand marls, stratified black and gray sandy clays and green fossiliferous clays. While the heavier beds or ore-deposits occur near the top of the series, thin strata or irregular laminæ, ranging from one-fourth inch to two inches of carbonate of iron occur throughout the whole series. This division rests directly upon the lignitic, forms the higher grounds or mountains of the central and western portions of the ore-region, and is extensively developed in Cherokee, Anderson and several other counties. Mount Selman and Gent mountain in Cherokee, Hynson's mountain in Harrison, and Cook's mountain in Houston county, all belong to this series. These beds may be correlated to a lower phase of the Claiborne, and have an aggregate thickness of 650 feet.

Age of the Ore-Deposits.

Considerable confusion appears to have arisen regarding the age of these ore-deposits. In the Tenth Census,* Professor Pumpelly

* *Tenth Census*, vol. xv., plate viii.

shows them as belonging to the Quaternary. In this report the only ore considered was that used at the Kelly furnace in Marion county, and this age was probably assigned to the deposits on account of their similarity in occurrence and structure to ores found in northern Mississippi by Dr. Hilgard,* and by him assigned to the orange sand. Mr. Lawrence C. Johnson† divides the ores of the region under consideration into two divisions—the nodular ore, which he assigns to the lignitic, and the lacustrine ore, which he divides into "buff crumbly" and "laminated" ores, and assigns to the Quaternary, with the reservation that some of it may be Tertiary. Dr. Penrose, in his preliminary report on the geology of the Gulf Tertiary of Texas,§ ascribes the origin of the laminated ores to the destruction of the glauconitic beds and consequently to the same age as the marine Tertiary beds, while the nodular ore he is disposed to associate with the underlying lignitic deposits. This view he emphasizes in his report on the iron-ores of Arkansas, where we find him saying:

"In eastern Texas where the geologic position of the Tertiary iron-ores is more easily defined than in Arkansas, two principal divisions of the Eocene contain noticeable quantities of ore; the lower one is the great series of sands and clays which forms the central part of the Eocene; the upper one is the Claiborne glauconite which overlies these beds."‖

In eastern Texas the lower division of Dr. Penrose's Timber Belt series is that division denominated by Mr. Johnson and by the Geological Survey of the State the "lignitic;" the upper division is the series of beds known by the Survey as the "marine beds" or lower Claiborne. Both are of Tertiary age. It is therefore unnecessary to consider Professor Pumpelly's statement as to the age of those ore-deposits. Mr. Johnson's division of the ages may also be passed for the present. Dr. Penrose's assignment of the different characters of ore to different divisions of the Tertiary deserves more attention.

After an exhaustive examination of the lignitic beds throughout their greatest extension in the regions in which the nodular ores form the prevailing ore-deposits, and also an extended series of ex-

* *Agriculture and Geology of Mississippi*, 1860, p. 10.

† *Report on the Iron Regions of Northern Louisiana and Eastern Texas*, 50th Cong., 1st Session, Ex. Doc. No. 195.

§ *First Ann. Rep. Geol. Surv. of Tex.*, 1889, pp. 65–84.

‖ "Iron-Ores of Arkansas," *Geol. Survey of Ark.*, vol. i., 1892, pp. 105–6.

aminations covering the greater portion of the territory devoted to the laminated ores as well as many hundreds of square miles of lignitic areas, in which no ore of any grade exists, the writer has come to the conclusion that all the ore-deposits, whether nodular or laminated, are of one and the same age, and that the existence of the nodular ore in that form is due altogether to the disintegration and consequent destruction of the greensand deposits.

As has been observed already, the uppermost deposits of the lignitic beds are a series of thinly laminated white and red sands and sandy clays, which lie in direct contact with the lowest members of the greensand deposits of the marine beds. These laminated deposits, wherever seen, have a covering of iron or ferruginous sandstone, varying from half an inch to one inch in thickness, and below which not one of the extensive ore-deposits of any kind is ever found.

Small quantities of nodular ore do occur in some portions of the lignitic beds but these are usually found at considerable depths, and are never extensive. Many of these nodules have the peculiarity, where hollow, of being filled with water; sometimes these nodules contain ocher; and I have yet to see one containing sand. It must also be borne in mind that throughout the very extensive range of country embracing the northern half of Robertson, great portions of Limestone, Henderson, Wood and Smith counties, as well as the southern portion of Harrison and northern half of Panola county, all of which are covered exclusively by lignitic deposits, no bodies of nodular ore exist. Occasional nodules are met with in digging wells; but these are generally small and unimportant.

The assumption that the nodular ores belong to the lignitic, both by Johnson, who first made the statement, and by Penrose, who apparently followed him in this as in several other statements, appears to have arisen from the generally accepted idea that this class of ore is found only in the lignitic areas of the State, and that the region embraced by Cass, Morris, Upshur and Marion counties, in which the nodular ores form the most abundant class of iron, is occupied altogether by deposits of the lignitic series.

While it may be admitted that the greater extent of the territory covered by these counties is occupied by beds belonging to the lower, or lignitic division, this class of deposits does not form the whole of the region. Widely spread fragmentary deposits of greensand marls and glauconitic sandstones occur in many portions of Cass, Morris and Marion counties. These fragmentary deposits often cover

several miles of territory, and their presence is always marked by the occurrence of heavy deposits of nodular ore and a greater or less extent of broken fragments of laminated ore.

In Cass county altered greensand belonging to the marine beds occurs, in association with both nodular and laminated ore, at the Berry Crawford mine, scarcely a mile north of Atlanta Station. Here the nodular overlies the laminated ore and the altered greensand underlying the ore-deposits rests directly upon the uppermost deposits of the lignitic series. The same ridge extends northwesterly and northerly for several miles; and nodular ore is found buried in a brownish gray to yellow sand forming the summit and sides of the ridge. The region around Hughes Springs, in the same county, is covered for several miles with broken deposits of laminated ore, resting upon beds of pyritiferous greensand. These beds extend westward to and beyond Daingerfield, and southward to near Little Cypress Bayou in Morris county. In this area, both laminated and nodular ores occur in close association. These deposits rest upon the same laminated white and red deposits seen in the Berry Crawford mine region. Altered greensand, associated with ore and sandstone of the same age, occurs also near Cusseta Post-Office in the northern portion of Cass county.

Coming southward into Marion county, the laminated buff crumbly ore, associated with ferruginous sandstone and nodular ore occurs on a small hill, about two miles north of Jefferson. Here also these deposits rest upon the laminated red and white sands and clays of the lignitic. Another deposit of greensand occurs in the banks of Cypress bayou, close to the town of Jefferson; and in the northwestern corner of the county there is a continuation of the same beds seen in Morris and western Cass county.

An extensive area overlain by these glauconitic marine deposits forms a high ridge through the center of Harrison county. The eastern or northeastern end of this ridge lies about six miles north of the town of Marshall, and the western end extends a short distance west of the western line of the same county. This area is fossiliferous, to some extent, throughout. The fossils, however, usually occur only as casts, and belong to the fauna common to the lower Claiborne greensands. Both nodular and laminated ores occur in close association on the top of some portions of the ridge, and many nodules of the same grade lie amongst the broken laminated ore along the sides and in the stream-channels flowing through or from the ridge. This ridge is cut off from the northern deposits

by Little Cypress bayou and the bottom-lands belonging to that stream, and its western extension is cut short by the Sabine river and its associated flat-lands. The glauconitic deposits of the ridge itself rest directly upon the uppermost stratified deposits of the lignitic, whose white and red ribbon-like sands and clays crop out everywhere along the base of the ridge, and in some of the deep cuttings extending through the area. A small outcrop of altered greensands, with thin laminae or strata of laminated ore, occurs on the north side of Little Cypress, at Allen's Bridge. The intimate association of these altered sands with the ore can readily be understood when it is stated that the latter lies interstratified with and filling numerous joints in the sands.

In Smith county, also, we find isolated hills of sands and laminated ores belonging to the same age. A hill half a mile west of Swann Switch shows this structure, and about nine miles southeast of Tyler a ridge, showing laminated ore, rests directly upon an altered greensand, containing casts of lower Claiborne fossils.

It must be remembered that these are all detached areas, separated by wide intervals from the main body of the greensand deposits, and these denuded areas are occupied by streams, some of which are of considerable size, and to the action of which a great part of the denudation may possibly be due.

The position of the nodular ores in relation to the laminated variety is somewhat difficult to determine exactly. In many places we find them overlying the laminated deposits; but, at the same time, it must be admitted that the greater portion of these ores lie at considerably lower levels. The lower-level ores, however, all occur in positions unconformable with and overlying the uppermost deposits of the lignitic. With but very few exceptions the nodules or geodes of ore, irrespective of their positions, are intermixed and interbedded with a brownish-gray or yellow sand, of the same texture and appearance as that usually found overlying the laminated ore-beds of the regions exclusively occupied by that class of ore. These nodules are not infrequently mixed with broken fragments of the laminated variety.

From these facts it would appear that the whole area was at one time completely covered by the marine beds, with their associated deposits of ore, and that through a prolonged period of erosion these deposits have been gradually broken down and destroyed. The iron, by a process of leaching and segregation, has acquired the nodular form in which we now find it. The theory advanced by

Dr. Hilgard* for the formation of these ores, as applied to their orange-sand origin, viz., that these nodules have been formed along the line of contact between the orange sand and the underlying lignitic clays by the oxidation and lixiviation of the iron from the sands, may as well be applied to their formation from the destruction and consequent ferrugination of the glauconitic material contained in the marine Tertiary beds. This erosion is going on rapidly at the present time.

Returning to Mr. Johnson's idea that the laminated ores are lacustrine and were produced at various stages of the Quaternary history of the region under consideration, and some of them possibly during the Tertiary,† the examinations made throughout the region show very conclusively that none of these ores belong to any portion of the Quaternary, but the whole of the laminated ores are connected with, and form an integral part of, the marine beds. Many of the sections examined in widely separated portions of the country show the same character of ore to occur at more than one horizon and to lie interstratified with the glauconitic beds. Borings in many places have demonstrated the existence of these ore-deposits within the interior of the beds, so that they are not solely due to atmospheric action. These lower deposits are usually much thinner than the upper, or what may be denominated the surface or main body of ore. Besides being thus connected stratigraphically with the greensand beds, the ores themselves are fossiliferous and carry a fauna similar to that of the greensand beds, and therefore belong to the marine or lower Claiborne stage of the Tertiary.

Classification of the Ores.

The iron-ores of east Texas are all hydrated, and have been classed for convenience as limonites or brown ores, although, strictly speaking, some portions of them may not belong to that species. From their physical structure, rather than their chemical constitution or metallic ratios, these ores have been divided into three classes, viz., nodular, laminated and conglomerate. Of the last class no account has been taken in this paper, since aside from the peculiarity of its structure, position and quantity it is of no great interest, and of no economic use whatever as a metal-producer.

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* *Agr. and Geol. of Mississippi*, 1860, pp 23–24.

† *Report on the Iron Regions of Northern Louisiana and Eastern Texas*, 1st Session, 50th Congress, Ex. Doc., No. 195.

The conglomerate ores are usually a mixture of ferruginous pebbles, sand and gravel, and in many places siliceous pebbles cemented together in an iron matrix. They are generally associated with streams, either (as in the case of the older deposits) lying high up in the bank, marking a former level of the stream, or near its source, or (as in the case of the newer deposits) lying close to the present water-level. Occasionally deposits of siliceous conglomerates are found capping the hills and ridges.

The nodular ore is usually found in the form of irregularly-rounded, oval and flattened or ellipsoidal nodules or boulders from a few inches to one or two feet in length. Outside, these present a smooth appearance and dull or earthy brown color. When broken, the shell presents a striated appearance of yellow and brown colors, formed by the alternate concentric rings of iron-ore and ocher. These striations usually do not exceed one-fourth to one-half inch in thickness, but in some of the larger nodules the iron has a thickness of over an inch, and in many the yellow ochreous concentric rings are absent, in which case the whole shell, with the exception of the brown outer covering, is dark blue. The interior coating of the shell is often a glossy black. Many of these concretions are hollow; a great number, however, have the interior filled with a core of brown or yellow ocher, similar to that forming the yellow rings; others have dendritic formations of ore spreading through the center and having the ends fastened to the inner side of the shell. Some few, particularly of the flattened oval form, have the entire center filled with convolutions of the inner ring. Most of the rounded forms are either empty or filled with the same character of yellow sand amongst which they lie. In no case has a single specimen been found containing water, as in the sporadic nodules found in the lignitic beds. Nodules containing siliceous pebbles and conglomerate are occasionally met with in Harrison county.

The laminated ores vary in appearance as well as texture and thickness. In places, these ores occur in thin laminæ of dark brown or chestnut color, interstratified with similar laminæ of bright orange or yellow. These laminæ rarely exceed a quarter of an inch in thickness. At other places, the ores become more massive, occur in beds from two inches to as many feet thick, and vary in color from a dark chestnut-brown to a lighter shade of the same color, with small irregularly disseminated patches of yellow showing throughout the mass. This ore also occurs in thin wavy laminæ of from chestnut-brown to black color, usually having the

spaces between the laminæ filled with fine clayey material. This grade is usually of a very crumbly nature, hence the name given to it of "buff crumbly." The laminated ores have also been made to include the botryoidal and mammillated forms frequently found intermixed with the other ores.

While the ores of both classes are found more or less in conjunction with each other, the whole ore region admits of a division into two districts in which, respectively, the different classes of ore predominate. In the northeastern portion of the area under consideration, the nodular-ore is the more prevalent, hence this may be distinguished as the nodular-ore region and made to embrace the whole of Cass and Marion counties, a small portion of Morris, the most of Upshur, the eastern side of Harrison and small portions of Panola and Shelby counties.

What may be designated as the laminated ore district comprises the greater portion of the entire ore-field, being the greater portion of Morris, Harrison, Rusk, Nacogdoches, portions of Smith, Henderson, Anderson and nearly the whole of Cherokee county. This class of ore reaches its greatest areal development in the last-named county. Small quantities of laminated ore occur also but no ore-bodies of any economic importance are known in Houston, Leon and Robertson counties. As we approach the Brazos river, the western end of the region parts with its ore and is to a great extent covered with a ferruginous sandstone.

Unlike the nodular ore (which is found as a rule in separate deposits with considerable areas of barren country between them and usually imbedded in sand), the laminated ore forms a continuous covering over large areas. Its continuity may be traced from hill to hill, as in Cherokee, over nearly two-thirds of the county, with only the stream-channels intervening. In places it is covered with deposits of a grayish or brownish-yellow sand; but it frequently forms the surface in spots covering several acres. Small secondary deposits lie at various depths in the glauconitic sands beneath the main bodies, and borings have been made throughout several portions of the field in the expectation of finding a heavy deposit, but without success. The secondary deposits are all without exception too thin and of too poor a grade to possess practical value.

Thus we have for the laminated ore a specific horizon, below which it appears to be useless to look for any economic ore. It is different with the nodular class. Ores belonging to this grade have no real horizon and it is difficult to say where they really do lie.

The only guide in that area is the laminated sands of the lignitic, below which it is useless to look for ore.

The ore-deposits are very variable in thickness. To the nodular ores no thickness can be given. They usually occur in the form of irregular deposits covering areas extending from ten to several hundred acres, and having a thickness of from one to five, and occasionally more feet. The laminated ores are more uniformly distributed over much wider areas, and have a thickness ranging from two to seven and sometimes ten feet, although throughout their greatest extent they are rarely more than two and a half feet thick.

Chemical Analyses of the Ores.

The typical brown limonites or bog-ores, the class to which our east Texas ores belong, have theoretically the composition represented by $2Fe_2O_3,3H_2O$, corresponding to ferric oxide 85.6 (iron 60) and water 14.4 per cent. Generally, however, these ores contain other bases, and sometimes silica, manganese, phosphoric, sulphuric, and arsenic acids and organic matter.*

Many of the ores of the region closely approach the theoretical composition, especially those occurring in the northeastern portion. Specimens have been obtained from Cass county, ranging in metallic iron from 55.38 to 59.99 per cent., and from Marion containing from 58.38 to 59.12 per cent., and one gave Dr. Everhart 61.38 per cent., while in Morris and Upshur the ores carry metallic iron as high as 56.14 to 59.48 per cent.

In any examinations upon which estimates of the value of an ore-field depend, the greatest care is necessary in selecting the samples for analyses. Any neglect in this operation may be the primary cause of very seriously misleading results. In the examinations made by myself and associates throughout the east Texas ore regions, having no interests to serve, we took the greatest care to obtain specimens typical of the average ore of each deposit. Many specimens were obtained from each field; these were again averaged in the laboratory; and the specimen selected for analysis was considered as fairly representative of the whole deposit from which it was taken. The results of these analyses are shown in Tables I. and II. Table III. shows the results of analyses of ores collected by other parties.

* Bauerman, *Metallurgy*, pp. 146, 147.

TABLE I.—*Analyses of Nodular Iron-Ores.*

	Ferric Oxide.	Phosphorus.	Sulphur.	Silica.	Alumina.	Lime.	Magnesia.	Water & Loss.	Metallic iron.	100 Iron gives Phosphorus—
Average of 21 sp. Cass.....	71.72	0.117	0.065	10.67	7.71	0.237	0.005	9.51	50.20	0.233
" 10 sp. Marion.	75.52	.....	.....	.....	.....	.....	.....	.....	.....	.....
One of these contains... { Ferr's oxide 2.61 }		0.081	0.084	9.05	3.71	0.39	0.10	10.37	54.89	0.147
Average of 4 sp. Upshur...	66.82	0.072	0.093	14.08	6.15	1.25	Trace	11.07	46.76	0.153
" 5 sp. Harrison	68.30	0.161	0.056	11.67	7.41	0.15	Trace	6.62	47.81	0.336
" 4 sp. Panola....	71.33	0.100	0.366	11.89	5.44	Trace	.....	6.87	49.93	0.200
" 5 sp. Smith.....	59.76	0.305	0.260	18.59	10.83	0.36	.....	9.80	41.83	0.729
Average of 49 sp.....	68.91	0.139	0.154	12.66	6.91	0.398	0.017	9.04	48.65	0.285

TABLE II.—*Analyses of Laminated Ores.*

	Ferric Oxide.	Phosphorus.	Sulphur.	Silica.	Alumina.	Lime.	Magnesia.	Water & Loss.	Metallic iron.	100 Iron gives Phosphorus—
Ave. of 4 sp. Cass county..	55.78	0.183	0.115	16.29	13.63	0.42	.....	13.46	39.18	0.464
" 7 sp. Harrison co..	65.63	0.043	0.040	16.22	6.36	Trace	Trace	11.46	45.94	0.093*
" 10 sp. Morris co.....	78.43	0.124	0.008	6.02	4.37	.....	.....	10.68	54.90	0.220
" 2 sp. Upshur co...	63.68	0.034	0.092	20.54	7.17	1.15	.....	7.52	44.57	0.076*
" 9 sp. Henderson co	65.17	0.226	Trace	15.27	9.53	0.15	Trace	9.28	45.63	0.495
" 22 sp. Smith co.....	60.49	0.310	0.032	18.46	9.30	0.24	Trace	10.48	42.34	0.732
" 2 sp. Panola co.....	57.42	0.227	.....	30.82	9.72	.....	.....	1.30	40.23	0.564
" 8 sp. Rusk co.....	70.29	0.364	0.060	10.25	9.87	0.02	Trace	8.26	49.19	0.739
" 12 sp. Cherokee co.	65.53	0.189	0.063	10.59	9.64	0.13	0.108	11.87	45.87	0.412
" 6 sp. Anderson co.	66.31	0.222	Trace	11.13	9.60	Trace	Trace	12.17	46.42	0.478
Average of 82 specimens..	64.87	0.192	0.041	15.56	8.92	0.21	0.010	9.65	45.43	0.422

* Analyses marked thus appear to be within the Bessemer limit of phosphorus.

These tables show the general character of the ores from the whole region, and probably may be regarded as setting limits above which the ratios of phosphorus, sulphur, and silica may not be legitimately expected to go, and below which we may reasonably infer the percentage of metallic iron will not fall; yet they contain the analyses of many specimens, some of them above the average in impurities, obtained from deposits which, from the smallness of their areas, as

TABLE III.—*Analyses of Iron-Ores Collected from East Texas, not Included in Tables I. and II.*

Number.	Locality.	Ferric Oxide.	Phosphorus.	Sulphur.	Silica.	Alumina.	Lime.	Magnesia.	Water & Loss.	Metallic Iron.	ANALYSTS.
1	Nodular ore. Nash Fur'ce Marion co.	79.60	.....	.....	8.12	.....	.....	.....	12.27	55.72	Dr. Riddell.
2	Nodular ore. Marion co.	87.69	0.04	0.02	6.83	.....	.....	.....	.....	61.38	Dr. Everhart.
3	Nodular ore. "	82.06	0.19	0.23	6.83	.....	.....	.....	.....	57.44	"
4	Nodular ore. "	75.77	tr'ce	0.53	5.45	5.40	0.65	0.43	12.04	53.04	Chauvenet & Blair.
5	Rusk co.	71.78	.....	.....	8.79	1.04	.....,	.....	13.39	50.25	Dr. Riddell.
6	Cherokee co.	66.50	0.153	.....	17.53	.....	.....	.....,	.....	46.55	J. Birkinbine—1885.
7	"	65.21	0.069	.....	17.67	.....	.....	.....	13.09	45.65	"
8	"	64.52	0.062	0.038	20.36	.....	.....	.....	15.25	45.17	"
9	"	59.04	0.315	0.01	23.84	.....	.....	.....	15.76	40.63	"
10	"	69.00	0.284	0.027	16.62	.....	.....	.....	13.71	48.31	"
11	"	64.52	0.062	0.038	20.36	.....	.....	.....	14.25	45.17	Regis Chauvenet & Co.
12	"	69.00	0.284	0.027	16.62	.....	.....	.....	13.71	48.31	"
	Average.....	71.22	0.135	0.11	14.08	.....	.....	.....	13.76	49.85	100 iron gives 0.271 of phosphorus.
	Average of 28 samples collected by Dr. Penrose over the whole area,	65.50	0.025	0.17	11.45	9.44	0.70	4.98	9.37	45.85	100 iron gives 0.55 of phosphorus.

well as the siliceous nature of the contained ores, can scarcely be considered as available material for blast-furnace purposes.

An analysis of these tables shows the ores to have the following composition, which, it may be remarked, compares favorably with that of the ores from the other Southern States:

	1. Nodular Ores.	2. Laminated Ores.
Metallic iron.....	52.77	45.84
Phosphorus.....	0.097	0.114
Sulphur.....	0.148	0.076
Silica.....	9.73	14 91
Phosphorus ratio...	0.183	0.248

In Tables IV. and V. the analyses of what may be regarded as typical specimens of the available ore-deposits of the various counties mostly embraced in this paper are shown:

TABLE IV.—*Analyses of Nodular Ores Found in Cass, Marion, Upshur and Harrison Counties.*

No.	Locality.	Ferric Oxide. Fe_2O_3.	Phosphorus. P.	Sulphur. S.	Silica. SiO_2.	Alumina. Al_2O_3.	Lime. CaO.	Magnesia. MgO.	Water and Loss.	Metallic Iron. Fe.	100 Iron gives Phosphorus—
	CASS COUNTY.										
1	L. Mornen, headright.	80.85	Trace	Trace	5.85	2.15	.....	.....	11.60	56.59	Trace
2	P. M. Keeton "	74.60	0.139	0.040	9.00	5.00	Trace	Trace	10.30	52.22	0.266
3	Hudson "	77.61	0.139	Trace	7.60	7.39	0.21	0.00	6.85	54.32	0.257
4	Berry Crawford mine..	73.97	Trace	0.044	14.60	4.63	Trace	.....	6.61	51.78	Trce*
5	A. Duncan, headright.	85.70	0.109	0.200	0.40	7.50	.....	.....	6.11	59.99	0.182†
6	J. Horton, "	77.56	0.139	0.068	9.00	4.04	0.51	.....	8.15	54.29	0.257
7	8 miles N.E. of Linden	67.41	0.056	0.100	7.20	14.76	Trace	0.032	10.41	47.12	0.118
8	H. Cunningham.....	70.83	Trace	0.096	17.20	3.17	Trace	Trace	8.22	49.58	Trace
9	R. M. Hadfield.....	79.32	Trace	0.510	6.20	4.68	Trace	Trace	9.17	55.52	Trace
10	W. R. Gilley.....	70.83	0.095	Trace	13.80	3.57	0.20	Trace	12.01	49.58	0.191
11	S. Burnham.....	81.36	0.139	0.102	3.00	4.24	0.82	.....	9.60	56.92	0.244

* Supplies Lone-Star Furnace. † Supplied Sulphur-Fork Furnace.

	MARION COUNTY.										
12	D. Richardson, hdt.....	79.06	Trace	Trace	6.04	0.69	1.50	.....	12.68	55.34	Trace
13	Lilly, headright.....	76.42	0.095	0.244	3.10	6.20	1.13	.....	9.90	55.52	0.171‡
14	Nash Furnace	76.34	0.095	0.040	4.24	6.86	.....	.....	12.60	53.43	0.177
15	J. Johnson, hdt.....	83.40	0.091	Trace	1.30	2.41	.....	.....	12.80	58.38	0.155
16	J. W. Duncan, 9 miles north of Jefferson.	65.79	Trace	Trace	0.93	19.96	Trace	.....	13.90	46.05	Trace
17	J. A. McKinney, 8 m. north of Jefferson.	60.44	Trace	Trace	6.77	19.56	Trace	.....	12.95	42.30	Trace
18	Barnes, 4 m. N.W. of Lassater.	84.46	0.083	0.068	2.90	Trace	.....	.....	12.20	59.12	0.140
19	2 miles S. W. of do.....	84.15	0.083	0.044	1.22	2.05	0.94	.....	12.20	58.90	0.140
20	Lassater Hill	69.75	.....	0.398	6.97	8.70	.....	0.235	12.15	48.83	Trace

‡ 2.61 FeO.

	UPSHUR COUNTY.										
21	3 m. S.E. of Coffeeville..	80.78	0.166	0.008	4.30	2.22	0.88	.....	10.47	56.54	0.293
22	Butler Survey, N.E. of Upshur.	65.79	Trace	0.144	18.77	8.31	2.30	.....	5.05	46.05	Trace
23	2 m. S.W. of Coffeeville..	71.78	Trace	0.122	14.10	2.82	.....	.....	11.10	50.24	Trace

	HARRISON COUNTY.										
24	J. Johnson.....	72.40	0.083	0.060	14.50	5.80	0.60	.....	6.30	50.68	0.163
25	Dan. Davis.....	72.40	0.418	0.030	11.30	6.80	.....	.....	8.21	50.68	0.836
26	J. Johnson	73.03	0.139	0.053	9.80	8.17	.....	Trace	8.20	51.12	0.271

TABLE V.—*Analyses of Laminated Ores from Morris, Harrison, Henderson, Anderson, Cherokee, and Rusk Counties.*

No.	Locality.	Ferric Oxide. Fe_2O_3.	Phosphorus. P.	Sulphur. S.	Silica. SiO_2.	Alumina. Al_2O_3.	Lime. CaO.	Magnesia. MgO.	Water & Loss.	Metallic iron. Fe.	100 Iron gives Phosphorus—
	MORRIS COUNTY.										
1	H. Proctor, headright..	80.20	Trace	Trace	4.20	3.10	Trace	.....	12.50	56.14	Trace
2	" "	76.26	"	"	5.20	6.84	"	.....	11.60	53.38	"
3	J. W. Duncan "	84.98	"	0.034	7.40	1.42	"	.....	6.00	59.48	"
4	" "	81.60	0.139	Trace	6.10	1.20	.....	.....	10.80	57.12	0.225
5	J. N. Gray "	76.54	0.209	Trace	6.90	3.06	.....	.....	13.00	53.58	0.390
6	Leander Kidd "	74.30	0.139	"	13.10	5.10	.....	.....	7.08	52.01	0.267
	HARRISON COUNTY.										
7	J. Decker, headright...	63.95	0.052	0.044	13.40	8.30	Trace	.....	14.20	44.77	0.116
8	Marshall Water-works	64.90	0.030	Trace	26.70	0.70	"	.....	7.81	45.43	0.066
9	E. Tally, headright.....	61.22	Trace	Trace	15.15	10.18	.....	.....	13.50	42.85	Trace
10	Hynson Springs.....	61.34	"	0.100	11.85	16.26	Trace	.....	10.40	42.93	"
11	6 m. N.W. of Marshall	64.03	0.152	0.128	16.70	10.77	.....	.....	7.90	44.82	0.339
	HENDERSON COUNTY.										
12	2 miles W. of Fincastle	80.45	Trace	Trace	4.30	3.15	Trace	.....	8.15	48.93	Trace
13	Brownsboro.....	73.60	"	"	10.06	9.89	.....	.....	6.75	51.52	"
14	Pilot Hill.....	69.90	0.152	.....	10.40	10.90	.....	.....	8.15	48.93	0.310
15	A. H. Caldwell	63.02	0.222	.....	14.30	9.58	.....	.....	12.50	44.11	0.503
	ANDERSON COUNTY.										
16	Fosterville.....	68.80	0.052	.....	13.36	3.40	Trace	Trace	13.70	48.16	0.107
17	N. of Nechesville.....	67.84	Trace	.....	9.64	8.16	.....	.....	14.69	46.76	Trace
18	8 m. S.E. of Palestine..	64.32	"	Trace	8.70	13.18	Trace	Trace	14.10	45.02	"
19	South of Palestine.....	60.17	"	.....	15.95	17.03	0.45	0.29	6.60	42.11	"
20	North of Palestine.....	69.50	0.340	.....	11.35	8.00	.....	Trace	10.50	48.65	0.698
	CHEROKEE COUNTY.										
21	1 m. N.W. State Furnace.	63.84	0.039	0.040	18.90	5.76	0.18	.....	11.03	44.68	0.087
22	5 m. W. of Jacksonv'le	65.32	0.265	Trace	9.90	10.08	0.50	Trace	13.70	45.72	0.579
23	2 m. N. of Rusk.....	70.79	0.152	0.092	9.50	6.01	Trace	.....	13.20	49.55	0.306
24	" "	75.94	0.030	0.040	2.20	10.46	"	.....	11.30	53.15	0.056
25	Mount Selman.....	73.04	0.554	.....	8.25	3.66	.....	.....	13.80	51.12	1.083

TABLE V.—(continued).

RUSK COUNTY.

No.	Locality.	Ferric Oxide. Fe_2O_3.	Phosphorus. P.	Sulphur. S.	Silica. SiO_2.	Alumina. Al_2O_3.	Lime. CaO.	Magnesia. MgO.	Water & Loss.	Metallic iron.	100 Iron gives Phosphorus—
26	2 m. E. of Henderson..	69.53	Trace	0.036	15.10	6.67	Trace	.....	3.20	48.67	Trce*
27	Iron Mt., Gould.....	71.79	0.106	0.422	7.45	5.01	"	.....	13.50	50.28	0.210
28	" Glenfawn.....	78.26	0.209	0.024	6.00	6.74	"	.....	6.20	54.78	0.381

* 5.60 MnO.

These analyses have all been made from the raw ores as brought from the field, and an examination of the tables shows the ores to carry a low percentage of silica. With the exception of three counties (Upshur, nodular ore, 12.39; Harrison, nodular ore, 11.86; laminated ore, 16.76; and Anderson, laminated ore, 11.80), the averages all range under 10 per cent. in silica.

In sulphur and phosphorus these ores also show low percentages, the highest of sulphur being 0.182 in Upshur and 0.161 in Rusk county ores. Regarding the phosphorus, it will be noticed that these ores average very low, and many of the individual specimens range well within the Bessemer limit. It also appears that the average of the total analyses of the Marion county ores is within that limit. In confirmation of the correctness of these analyses, Messrs. John A. Kruse & Co., the proprietors of the Lone-Star Iron Company, write under date of February 24, 1893: "Almost all of our iron is within the Bessemer limit. We have a great number of analyses made from the ores on different tracts, in which all numbers were away within the Bessemer limit for phosphorus. The iron is neither cold- nor hot-short, but is almost a strictly neutral iron." This refers to the ores of Cass and Marion counties, the region in which the Lone-Star furnace is situated. With reference to the laminated ores, Mr. Barrett, late manager of State furnace, and now manager of the Cherokee Iron Manufacturing Company, at Rusk, writes: "All our solid brown ore is practically free from phosphorus."

It will also be observed that there is a general absence of titanium from these ores.

The average percentage of metallic iron in these ores ranges from 44.20, laminated ore, in Harrison to 55.28 in Morris county, while of the ten groups represented one exceeds 55; two, 53; one, 51; two, 50; and the other four range from 44 to 48.84.

The best tests of any ores, however, are the products. The character of the metal furnished by the east Texas ores is shown in Table VI:

TABLE VI.—*Analyses of Pig-iron made from East Texas Iron ores.*

Average of 8 analyses of iron made from nodular ores. (Supplied by the Lone-Star Iron Company, Jefferson, Texas.)

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	
Graphitic carbon	3.86	3.64	3.49	3.04	2.594	NOTE.–The phosphorus shown in No. 4 is the highest shown by any analysis.
Combined carbon	0.76	0.44	0.76	1.09	1.421	
Silicon	3.197	2.148	1.881	1.091	0.602	
Sulphur	0.008	0.002	0.001	0.024	0.049	
Phosphorus	0.058	0.108	0.117	0.161	0.151	
Manganese	0.208	0.345	0.215	0.342	0.117	

Two analyses made by the analyst of the National Tube Works Company, from a car-load lot sent to that company by the Lone-Star Iron Company, show:

	No. 1.	No. 2.
Graphitic carbon	3.50	3.40
Combined carbon	0.45	0.50
Silicon	3.15	2.25
Sulphur	0.021	0.025
Phosphorus	0.019	0.017
Manganese	0.44	0.45

The analyses of the pig-iron made from the laminated ores, as shown by the work at the State furnace at Rusk, are:

	No. 1.	No. 2.	No. 3.	
Graphitic carbon	Not det.	1.59	1.68	NOTE.—No. 2 is of No. 1 grade of pig-iron. No. 3 is of No. 2 grade of pig-iron.
Combined carbon	"	0.51	0.12	
Silicon	2.250	1.90	1.75	
Sulphur	0.005	0.014	0.007	
Phosphorus	0.477	0.590	0.512	
Manganese	Not det.	.....	.....	

Tests of the tensile and transverse strengths of the cast-iron made at the Lone-Star furnace have been made by Messrs. Robert W. Hunt & Co., of Chicago, and show the following results:

1. *Tensile Strength.*

Samples marked.	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Ultimate strength in pounds per square inch, . .	19,529	22,441	19,143	22,038	26,931
Remarks—Broke in wedges.					

2. *Transverse Strength.*

Samples marked.	No. 1.	No. 2.	No. 3.	No. 5.
Ultimate strength in pounds,	21,350	25,400	25,430	27,950
Remarks—Transverse test made on pieces 1¾ inches square. Supports, 18 inches apart.				

No tests of the State furnace products have been made.

Comparative analyses of the Texas iron-ores with those of the other Southern States, Alabama, Tennessee, Kentucky, and Georgia, give us results by no means unfavorable to the Texas article. Arkansas and Louisiana are not included in this comparison chiefly because the Arkansas ores do not prove commercially useful, and nothing practical is known regarding those of Louisiana. In Table VII. I have shown the complete analyses of typical specimens of the limonites found in the several states, and have for further comparison added a typical Swedish limonite and a Canadian bog-ore. These ores are all of other ages than the Texas ores, and, with the exception of the Swedish and Canadian, of no later date than the Subcarboniferous.

In Table VIII., compiled from the Tenth Census for all except the Texas ores (the authority for which is the analyses made by the chemists of the geological survey of the State), the comparative values of metallic iron contained, phosphorus, and phosphorus ratios of the ores of the States represented are shown, while Table IX. shows the composition of the resultant pig-iron. To this table has also been added for comparison the analysis of standard No. 1 pig-iron of the Thomas Iron Company, and three analyses of iron classed as comparatively free from phosphorus. Table X. shows the average yield of iron from the furnace-returns of the Southern States and the general character of the ores used (from the Eleventh Census, vol. i.).

TABLE VII.—*Analyses of Limonites from Alabama, Tennessee, Kentucky, Georgia, Sweden, Canada and Texas.*

Constituent.	Alabama. J. B. Britton.	Tennessee. 10th Census.	Kentucky.* 10th Census.*	Georgia. Spencer.	Flatur, Sweden. Svansberg.	Lac-a-la Tortue, Canada. Griffin.*	Texas.	
							Nodular.*	Laminated.*
Ferric oxide	79.93	72.28	53.93	81.26	67.59	70.04	73.97	75.94
Ferrous oxide	.....	0.36	4.93	.....	.....	.....	.....	.....
Manganese oxide	0.92	2.83	1.26	0.43	1.45	1.78	.....	.....
Silica	6.04	12.76	14.37	5.79	7.81	7.84	14.60	2.20
Alumina	1.43	0.31	9.36	1.12	4.18	2.20	4.63	10.46
Lime	0.07	0.42	1.03	0.12	0.47	0.32	Trace	Trace
Magnesia	Trace	0.17	0.52	.....	0.23	0.27	.....	.....
Sulphuric acid	.....	0.04	0.02	S. 0.01	.....	0.23	S. 0.044	S. 0.040
Phosphoric acid	P. 0.45	0.543	0.290	P. 0.05	0.18	0.76	Trace	P. 0.030
Titanic acid	.....	.....	.....	.....	.....	.....	.....	.....
Water and loss	10.49	10.406	14.402	11.45	17.81	16.84	6.61	11.30
	99.33	100.119	100.112	100.23	99.72	100,28	99.854	99.97
		S. 0.069	S. 0.227			S. 0.092		
Metallic iron	56.10	50.91	40.61	56.88	47.32	49.03	51.78	53.15

* See Analyses of Pig-iron made from these ores, in Table IX.

TABLE VIII.—*Comparative Partial Analyses, Showing Metallic Iron, Phosphorus, and Phosphorus Ratio of Iron-ores from Southern States.*

	State	No. of Specim'n	Metallic Iron.	Phosphorus.	Phos. Ratio.	Authority.
1	Alabama	77	48.88	0.329	0.693	10th Census, vol. XV.
2	Tennessee	148	51.25	0.441	0.860	" "
3	Georgia	47	52.00	0.546	1.050	" "
4	Kentucky	34	39.79	0.439	1.103	" "
5	Texas nodular	53	52.77	0.093	0.183	Geol. Survey of State.
6	Texas laminated	82	45.84	0.114	0.248	" "

From these tables it will be seen that in point of quality the Georgia ores show the highest proportion of metallic iron, while the Texas ores stand between those of Alabama and Tennessee in point of richness, containing from one to three per cent. more metal than the Tennessee ores, and from two to thirteen per cent. more than Kentucky, Sweden and Canada, where special average analyses

TABLE IX.—*Comparative Analyses of Pig-iron.*

		Graphitic carbon.	Combined carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese.	Iron.	Furnace.
1	Texas nodular ore.	3.40	0.50	2.25	0.025	0.017	0.45	.....	Lone-Star Furnace.
2	" "	3.86	0.76	3.197	0.008	0.058	0.208	.....	" "
3	Texas lamin'd ore.	.....	.....	2.250	0.005	0.477	.....	.....	State Furnace.
4	" "	1.59	0.51	1.90	0.014	0.590	.....	.....	" No. 1 grade.
		1.68	0.12	1.75	0.007	0.512	.....	.....	" No. 2 grade.
5	Alabama	3.579	0.78	3.295	0.005	0.109	.....	.....	Eureka Furnace. E. J. Schmitz.
6	Georgia.	.....	.....	0.337	.....	0.393	0.124	.....	Etna ores. Spencer's Geol. Survey of Georgia.
7	Kentucky	2.790	Trace	4.106	0.150	0.695	0.084	.....	Hunnewell Furnace. Peters' Geol. Survey.
8	Kentucky	3.310	0.46	1.955	0.029	0.488	0.213	93.54	Grand River; hot-blast. No.1 recommended for car-wheels.
9	Canada.....	3.256	0.701	1.269	0.0406	0.6332	0.597	93.52	Radnor iron, No. 1½. Griffin, A. I. M. E., *Trans.*, xxi., 990.
10	Standard No. 1.....	3.52	0.13	2.44	0.02	1.25	0.28	92.37	Thomas Iron Company.
11	Workington No.1 Bessemer.	3.85	0.24	2.20	0.05	0.10	1.15	.....	Phillips's Metallurgy, 3d edition, 1891, p. 274.
12	Cleator gray Fnd'y	4.18		1.92	0.05	0.08	0.02	93.94	Classed as comparatively free from sulphur and phosphorus. Art. "Iron," Encycl. Brit., IX. ed., vol. xiii., p. 506.
13	Lake Supr'r charc'l	3.39	0.38	2.28	0.03	0.10	0.17	93.34	
14	Nova Scotia No. 1..	3.50		0.84	0.02	0.19	0.44	94.85	

are taken. When a comparison is made of the whole of the ore regions of the five southern states, including Texas, the Texas nodular ores show higher metallic contents than even Georgia, while her laminated ores stand between Kentucky (39.79) and Alabama (48.88), and show a percentage of 45.84.

With regard to the purity of these ores, the Texas nodular ores show a greater percentage of silica than any of the other States although but slightly higher than those from Kentucky, while in the laminated ores this impurity is less than half the percentage (5.79) shown in the Georgia ores, the next lowest in the group,

TABLE X.—*Furnace Returns, Showing Average Yield of Iron in Different Southern States.*

(From Eleventh Census.)

	State.	Average yield o Iron.	Character of Ores used.
1	Alabama	44.4	70 per cent. red hematite.
2	Georgia and North Carolina.	40.7	0.5 red hematite; 0.3 brown hematite, and 0.2 magnetite and mill-cinder.
3	Kentucky	46.2	Roasted carbonate and brown hematite.
4	Maryland	41.0	Local and Virginia ores.
5	"	47.7	Cuba and Mediterranean ores.
6	Missouri	56.4	Principally red hematite, with small admixture of brown hematite.
7	Tennessee	41.6	Local ores, with additions from Alabama and Ga.
8	"	38.8	Where brown hematites only are used.
9	Texas*	45.8	Brown hematites (bog-ores) when roasted.
10	Virginia	43.4	90 per cent. brown hematite; balance, local red hematites, magnetites from N. Carolina, and a small amount of mill-cinder.
11	"	48.6	Brown hematites only.
12	"	43.3	
13	"	41.4	

* In a letter from Mr. D. T. Jones, Superintendent of the State Furnace at Rusk, received a few days ago, he says the average iron obtained from the furnace is from 48 to 53 per cent. (Dec. 22, 1893.)

Table VII. Of sulphur, both of the Texas classes show very small quantities, being bettered only in this respect by the Swedish, Alabama and Georgia ores, as shown in Table VII. The phosphorus, as shown in the same table, places both of the Texas specimens in the first place as regards freedom from this impurity, a condition which holds good when the phosphorus contents and phosphorus ratios of the whole regions are considered as shown in Table VIII. Indeed, as a matter of fact, Alabama, with the next ores in purity in this respect, has a phosphorus ratio almost three times as great as the highest ratio shown by the Texas ores.

From Tables IX. and X., it will be seen that the average yield of pig-iron from the Texas ores ranks fifth in the list of States shown in Table X., giving seven per cent. more than the Tennessee brown hematites; and while these ores fall below the yield of the Virginia ores in one instance, in three other localities the Texas ores exceed the Virginia yield.

In purity the pig-iron produced from the Texas ores also ranks high, and in freedom from sulphur and phosphorus takes a position

even higher than the typical irons classed as comparatively free from these impurities, shown in Table IX.

Altogether the Texas ores may be classed as having fair average metallic contents, a medium admixture of silica and low percentages of both sulphur and phosphorus, as yielding rather above the average, for their grade, in the furnace, and as producing a very high grade of perfectly neutral iron, much of which is well within the limits of the requirements of Bessemer iron.

With these qualities we have also the additional and probably most essential one at the present time, that the ores are easily mined and can therefore be obtained at a low cost.

Methods and Cost of Mining and Production.

There are no real difficulties in the way of mining the east Texas ores anywhere. This can be done altogether by open working, and the conditions under which the ores are found facilitate in every possible way the winning of them. In every field throughout the country the ores best developed and of the highest grade are invariably found as surface deposits, or lying at no great depth, beneath a covering of soft brown or brownish-gray sand. Wherever necessary, this sand can be moved readily and at a trifling cost. No skilled labor is required; and the ore can be obtained by any ordinary class of laborers at wages averaging about one dollar per day, and with no other implements than an ordinary pick and strong shovel.

In the case of the laminated ores, the overlying sand is removed with scrapers and the exposed ore-bed is broken up with picks. It is piled into heaps, roasted and then carried to the furnace.

In the neighborhood of Rusk and New Birmingham there are three companies practising this method of mining. The mines lie from a mile to a mile and a half from the furnaces to which they belong. In the case of two of the works, the ore is transported by tramway, and in the third by wagon.

In the northeastern portion of the ore-regions, where the ore is almost altogether nodular, and the masses of ore lie among loose sand in many cases without any covering at all, the extra expense of moving the surface material is to a great extent saved, and a much greater quantity of ore can be obtained within the same limits of time. In the case of these ores, however, some of the blocks or concretions are of such size that they require to be broken before roasting, but probably eighty per cent. of the ore exists in such size as to be easily handled.

The only furnace at present using these ores is the Lone-Star, at Jefferson, and the necessary transportation is all carried on by rail, the ore being brought for several miles.

In the present furnace-practice none of these ores are washed; nor do the furnace men consider such operation necessary. By this alone a considerable saving is made in the cost of production, as compared with other Southern states. The average cost of producing these ores, according to the Eleventh Census, is about $1.05 per ton when roasted. This I think is a little too high. It should only reach this figure in very extreme cases and should not under the present condition go over 95 cents or $1.

The average cost of producing pig-iron in the South as calculated by Mr. Carroll D. Wright from the returns of 24 furnaces appears to be $10.755, which he divides as follows:

Ore and cinders,		$3.482
Fuel (coke and coal),		4.462
Limestone,		0.470
Total for materials,		$8.414
Labor,	$1.524	
Officials and clerks,	0.164	
Supplies and repairs,	.614	
Taxes,	.039	2.341
		$10.755

The cost of manufacturing pig-iron in east Texas appears to be:

$2\frac{1}{4}$ tons of ore at $1.05,		$2.36
110 bus. charcoal at 6 cents,		6.60
$\frac{5}{10}$ tons limestone at $1.50,		0.75
Cost of materials,		$9.71
Labor and salaries,	$1.50	
Interest,	0.30	
Repairs and incidentals,	0.50	2.30
Total cost,		$12.01

This is $1.255 per ton more than the average cost given by Mr. Wright. The difference in the cost of production is altogether due to the higher cost of fuel and limestone but is more than compensated by the higher prices obtainable for the grades made.

These figures are based upon the estimated cost at the State furnace and two others, but will of course vary in other instances ac-

cording to location of furnace, cost of transportation, fuel, etc., all of which will have to be considered separately in locating a furnace.

Fuel.

With the existence of iron-ore in abundance our attention naturally turns to the next most essential requirement in furnace operations, viz: the question of obtaining suitable fuel in adequate quantity for smelting.

Within the area occupied by these iron-ores we have no mineral fuel that may be said to be adapted for the use of blast-furnaces as now constructed. Extensive deposits of lignite occur within close proximity to some of the heaviest ore-deposits, but this class of fuel, however useful it may be for many purposes, has not yet been demonstrated to be suitable for the operations of an iron-furnace.

Throughout some portions of northern Texas there are considerable areas occupied by beds of bituminous coal. These, however, lie at points far removed from the ore-fields and have no adequate transportation-facilities; so that their use may be considered impracticable. Besides, with these coals the seams are generally small, thereby increasing the cost of production, and judging from the few analyses made, their sulphur contents are too high to be safe. Looking at the question of the utilization of these coals from every point of view presented by our present knowledge of their conditions, they may be considered unavailable.

The most, and indeed the only, available fuel within the ore-areas is charcoal. Notwithstanding the reckless destruction and in many instances absolute waste of timber which has been carried on for many years throughout the area by the excessive number of saw-mills, the operations of railway-tie contractors, fires, cattle and the habit many farmers have of girdling the standing timber in order to destroy it on land to be cleared for cultivation, and afterwards piling the dead trees in heaps and burning them, the country east of the Brazos river may be said to be still heavily timbered with pine, various classes of oak, walnut, gum, hickory and trees of smaller growth. Extensive forests also occur in the portions of Arkansas and Louisiana adjoining Texas which may be considered, or may be made, tributary to the iron industries.

A recent writer, Mr. R. E. Kelley, one of the best authorities on Texas timber, estimates that there are about 25,000,000 acres of timbered land within the State, the greater portion of which is distributed among the forty counties lying east of the Brazos or within

the area of east Texas. Of these, twelve are within the immediate ore-districts, while most of the others lie within available distances. It is true that many of these counties are denuded, or partially so, of their pine; but the immense acreage of hard wood has as yet hardly been touched.

While the chief sources of the destruction of the timber are, as already stated, the numerous sawmills, large and small, scattered throughout the country (the extent of this destruction can readily be understood when it is stated that these mills have a total daily capacity of 4,367,000 feet of lumber), and the recklessly extravgant manner in which the contractors for railroad-ties execute their work, much of the loss might be avoided by the utilization of the waste products derived from both sources. It may be computed that, among the sawmills alone, something like 1200 cords, or an equivalent of 48,000 to 50,000 bushels of charcoal, are daily destroyed by fire at the mills in the form of waste, and this without considering the enormous loss in the form of trunks, tops and limbs left in the wood, of which no reliable estimate can be made; and while there are no means of determining the loss entailed by the tie-cutting interest, there can be no doubt that it amounts to a very large aggregate, all of which might be utilized for charcoal purposes, and a proper method of saving the by-products might largely decrease the cost of manufacture of the coal.

The climate of this portion of the State is in every way adapted to forest-growth. The general humidity and warm temperature are greatly in favor of the rapid growth of timber; and, as a general thing, the denuded portions of the country can, with a little care and attention, be rapidly covered with a dense growth of pine, so that these portions will offer a fresh supply of fuel every fifteen or twenty years.

The present destructive methods of dealing with these forests are rapidly exhausting the timber-resources of this portion of the State, and unless checked by some judicious legislation in the form of forestry laws, the whole of East Texas will, in a few years, be entirely devoid of timber.

The best estimate of the quantity of wood existing on the timbered areas gives about 30 to 35 cords per acre, or an equivalent of 1400 bushels of charcoal. This would be equivalent to using the wood from $4\frac{1}{2}$ acres in the daily operations of a 50-ton furnace.

Everything considered, it may safely be assumed that, with ordinary care, in the management of the forest-portions of the country,

and a due regard to economy in the preparation and use of the fuel, there is sufficient fuel-material within and adjoining the ore-fields to successfully operate enough furnaces to utilize the whole of the ores.

Flux and Markets.

While it may be accepted as settled that no very great difficulties may be encountered in the successful working of the east Texas iron-ores, either with regard to the ores themselves, either as to their quantity or purity, or the fuel-supply, yet their manufacture is not unattended with serious difficulties. In the first place, we have no fluxing material within the ore-regions. Limestone occurs at several points within the Tertiary areas, but it is generally too siliceous to be of use; and the nearest limestone of sufficient purity occurs within the Cretaceous belt, requiring a haulage of between 75 and 150 miles. Again, it may be truly said that we have no local markets in which to place the products of the furnace. It is true, we have two car-wheel establishments, a number of machine-shops, and several small foundries throughout the State; but the amount of pig-iron required by these establishments is small. A paucity of railroads through the ore-fields, and discrimination in the rates of those already there, have also a detrimental effect upon the successful working of our ores.

THE MANUFACTURE OF OPEN-HEARTH STEEL IN SWEDEN.

BY ERIK G:SON ODELSTJERNA, ENGINEER OF THE JERN-KONTORET, FILIPSTAD, SWEDEN.*

(Virginia Beach Meeting, February, 1894.)

Although the Swedes have not taken as prominent a position in the open-hearth as in the Bessemer industry, the successful development of which in Sweden has been described by Prof. Åkerman, in a paper read before the Institute,† nevertheless our work with the Siemens-Martin method may possess some interest, even for countries outside our own, and especially for the United States, where the charcoal-iron industry competes with that of Sweden for the first rank in extent and importance. I take the liberty therefore of giving in

* Translated by the Secretary.

† *Trans.*, xxii., 265.

this paper an account of our Swedish open-hearth practice, hoping that my honored professional colleagues on the other side of the Atlantic may find in it at least a few suggestions worthy of consideration. If any of them should be moved to visit Sweden, it would give me great pleasure to return in some small degree the abundant kindness received by me, during my journey in America, both at the World's Fair and at the iron-works of the United States, in the way of professional information and personal hospitality.

Already at the Paris Exposition of 1867, two officers of the *Jernkontoret* of Sweden, namely, K. Styffe, managing director, and L. Rinman, director, received from Mr. Pierre E. Martin a description of his recently invented process for the manufacture of steel without crucibles, by the fusion of pig and wrought-iron; and as the Siemens furnace had been for some years already in use in Sweden, in conjunction with F. Lundin's gas-producers, for wood and peat, to heat iron for rolling-mills and hammer-works, and Mr. Lundin even in 1867 had just melted cement steel in a similar heating-furnace, and obtained a useful product, the above-named gentlemen saw at once the importance for our country of this method of steel-manufacture; and when Mr. Styffe returned to his work as director of the Engineering Academy, Mr. Rinman, at the invitation of Mr. Martin, went to Sereuil, and made a very careful investigation of the new method.

After Mr. Rinman's return to Sweden in 1868, and upon the earnest recommendation of Messrs. Styffe and Rinman, the *Jernkontoret* granted the necessary funds for experiments with the method at the Munkfors works in Värmland, where a Siemens heating-furnace by Mr. Rinman was changed to a steel-melting furnace.*

These experiments were on the whole very successful, and clearly showed that steel could be advantageously melted with our Swedish fuel, wood and peat, provided the Siemens regenerators and the Lundin gas-producers with condensers were used; and Director Rinman was called upon during the next year to build two steel-works, one at Kilafors and one at Hellefors, and to commence also the working-plans for two other steel-works.

The first Swedish open-hearth furnaces were, as the drawings of the Kilafors furnace (Figs. 1 to 9 inclusive) show, very small in comparison with those of the present day, or even with furnaces in other countries at the same time. They were built for charges of about

* See *Jernkontorets Annaler*, 1869.

470 to 500 pounds only. One reason was that the owners of the works did not wish to go to the expense of more extensive plants, before they knew whether foreign consumers would accept the well-known Swedish iron in this new form. Moreover, it was already foreseen, and urged by several professional men—among them Prof. R. Åkerman—that the open-hearth method in our country would have to be planned and practiced under other conditions, and for other purposes than elsewhere, and that its success was therefore still to be proved.

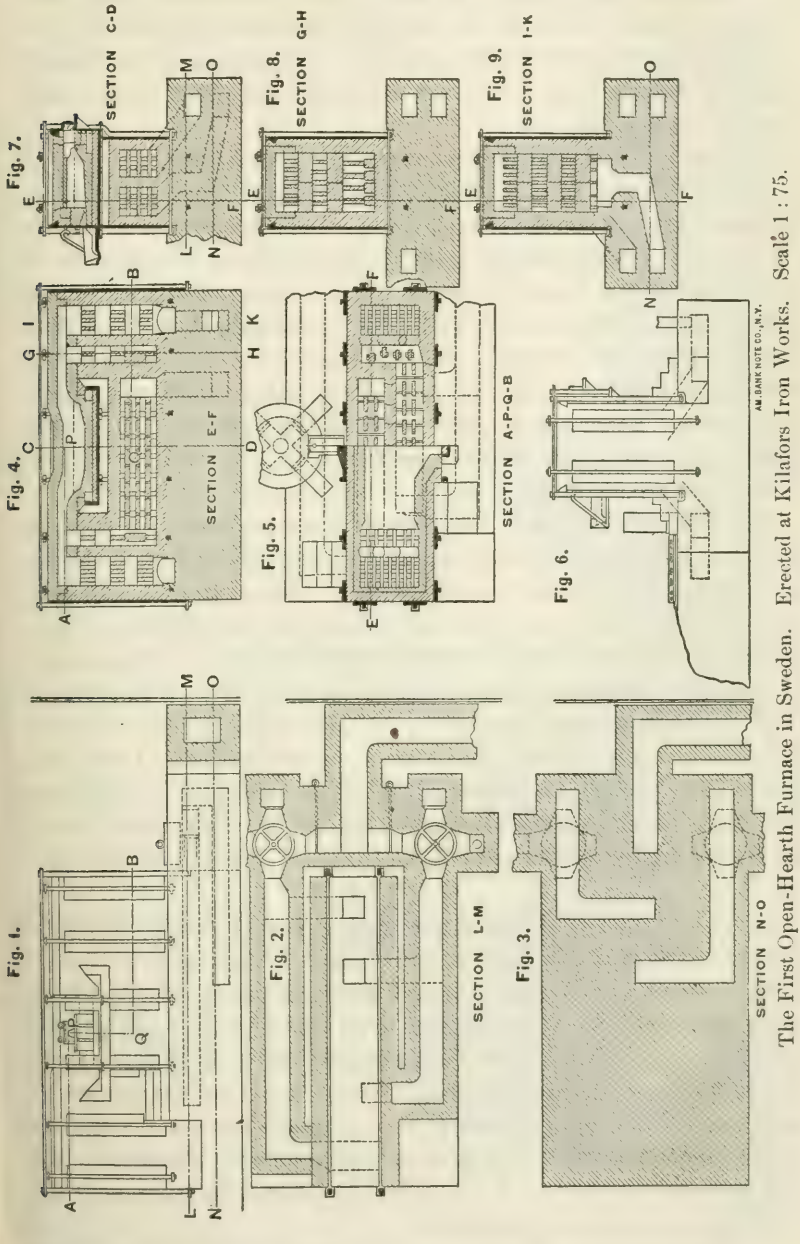
In other countries the open-hearth steel-industry was at that time specially based on the use of cheap scrap and pig-iron, to produce a second-class soft steel, and also on the use of relatively cheap low-phosphorus puddled iron, with as small a proportion as possible of more expensive low-phosphorus pig-iron, to produce hard-steel for domestic consumption. Not so with us. We have always been, and are still, obliged to sell most of our iron-product to other countries; and we have always to hold our market by maintaining the very best quality, since our expensive raw materials and transportation have made it impossible for us (at least up to the present time) to make cheap iron. We have therefore to aim chiefly at the production of the finest, softest steel and the very best tool-steel, to which has been lately added the manufacture of good steel for castings; and we must make those products in the way that is cheapest for us, that is, with the use of as much pig-iron and ore and as little wrought-iron and scrap as possible, because we have pig-iron, free from phosphorus and sulphur, a good deal cheaper than wrought-iron and scrap; whereas, the opposite relation between non-phosphoric pig-iron and puddled wrought-iron has obtained in other countries.

The iron-works proprietor, Mr. C. A. Rettig of Kilafors, and Director L. Rinman are really the gentlemen who established the Swedish open-hearth steel business, in that they employed with entire success charges of 60 per cent. of pig-iron and 40 per cent. of wrought-iron, and even with a greater proportion of pig-iron. Director Rinman successfully used rich iron-ore also for the process.

At the small furnaces first built, the fuel-consumption was of course very great—from 375 up to 560 pounds per 100 pounds of steel produced in furnaces of 470-pound heats.

In 1869 a furnace was built by J. L. Sebenius, carrying one ton at a charge and running with a fuel-consumption of about 275 pounds per 100 pounds of steel. After this, the dimensions of new furnaces were increased and their fuel-consumption reduced, until at

the end of 1878 there were seven steel-works in operation, with eleven furnaces, the highest charge-capacity being four tons, and the



The First Open-Hearth Furnace in Sweden. Erected at Kilafors Iron Works. Scale 1 : 75.

fuel-consumption about 200 pounds per 100 pounds of steel.

In that year we had started at the Bofors works the manufacture of steel castings, and had found that by this process, without forging afterwards, we could make, after the Terre-Noire method, steel castings which would compete for strength, toughness and solidity with the best foreign crucible-steel forgings. This increased the general faith in the open-hearth steel-process, and additional works with larger furnaces were built.

In 1882 we succeeded in constructing a gas-producer for coal (Figs. 14, 15 and 16, described below), with the help of which we reduced the fuel-consumption at our 10-ton open-hearth furnaces, until, at the present time, it amounts to only 20 to 25 per cent. of the weight of the ingots produced. As a consequence, even works which lacked sufficient forests to produce the needed wood for fuel, could build larger furnaces; and the demand for such constructions received a considerable impetus, particularly as our furnaces acquired an excellent reputation for good working and fuel-economy, so that even from Russia and Finland many orders for designs of open-hearth furnaces were received.

For the last few years, however, a decrease has taken place in new construction, as a consequence of the low prices commanded by the product. Yet these years have been of some benefit to us, since, by perfecting the process and improving the construction of producers and furnaces, we have attained a much smaller running-cost than before, while securing still better steel and steel castings. This latter statement I think all who did us the honor to inspect our iron and steel exhibits in Chicago can confirm; and I take this opportunity to express to Mr. E. C. Potter and Dr. H. Wedding the sincere thanks of the representatives of Swedish works for the very kind notices they have given in the *Iron Age* and in *Stahl und Eisen.*

A revival of the building of new furnaces in Sweden is now likely to take place, in consequence of the success of tests made at the Söderfors works with another type of producer for wood-gas, with a connected apparatus for drying the wood (see Figs. 17 to 21). This reduces the fuel-consumption nearly to one-half of what it has been, or for furnaces of 3-ton charges, to not more than 70 pounds of wood, and for 10-ton furnaces to only about 45 pounds of wood per 100 pounds of steel. This is a very considerable decrease in the fuel-expense, and makes it a good deal cheaper in Sweden to use wood than coal for gas-producers.

Gas-Producers.

The first gas-producer employed was that of F. Lundin, which

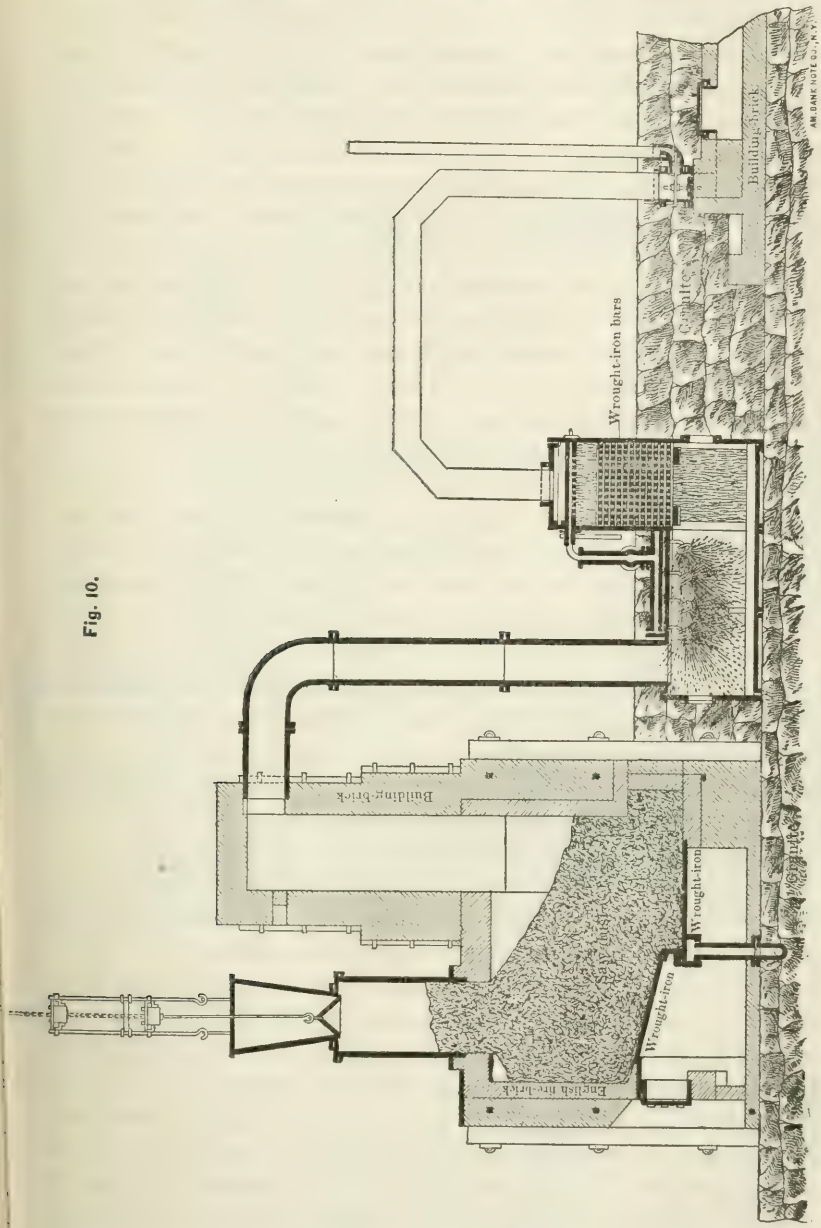


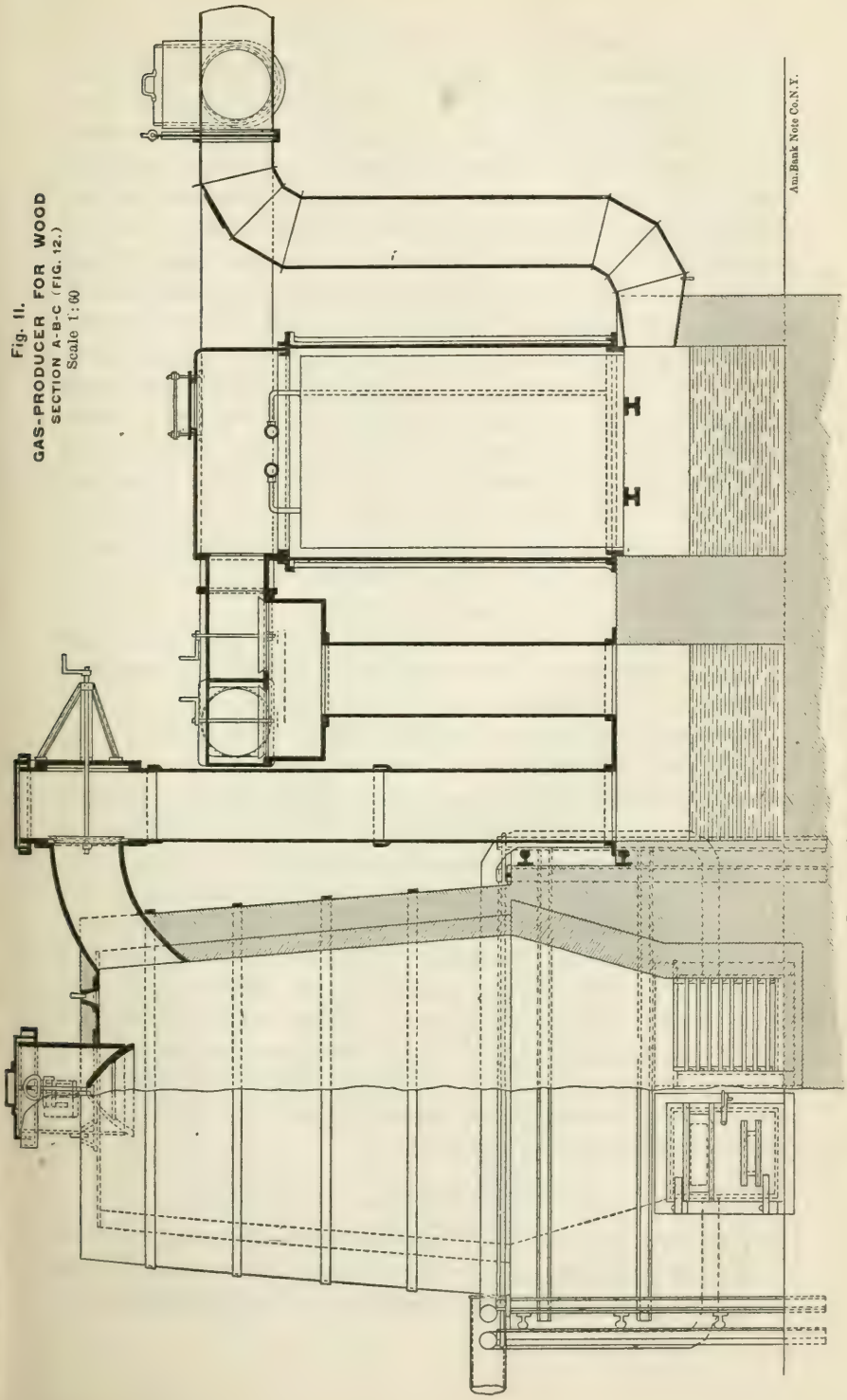
Fig. 10.

Lundin's Gas-Producer for Wood. Scale 1:75.
(Pipes and plates in black are cast-iron, except where otherwise specified.)

had a condensing-apparatus for the gas. The producer consisted, as

Fig. 10 shows, of a square generator, supplied at the bottom with a grate and at the top with a tight-fitting charging-funnel, having at one side an upright brick flue, from which the gas passed through a cylindrical cast-iron pipe down to the condenser. In the first section of the condenser, the gas was struck by a fine spray of water, and thence passed through a steady rain of water, up between iron cross-bars where the last of the vapors which could be condensed were collected, together with the greatest part of the tar. The gas then passed through pipes to the furnace-regenerators. Figs. 11, 12 and 13 show a modification of this producer, which has been more generally used of late years at the Martin furnaces. It is supplied with another type of condenser called the E. A. Wiman surface-condenser. The water which in the Lundin condenser was sprinkled in the gas, taking out vapors, tar and acetic acid, used to run, charged with these substances, into rivers where it poisoned the fish and made the water unfit for human use. This resulted in a good deal of trouble, involving law-suits and the payment of damages by the iron-works, as well as the ultimate prohibition of the discharge of this water into the rivers. Björklund then constructed a surface-condenser, consisting of brass tubes surrounded by cold water, through which tubes the gas was forced to pass. These tubes being difficult to keep clean, Wiman designed as a substitute his surface-condenser, which consists of thin plate-iron boxes, placed near together, into which the cooling-water is admitted at the bottom, to pass out at the top, while the gas passes between the boxes from the top downwards. This method makes it practicable to separate independently the water, tar and acetic acid from the gas, and permits the transportation of these noxious substances by rail to places far from the rivers.

The producer itself consists of a circular shaft, about 20 feet high, with its greatest diameter about half-way between top and bottom, and covered with a cast-iron plate, furnished with a bell-and-hopper. At the bottom of the producer is the ash-pit, with grate and inlet for the blast. This producer has been constructed according to calculations based on many analyses of the gases taken from producers of different sizes and produced from the three different water-containing fuels, namely, wood, peat, and saw-dust; and it is so calculated that it should deliver a gas with the least possible percentage of carbonic acid. For this reason it is larger than those previously employed. It consumes also less fuel for the same product than the previously-constructed condenser-producers.



Improved Gas Producer, with Wiman Condenser.

Figs. 14, 15, and 16 show the coal-gas producers as they were built as late as last year. They are a good deal smaller than the condenser-producers, and are supplied with step-grate and blast-connection. In Sweden we have always used blast, both for the production of the gas in the producers and for its combustion in our steel-furnaces; by which means we have avoided the building of high chimneys, and have been able to regulate completely the admission of gas and blast to the furnaces. For some years past the writer has had occasion to observe that the tar condensed out of our water-containing fuels gives more heat, if it is allowed to pass into the furnace and be consumed there, than the equivalent of the heat which the water-vapors in the fuel carry from well-constructed modern furnaces with large regenerators. In accordance with this observation, we experimented at the Söderfors works with both green and dry wood and refuse from saw-mills for firing the open-hearth furnace. These experiments led to the producer-type shown in Figs. 17 to 20 inclusive, with drying-apparatus (Fig. 21), by means of which such a considerable saving in the wood is made that it now becomes much cheaper to fire with wood than with coal.

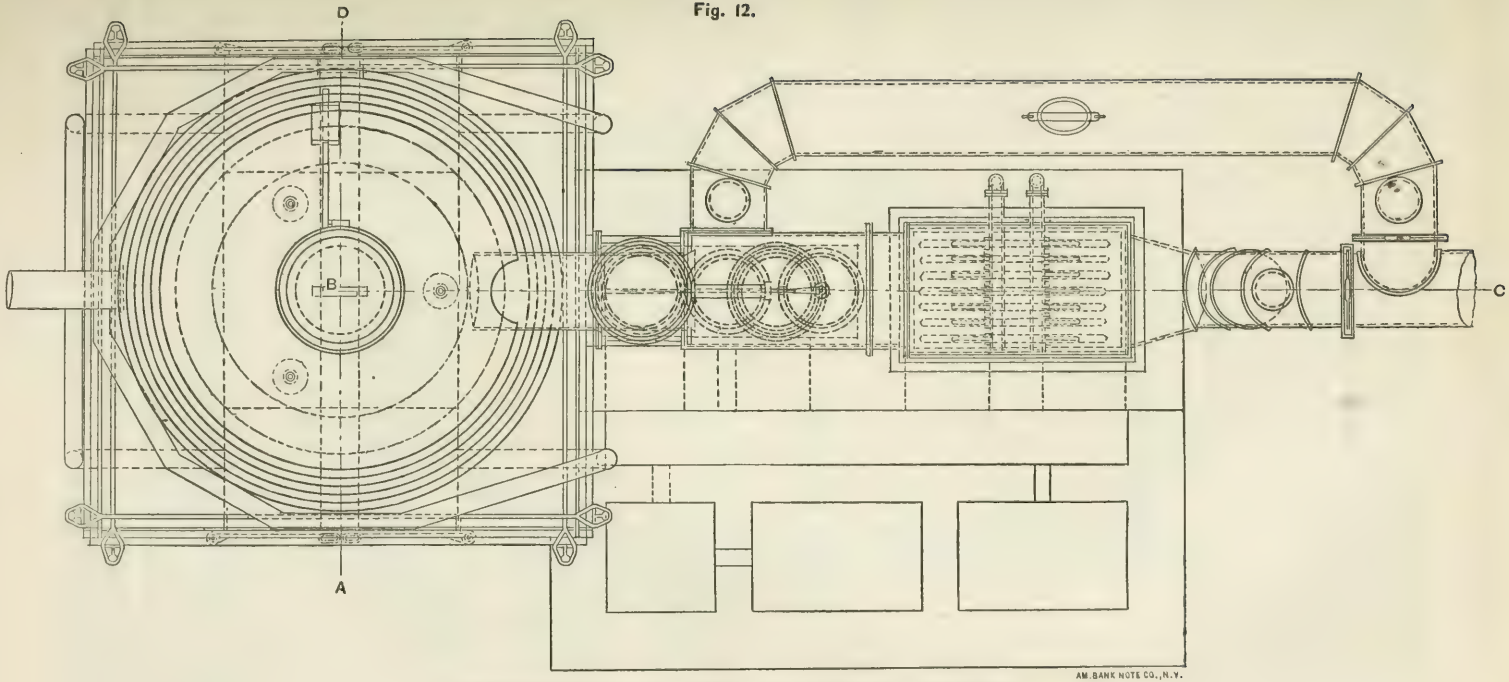
As the producer is not larger than one of our common coal-gas producers, it can be run with coal as well. No further description is needed, as it is sufficiently shown in the drawing.

The apparatus for drying the wood, shown in section in Fig. 21, consists, for a 10-ton furnace, of three steel-plate cylinders, covered on the outside with asbestos-board, and this again protected by a thin cover of wood. A certain small amount of dry wood is taken out at a time, while the same amount of fresh wood is put in, so that the apparatus works continuously. The drying-current is furnished by the products of combustion escaping from the air-regenerators of the open-hearth furnace. These, instead of passing to a chimney, are drawn by a suction-fan into and up through the drying cylinders. As the drying-current cannot be allowed to have a higher temperature than 120° C., an automatic heat-regulator is provided by inserting in the blast-pipe a copper wire, which, expanding as the temperature of the drying-current is raised, opens a valve admitting cold air, and *vice versa*.

The Steel-Melting Furnace.

Almost all our newly-built open-hearth furnaces are of the style shown in Figs. 22, 23, and 24. Since 1883, we have generally used very high-roofed furnaces with dome-arch and with alternating, or

Fig. 12.



GAS-PRODUCER FOR WOOD – HORIZONTAL SECTION.

Scale 1:60

so-called "gallery-ports" for gas and air. We prefer, for two reasons, this kind of ports. In the first place, as we like to use the largest practicable percentage of pig-iron, we desire that half of the charge should be melted in the hot-air currents coming from the ports. In this way, almost all the silicon and even a part of the carbon are oxidized during the fusion, so that the reactions in the bath can commence sooner. Again, a smaller part of the material is thus exposed to contact with the gas-flame. In works where coal or peat containing sulphur are used to make the gas, it is an advantage to have the metal less liable to take up sulphur during the melting. That this is not an imaginary benefit, is shown by the considerable reduction of the amount of ferro-manganese required in a furnace which, after having had the gas-ports below and the air-ports above, has been changed to the gallery-type. The difficulty was, formerly, that the partition-walls between the ports soon melted down; but we have overcome this by the use of fire-brick of more suitable dimensions and form. We have also been using for many years so-called dust-pockets for the collection of the spray of slag which accompanies the products of combustion from the furnace.

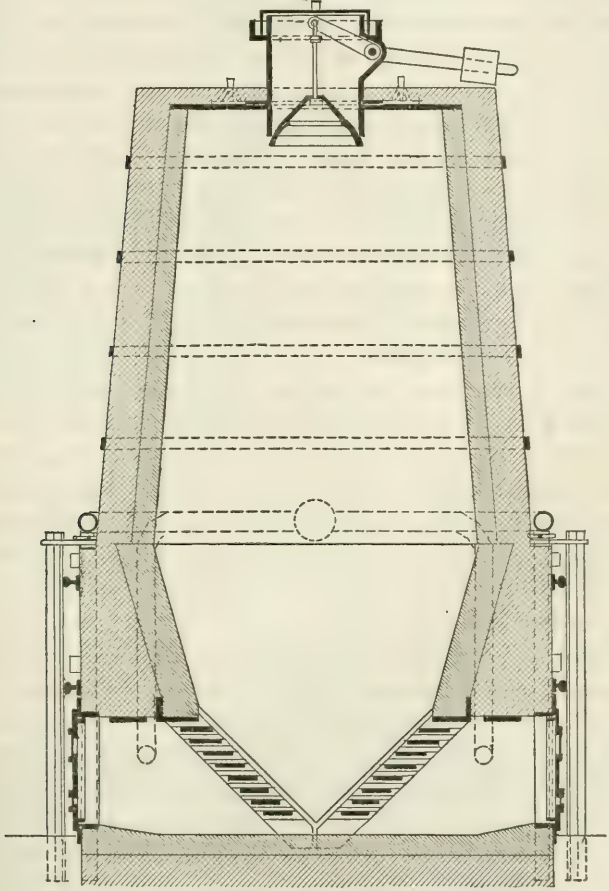
We let the ports incline downward to the bottom of the hearth, preferring to be able to get the bottom sintered quickly after repairs between the charges. This, however, certainly occasions a loss (though a very small one) in fuel, by reason of the less perfect combustion of the gas just after a fresh cold charge has been added. The use of the gallery-ports secures perfect combustion in the furnace as soon as the cold charge has become red-hot, and hence no smoke can be seen coming from the chimney of a Swedish open-hearth works, except for a short while after a new charge has been made, or at the reversing of the valves.

The regenerators at our open-hearth furnaces are very large as compared with those of other countries. We provide in each regenerator 2.5 cubic meters (88.3 English cubic feet) of regenerator-capacity per ton of steel, which the furnace is to give at each cast. That is, for a 10-ton furnace, each regenerator has a capacity of 25 cubic meters (883 cubic feet), or, for all the four together, 100 cubic meters (3532 cubic feet).

Before 1880 there was, here and there, an old furnace with wholly or partly horizontal regenerators, but as they never could be constructed so as to receive and give out in a fully satisfactory way the heat of the products of combustion, we now always use vertical regenerators. Moreover, to prevent loss of heat, we bury them as far

as practicable in dry ground, and the portion of the regenerator left above the ground is in most cases covered with asbestos-board, and this again protected with sheet-iron. Over the roof of the regenerator is laid a cover of about one foot of dry sand. The object of all this

Fig. 13.



GAS-PRODUCER FOR WOOD
SECTION A-D (FIG. 12.)
Scale 1:60

is to save fuel. Only the walls of the furnace proper have not this covering, but the brick is protected by one-inch cast-iron plates.

The best valves we have are the so-called American lift-and-drop dish-valves, which rest against water-cooled seats and are easily adjusted so as to be always entirely tight.

As our gas from wood and peat usually enters the furnace as cold

as the air, or colder, we make the gas-regenerators of the same size as the air-regenerators.

The Process.

As the fuel is with us the most expensive item (coal, for instance, costing us at the works about $6 per ton), we try, above all, to reduce the fuel-consumption to a minimum by covering the furnaces* and by driving the process as fast as is consistent with the best quality of the product. I suppose many foreign engineers may be surprised that, with our attention mainly directed on this point, we have, nevertheless, not got below a fuel-consumption (for coal) of 20 to 25 per cent. of the weight of the steel produced. But it must be kept in mind that, partly for the lack of good soft scrap and partly also because we believe that we get a better product the more pig-iron and the less scrap we use, we work with a high pig-iron percentage and consequently consume more time to each heat than is customary in other countries.

We have, for instance, steel-works here which use no wrought-iron at all when they make the very finest qualities of steel, but work only with pig-iron and ore. In such cases, however, the fuel-consumption rises somewhat above the figures given above, which are for charges of 60 to 70 per cent. of pig-iron and 30 to 40 per cent. of scrap respectively, melted in 10-ton furnaces, which is the usual size of our modern furnaces.

The largest furnaces we have are of 15 tons' capacity, which may be said to be very small for the present time and in comparison with what is used in America. It is, however, the common opinion here that the maximum advantageous size is reached at 10-ton charges, if strictly first-class tool-steel and steel castings are to be manufactured. Moreover, but few of our works make entirely soft open-hearth steel in quantities large enough to run 40- to 50-ton furnaces, and these works prefer, so far, to use three smaller furnaces instead of one large one, fearing to get an inferior and not entirely uniform product.

It is possible that on this point we are too conservative, and that we could learn to make in much larger furnaces just as tough and uniform metal; but as it is our object to produce an open-hearth steel in every respect fully equal to the best crucible-steel, the difficulties of a furnace of more than 10 tons charge-capacity are as yet too great

* The products of combustion leave our best furnace-regenerators at only about 200° C.

Fig. 14.
SECTION A-B-C (FIG. 15.)

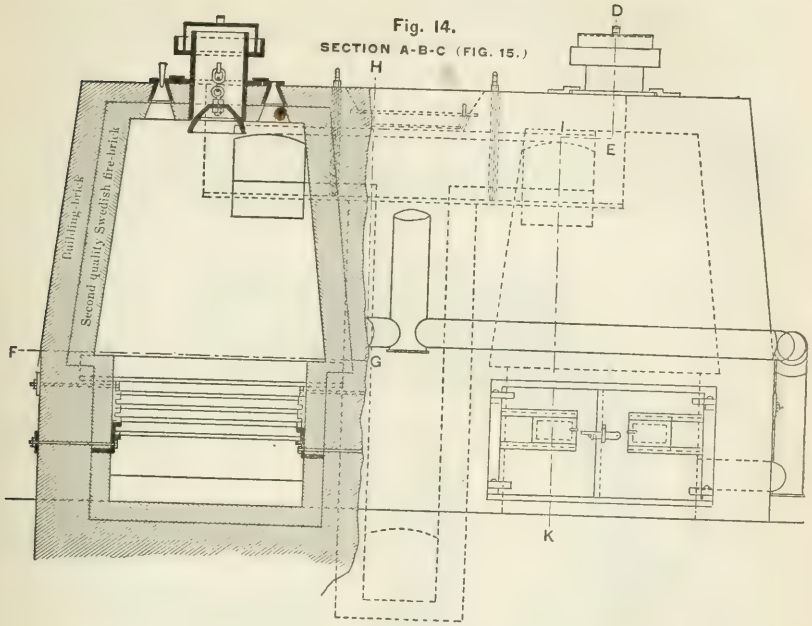
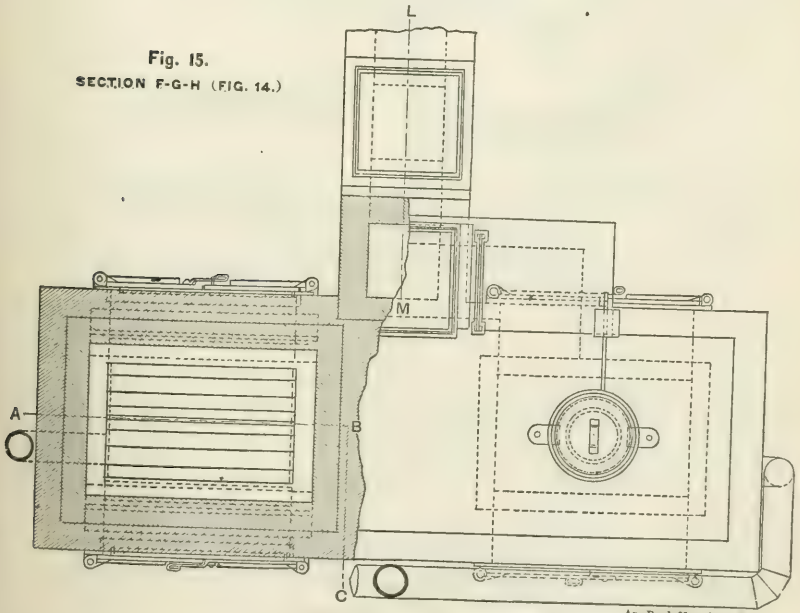


Fig. 15.
SECTION F-G-H (FIG. 14.)

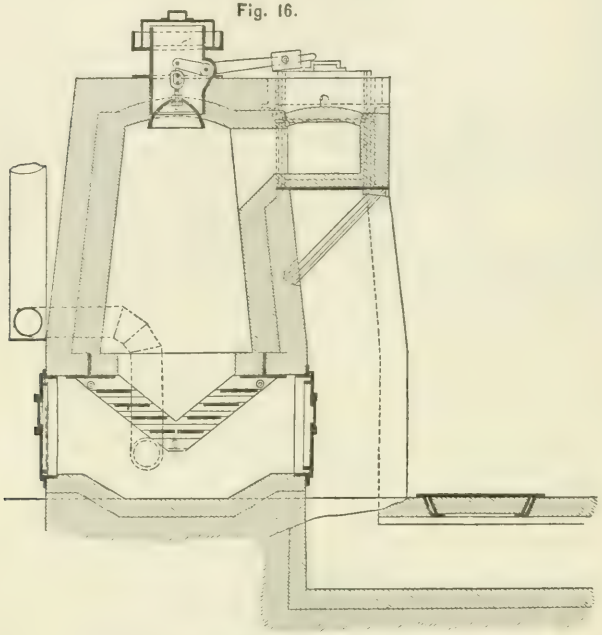


GAS-PRODUCER FOR COAL
Scale 1 : 60.

Am. Bank Note Co. N. Y.

for us. It should be added that most of our works make steel castings also, and that these are ordered (or ought to be) of different quality for different purposes; so that it is impossible to be always so well supplied with orders that charges of over 10 tons could be regularly tapped. But if a 25- or 50-ton furnace is run at times with small charges, to make castings, the fuel-consumption is ruinous; while, on the other hand, we do not like to run with full charges for

Fig. 16.



GAS-PRODUCER FOR COAL
SECTION D-E-I-K, (FIG. 14.) AND L-M (FIG. 15.)
Scale 1 : 60.

steel-castings and cast the rest of the steel into ingots, because we neither can nor care to sell such steel (in most cases containing silicon and manganese) as second-quality, to be used for rolling or forging.

It may be said that we make only three kinds of open-hearth steel:

1. The softest steel, containing 0.15 per cent. or less of carbon;
2. Tool-steel, containing 0.45 per cent. or more of carbon; and
3. Steel-castings.

Steel containing between 0.20 and 0.40 per cent. of carbon is seldom made, except at works which manufacture heavy plates. Of

these, however, we export a considerable quantity, chiefly to England, Finland, Russia, and Denmark.

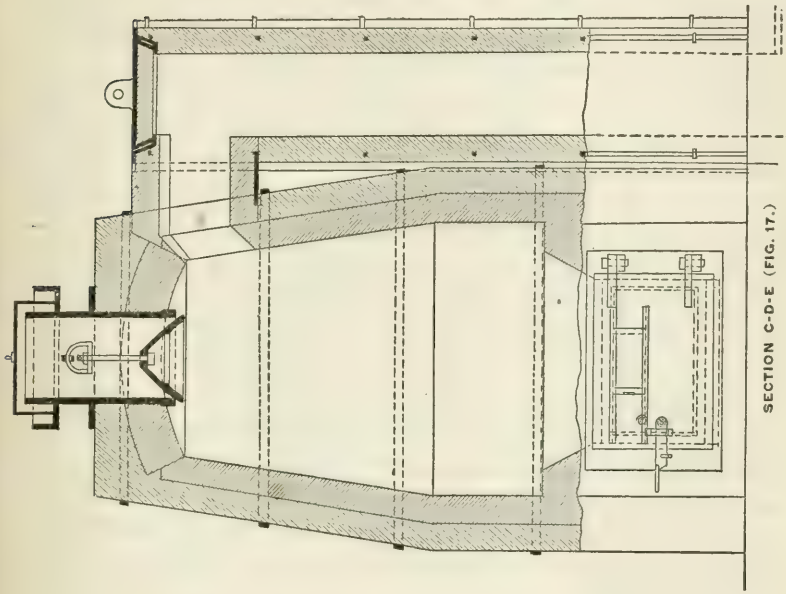


Fig. 18. GAS-PRODUCER FOR WOOD OR COAL Scale 1:40 SECTION C-D-E (FIG. 17.)

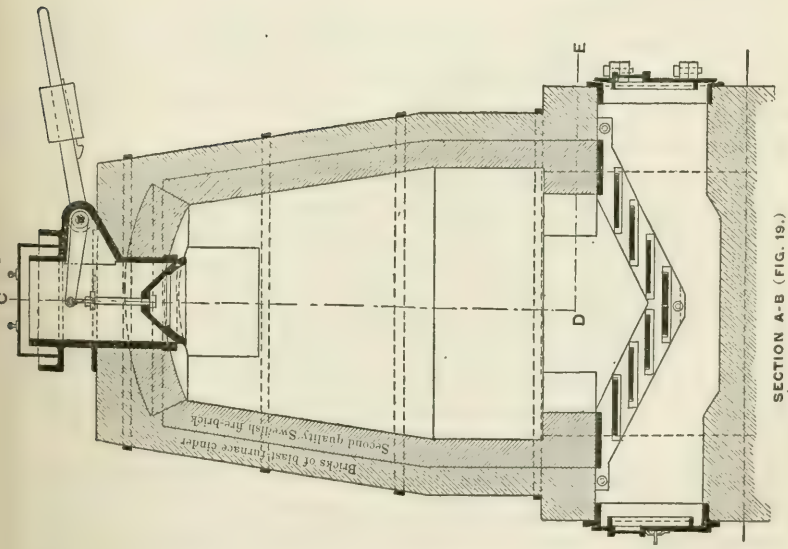


Fig. 17. SECTION A-B (FIG. 19.)

Am. Bank Note Co. N.Y.

1. *Soft Steel.*—In making the softest steel, great care is taken to

select pig-iron as free from sulphur as possible. The sulphur is under no circumstances allowed to exceed 0.015 per cent., if the best quality is required. It is also of importance that the scrap to be used shall be as perfectly free from rust as possible; for which reason many works clean the finer scrap in a rotating apparatus. The rust contains in most cases a considerable amount of sulphur taken up from the coal-smoke of factory-chimneys.

The pig-iron and the scrap are charged at the same time into the furnace, and after the mixture has been melted about three-quarters of an hour, the charging of ore is commenced. This ore is always of the richest kind (preferably not less than 64 per cent. of iron) and with gangue of quartz for the acid, and talc or lime for the basic method. It should either be originally nearly free from sulphur, or it must be roasted in calcining furnaces before use. Whatever sulphur it contains will be taken up by the iron, even in the basic open-hearth process. For the same reason the lime used in the basic process must be free from sulphur. This, however, is not a difficult requirement in our country, which abounds in the purest limestone.

The main thing in the making of the toughest Swedish soft open-hearth metal, is that the percentage of sulphur and oxygen in the metal at the end of the melt shall be so small that, if possible, no ferro-manganese need be mixed with it, or, at the most, only a few tenths of 1 per cent.

For many purposes, for instance in telegraph-wire and horse-shoe nails, it is necessary that no manganese shall be found in the metal, if it is to give satisfaction to customers; and we have, as the rule for *all* our products, "as little manganese as the requirements regarding red-shortness will permit."

The works which can use gas made entirely from wood, are more favorably situated as to sulphur than those which have to use partly or wholly coal; for although, in most cases, when coal is employed, we use a considerable amount of lime in the producers, to absorb the sulphur, nevertheless this gas does, as, of course, the wood-gas does not, give some sulphur to the melting iron. In using coal-gas, therefore, we cannot avoid the addition of from one-fourth to one-half of 1 per cent. of manganese; nor do we get absolutely first-class steel.

It is also of importance that the proper temperature shall be maintained during the whole heat. If the melter, at any time during the melt, lets the charge become what we call "cold," the metal will be red-short; and this defect follows to the end, in spite of later

raising of temperature, and necessitates a greater addition of manganese. One-fourth of 1 per cent. of manganese decreases the tough-

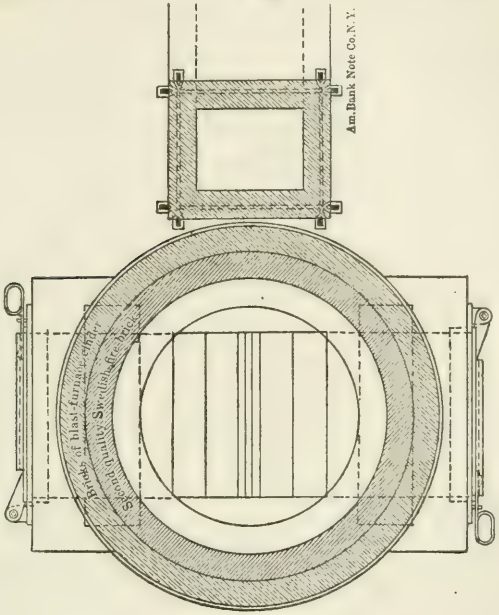


Fig. 20.

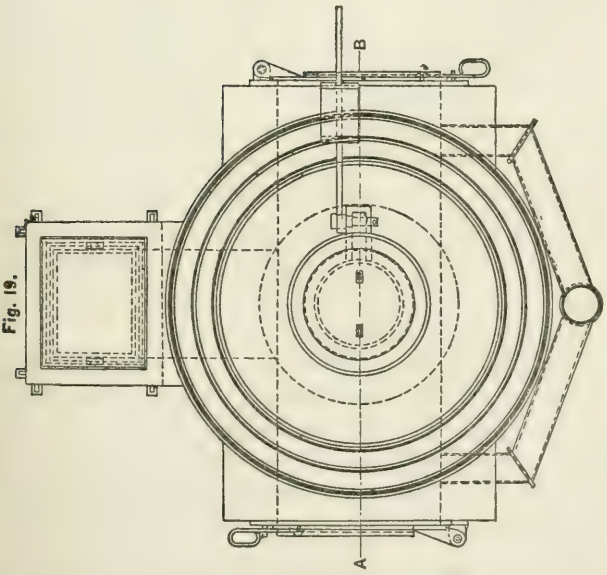


Fig. 19.

GAS-PRODUCER FOR WOOD OR COAL
Scale 1:40

ness of the steel more than an increase of phosphorus from 0.020 up

to 0.045 per cent., which latter is the highest phosphorus-percentage in first-class soft Swedish open-hearth steel. On the other hand, as we do not want to have silicon in our soft open-hearth steel, the furnace must not be so hot that the bath will take up silicon from the sides (in the acid furnace). It must be kept so that the ingots shall show some blow-holes, but free from oxide-coating and placed a considerable distance from the surface, preferably in a ring around an absolutely solid core. The blow-holes, however, must not be too numerous or too close together; because if the ingot is rolled in a mill with gothic or triangular grooves, it may happen that the material between the blow-holes is so much crushed in rolling as to make a more brittle steel than otherwise would result.

After J. A. Brinell published his investigations on the influence of heat upon the texture of soft steel,* we commenced at some works to investigate whether the quality of the steel was the same, whether the ingots were left to cool off entirely before being heated for rolling, or were rolled with their initial heat, that is, were only partly cooled before they were placed in heating-furnace or soaking-pit, and then rolled. For this purpose, we permitted every alternate ingot of the cast to be entirely cooled, and the remainder to go warm to the heating-furnaces or the soaking-pits. Tests of the plates after rolling showed, as we had feared, that the steel possessed less ductility when it had not been entirely cooled and re-heated before rolling. We think, therefore, that the use of Gjers soaking-pits is not advantageous for steel which is to be finished at one operation, or which is not, in subsequent treatment, to be raised to bright-red heat at least, as for instance ship- and boiler-plates, material for railroads and bridges, and plates for cold-cut horse-shoe nails, etc.

Again, if the ingots are to be rolled to blooms or billets, which are allowed to cool before they are further worked, it makes no difference whether they are put in the soaking-pit hot, or cooled off and then re-heated. The difference in ductility, however, is only of importance where strictly first-class steel is wanted.

2. *Tool-Steel.*—For the manufacture of hard, first-class tool-steel pig-iron as low as possible in phosphorus is, of course, required, and is made from our most non-phosphoric ores, with the use in the blast-furnace of charcoal nearly free from phosphorus. Tests made by Prof. R. Åkerman† have shown that the charcoals delivered from the saw-mills in Norrland, and made of wood from which

* See *Jernkontorets Annaler*, 1885.

† See *Jernkontorets Annaler*, 1888.

the bark has been taken off before it is floated down the rivers, are those most free from phosphorus. If such charcoals cannot be had cheaply enough, we use, following the same authority, local

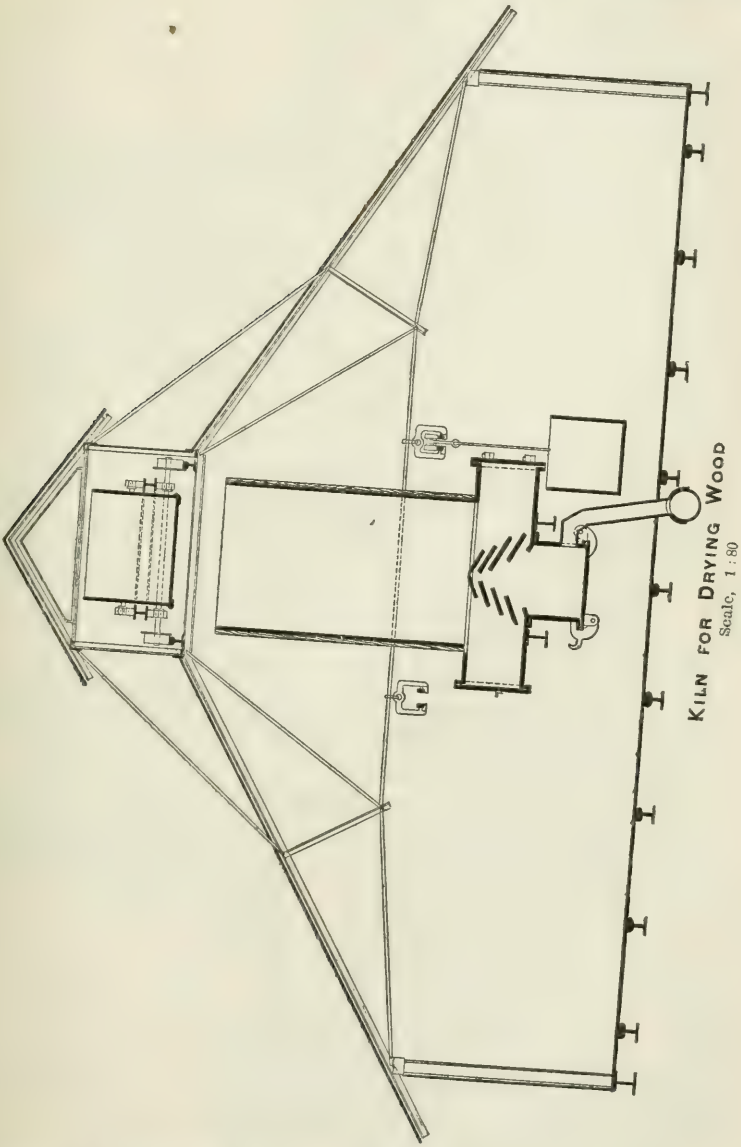


Fig. 21.

KILN FOR DRYING WOOD
Scale, 1 : 80

pine charcoals, avoiding the more phosphoric charcoals made from other kinds of wood.

In hard steel we prefer to keep phosphorus under 0.03 per cent., and call no steel first-class which has 0.035 per cent. or more. Our best sorts contain 0.022 per cent. and less. The sulphur in the pig-iron has not quite as much influence on this steel as it has on the soft steel, because the effect of manganese on the toughness is not as noticeable with the higher percentage of carbon, and therefore more manganese can be added; but the pig-iron and steel are not allowed to contain more than 0.02 per cent. of sulphur—if, indeed, even this does not require too great a percentage of manganese. This is not a very difficult condition; for with the careful roasting of the ores which we always perform at the blast-furnaces, and with our sulphur-free fluxes, it very seldom happens that the percentage of sulphur in the pig-iron is higher than the figure given above.

The steel-melting is so conducted that a good steady heat is kept in the furnace during the process, in order to get the steel, if possible, entirely free from gas, at the time of tapping, without too high a percentage of silicon; that is to say, so that the ingots may be solid and free from blow-holes, without too much tendency to pipe.

If the steel is coming from the furnace too hot, so that piping is to be feared, it is either kept long enough in the ladle to sink to the proper temperature, or the so-called "De Laval" heat-rings are used. These are iron rings lined with fire-proof material heated to very high temperature, which are placed on top of the moulds, and, in casting, are filled with steel, which remains melted in them, and sinks until the ingot below the ring is entirely solidified.

Probably the danger most difficult to avoid in the making of hard steel is getting it too hot, which renders it liable to surface blow-holes. We have found, however, that such blow-holes are formed only when the melter, at some time during the process, has let the furnace become so cold that the molten mass has taken up too much gas, and afterwards has attempted to repair this fault by forcing the temperature at the end of the heat. Under proper discipline, the tapping of such steel has therefore become more and more rare. It is certain, however, as was shown at the World's Fair at Chicago, that we pay more attention to this matter than do the steel-makers of other nations; for among the steel-ingots exhibited from other countries such faults, and especially pipes, were common. While we are not able to sell any open-hearth ingots to England if they have "pipes" or surface blow-holes, most of the small crucible-ingots, even, exhibited from England had such features.

3. *Steel Castings.*—In the manufacture of steel for castings we

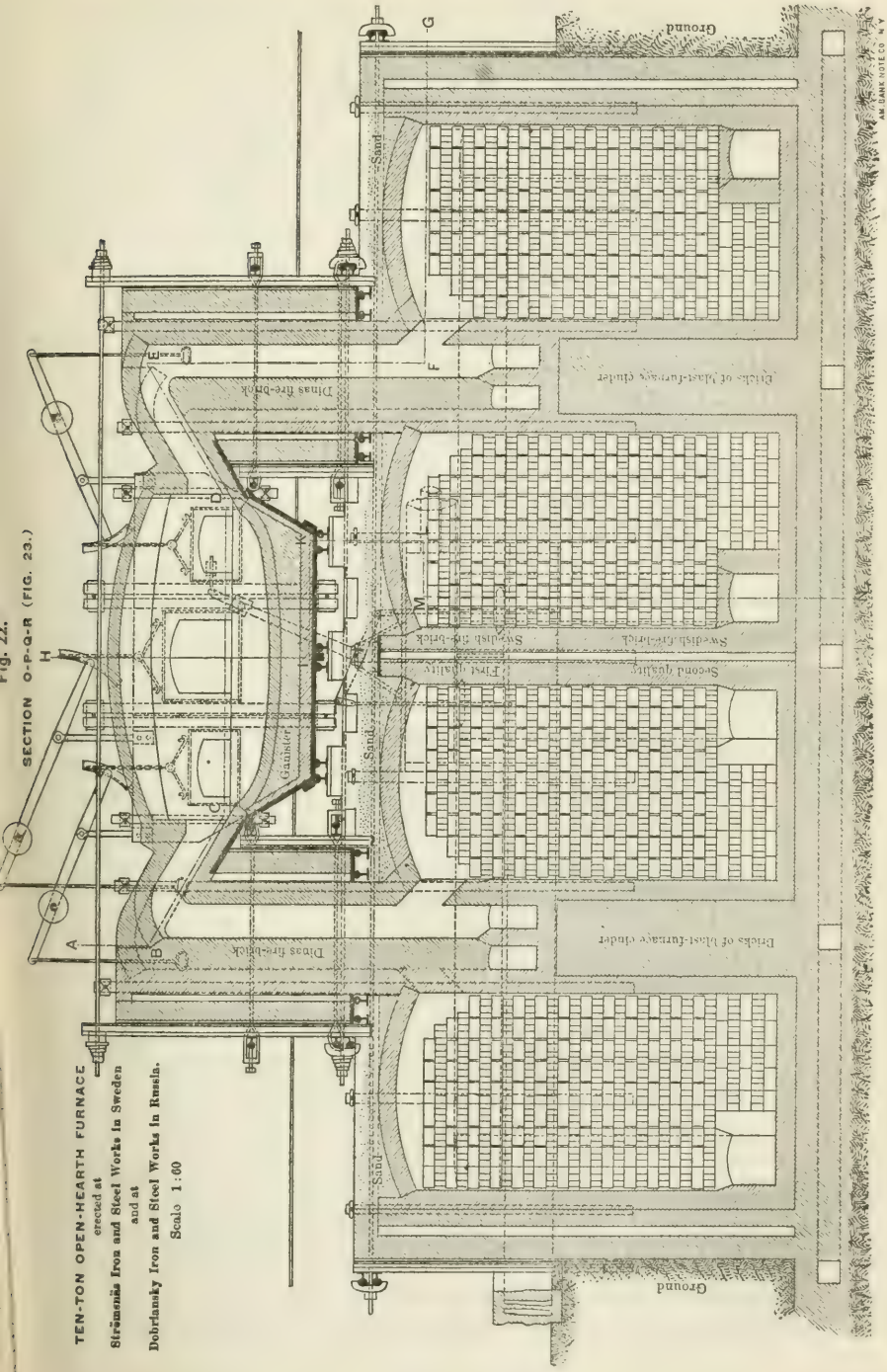


Fig. 22.

SECTION O-P-Q-R (FIG. 23.)

TEN-TON OPEN-HEARTH FURNACE
erected at
Strömsnäs Iron and Steel Works in Sweden
and at
Dobrianshy Iron and Steel Works in Russia.
Scale 1:60

AM. BANK NOTE CO. N.Y.

had at the outset many difficulties to overcome, because our furnaces were not suited to the business. They were made very shallow, in order to permit the pig-and-ore process to be operated with rapidity; that is, the furnace was very long and wide in proportion to the size of the charge. This causes, of course, in the making of steel for castings, too quick heats, with too much liability of the metal to take up a large amount of gases, and also, after the addition of ferro-silicon, too much oxidation of the silicon before it has had time to be dissolved in the bath.

At the steel-casting works—all of which make soft steel also—we have, therefore, to select a middle course, and to increase the depth of the hearth (thus extending the duration of the heat), although we cannot increase it enough to get the ideal of a furnace for steel castings, because the hearth would then be entirely unfit for the production of soft steel. It follows also that to make steel castings of perfect quality, we have to maintain a very high temperature in the furnace during the melting; that is to say, so high that the metal never becomes free from silicon, whereby the absorption of gas is resisted. For this reason the furnaces are much more severely strained in this manufacture than in making steel to be rolled, and they do not last half as long.

Another result is that we almost always have to tap the steel a little too hot, although we possess a protection against over-heating in the charging of the relatively cold ferro-silicon, near the end of the operation. This excess of heat, however, is of use in the manufacture of small castings; and for large ones we can always let the steel stand and cool in the ladle. In casting small pieces, it is kept hot enough to be tapped from the big ladle into crucibles or hand-ladles, and from these into the moulds, whereby a finer surface is obtained, because the metal is not poured in such a heavy stream as to detach loose particles from the surface of the mould.

The main point in the production of steel castings is, and will always be, that the moulds shall be made of proper material. We use for this purpose partly a kind of red sandstone of a very suitable coarse grain, and partly the purest, preferably amorphous, quartzite, containing 99 per cent. or more of silica. Both of these are ground to the right degree of fineness. It is very important that the material shall be neither so fine as to prevent steam and gases from passing through it, nor, on the other hand, so coarse that the steel can press into the pores, which would give a rough surface to the castings.

Molasses, a solution of carpenter's glue, beer, wheat-flour, or

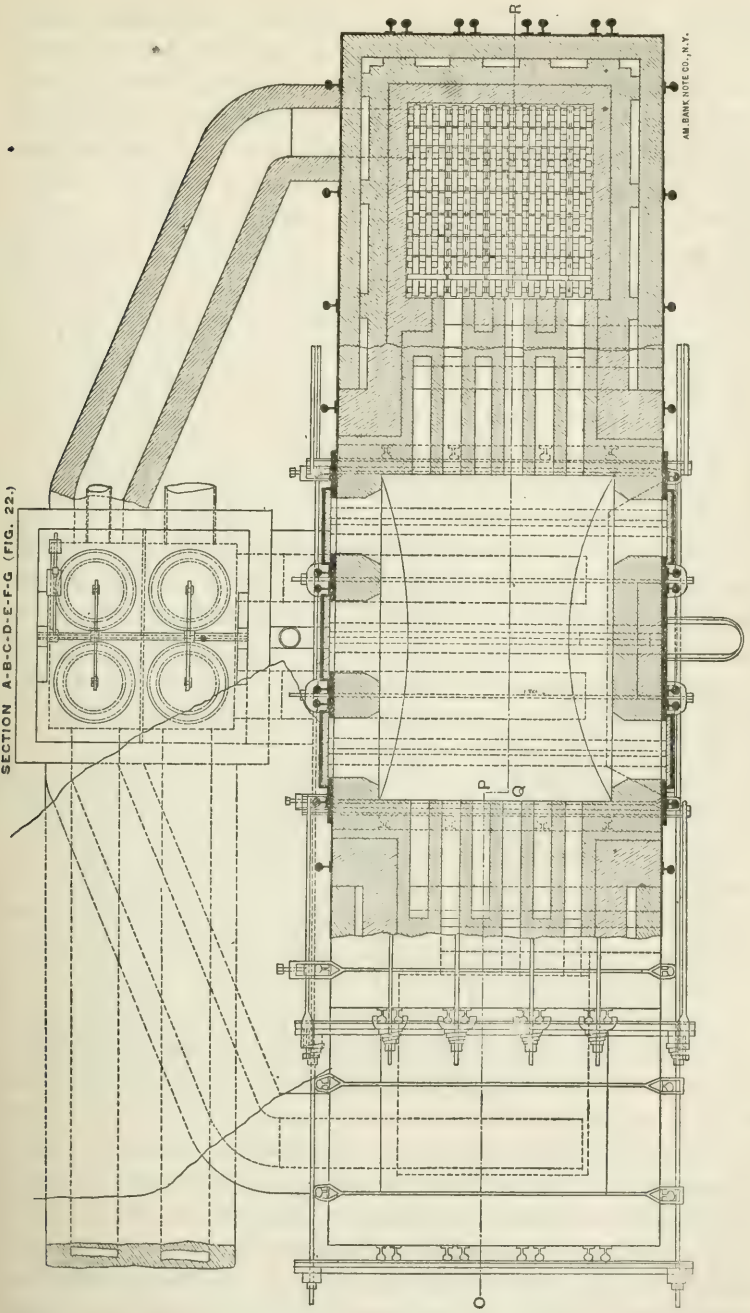


Fig. 23.
SECTION A-B-C-D-E-F-G (FIG. 22.)

TEN-TON OPEN HEARTH FURNACE
erected at Strömsnäs Iron & Steel Works in Sweden and at
Dobrianesky Iron & Steel Works in Russia.
Scale 1:60

Dutch clay, is used to bind the material together. For facing, we use either the finest quartz-dust, or (as at most works) siliceous earth (*Kieselguhr*), which, if pure, is beyond comparison best. At the beginning, we bought such material in England, under the name of "Rosmedium," and possibly this does well for steel cast so cold, as is the practice with the more phosphoric and more fusible English metal; but with our steel the facing burned fast to the castings and gave them a bad-looking surface. We have now succeeded in getting a German *Kieselguhr* which is unusually pure and good, and when stirred into a solution of carpenter's glue, gives the most handsome surface.

The moulds should be well vented and dried, at least for small castings, until every trace of moisture has disappeared. Heavy castings are not so sensitive; they will always drive the moisture out of the mould-mass; and it takes a very careless moulder or melter to make them unsound.

In order to save the expense of maintaining so high a temperature during the melt, we have practiced, for some years past, in the manufacture of goods of minor importance, such as common trade-castings, the addition of about 0.002 per cent of aluminum at tapping; but, believing that we have found the aluminum to decrease the tensile strength of the metal, we do not use it when the strongest product is desired. As may be easily seen if a steel ingot from a charge containing aluminum is broken in two, this element gives the steel a tendency to arrange itself in large crystal-faces from the outside towards the center. By such an inspection, it is easy to discover in an ingot or large casting, whether even a small amount of aluminum was used in the manufacture or not. These crystal-faces are also the reason that it is much easier to break cold by blows a steel ingot containing aluminum than one free from this metal.

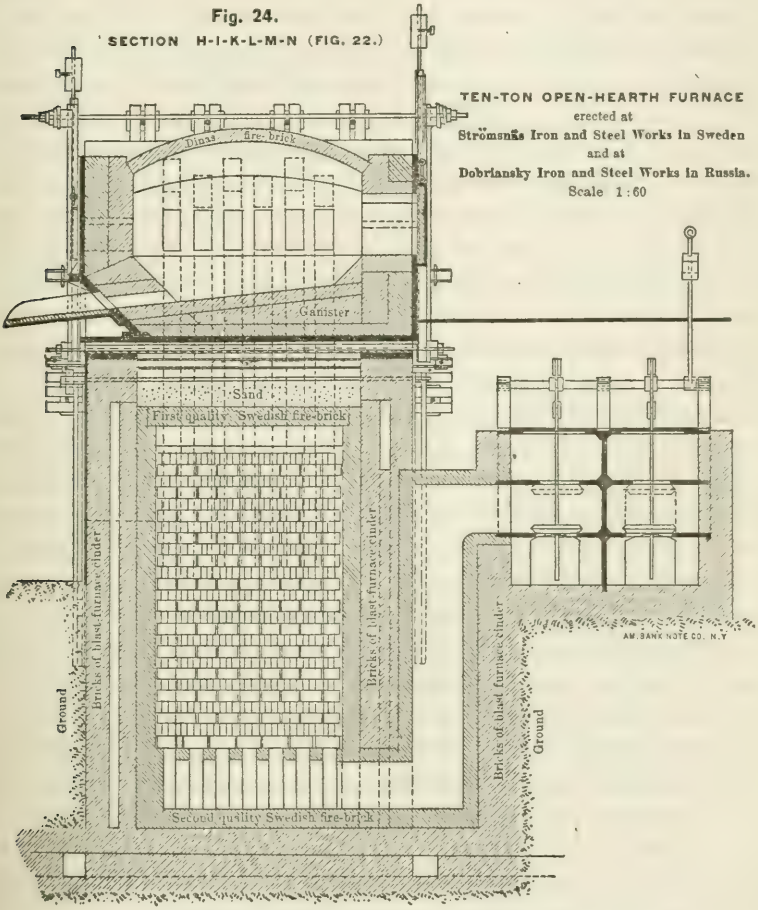
It is indeed true, this crystalline structure is to some extent destroyed by annealing the castings; but probably the destruction is not complete, and we cannot permit our product to have the least tendency to a crystallization which would impair its toughness, because it is subjected to the severest tests. For instance, it must successfully compete for guns and armor-piercing projectiles with the forged steel of other countries, although our products are not forged at all, but, after casting, only annealed and tempered.

I have recently seen in the *Iron Age* that a leading authority in this field says he has found that from 0.85 to 0.95 per cent. of manganese has no bad effect. As I have not had the opportunity to test

foreign steel castings, I will not question this assertion so far as they are concerned ; but tests for ductility, and especially for drop-tests at low temperature, made of our open-hearth castings, have shown that they possess most toughness where the silicon is 0.25 per cent. or less, and the percentage of manganese is *as small as possible*, and that

Fig. 24.

SECTION H-I-K-L-M-N (FIG. 22.)



the quality of the steel will be the very best, when the desired high tensile strength has been obtained by increased percentage of carbon, or, still better, by the addition of a sufficient amount of nickel or chromium, or often both these metals combined.

As I have said, we regard manganese in steel castings and tool-steel as a necessary evil, and in the softest steel as an *unnecessary* evil ; I except, of course, the so-called manganese-steel with high

percentage of manganese, which we have indeed tried, but without much satisfaction.

At the Finspong gun-works, we have now commenced to make even plates for armor-towers by direct casting, without any forging afterwards; and although this has been done as yet to a small extent only, it is clearly shown that the right way to make armor for fortifications is, without doubt, to cast the plates and then only heat and temper them after machining, thus omitting entirely the forging or rolling. Only a small increase in thickness is required to make them equal to rolled plates; and they are so much cheaper to make, that they can be cast to great advantage a good deal heavier than any of our present rolling-mills can make their plates. To this may be added, that armor-plates made in this way can be cast with strengthening ribs, and in shapes impossible for rolled plates. The one thing necessary is that metallurgical engineers and contractors unite their experience to perfect the details of practice.

I deem it unnecessary to state the theory of the open-hearth process as we operate it, since Messrs. H. H. Campbell and Russell W. Davenport have already, in 1893, so admirably explained it on the basis of American practice, and most of what I could say would only repeat nearly what they have brought forward. I will only add, therefore, that, in the last few years, we have been going over to the basic process, which in my opinion will, in all probability, gain a still stronger foothold among us. At the same time, I wish to say most emphatically, that we have not at all changed to this method in order to get a better product than we were making already; for I hardly think we can do that. Our sole reason for the change is the saving of time and fuel. At a number of our basic furnaces, magnesite linings, and at others dolomite linings, are used; and we believe, as do the engineers at the basic works in America, that the former are the best; but, in many places, we prefer the dolomite, for the same reason as they do in America, namely, because we have a good deal of it in our own country, and therefore can get it cheap, while the magnesite must be imported from Germany.

The operation of the basic process is the same here as in America, except that we, of course, do not have to tap any phosphoric slag towards the end of the process, owing to the low percentage of phosphorus in our material. But since, even in the basic furnace, we try to use as much pig-iron and as little scrap as possible, we have to add about as much lime, to counteract the silicon of the pig-iron. The greatest difficulty we have had to overcome in the basic method has

been to make solid, hard steel and steel castings without adding aluminum; but this has been overcome by the addition of ferro-silicon and ferro-manganese, previously melted in a crucible, to the steel in the ladle.

A peculiarity in both our acid and our basic open-hearth process is, perhaps, that we prefer not to recarburize. We tap the charge when the forge-test and the carbon-test show the required degree of hardness; and if, in filling a very pressing order, the melter should have let the steel go too low in carbon, so that we are forced to recarburize, we do this always by adding a good deal more pig-iron than is needed to supply the carbon, and letting the charge boil a few hours longer, in order to get an entirely homogeneous product. It was common, before 1880, to recarburize, if necessary, by adding just as much pig-iron as was needed to bring the carbon to the right degree; but, in the beginning of that year, a works which had delivered plates containing 0.20 per cent. carbon to a steel-bucket factory, received complaints that, in the shaping, small particles of adamantine hardness were pressed out of the soft plate; and these were found to be white pig-iron from the recarburizing-pig. Microscopic examination of the fracture-surfaces of tensile test-pieces has shown that even the ferro-silicon and ferro-manganese, added towards the end of the heat, partly remain in granular form, if sufficient time has not elapsed between the addition and the casting.

According to the reports of the Royal Department of Commerce, the production of open-hearth steel in Sweden was, in 1886, at 13 works, 22,460 metric tons, and, in 1892, at 22 works, 76,556 metric tons. This is (in proportion to the size of our country) a rapid growth, both in number of works and in aggregate product, the former having been nearly doubled, and the latter more than trebled in six years. At the present time, there are two new works under construction, with furnaces of 10 tons' capacity.

A TWELVE-MILE TRANSMISSION OF POWER BY ELECTRICITY.

BY THOMAS H. LEGGETT, BODIE, CAL.

(Virginia Beach Meeting, February, 1894.)

A NUMBER of papers on the use of electricity in mining operations have been presented at the Institute meetings recently, but only a very few of them have given particulars and details of the

application of this power. It is therefore believed that a description of one of the most extended transmissions of power by electricity in operation to-day may be of interest.

At Bodie, Cal., the ruling price for wood has been, for years past, $10 per cord, so that the monthly fuel-bills of a 20-stamp mill, crushing and amalgamating 50 tons of ore per day, would often amount to $2000. To reduce this excessive cost of motive-power was the problem in hand, and the use of electricity generated by water-power has solved it. No sufficient water-power could be found nearer than 12½ miles, the distance from Bodie in a straight line over the hills to the east flank of the Sierra Nevada.

Eighteen months ago, when the installation of this plant was under consideration, the limitations of the direct current for such a lengthy transmission were by no means as well-established or as generally admitted as they are to-day. Therefore, when an eminent electrical engineer, and strong advocate of that system, coolly proposed to transmit the 120 horse-power, required at the motor-end of the line, by using the continuous current with 5000 volts on the commutator of the generator, he was listened to with profound respect, not unmixed with the admiration that the eloquent electrician usually elicits from the untutored layman, when he expatiates upon the possibilities of electricity and asserts what can be done, though perhaps he may never have tried to do it.

Electricity has undoubtedly accomplished wonderful results during the past decade, proof of which was afforded in abundance at the World's Fair, but there are still some electrical feats that have not been attempted; and this, so far as the writer is aware, is one of them.*

There has recently been installed near Genoa, Italy, a continuous-current plant, transmitting 300 horse-power a distance of 18 miles to that place.† At the power stations (of which there are three, one below the other) there are four groups of dynamos, each group of two dynamos driven by turbines (Piccard system) of 140 horse-power, under heads varying from 225 to 495 meters. These are connected in series, one group being held in reserve, and produce each a current of 47 amperes and 1000 volts, the resulting electro-

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* "Electric Transmission of Power Long Distances," by W. F. C. Hasson, *Trans. Technical Soc. of the Pacific Coast*, vol. x., No. 4, p. 51.

† *Bulletin de l'Association des Ingénieurs Electriciens sortis de l'Institut Electro-Technique Montefiore*, 2d series, vol. iv., p. 179.

motive force sometimes reaching 6000 volts during the hours of maximum load. The motors are also connected in series, no one machine, it will be noted, carrying a potential exceeding 1000 volts at any time.

This necessary "series" arrangement of dynamos and motors forms the great drawback to the use of the continuous current for long-distance transmission, more especially where the range of work to be performed is wide and varied.*

Nearly three years ago the Westinghouse Electric and Manufacturing Company installed a 120 K. W. generator for the San Miguel Consolidated Mining Company, at Telluride, Colo., and transmitted 150 horse-power a distance of 3 miles by the single-phase A. C. synchronous system, successfully operating the 40-stamp mill of the Gold King mine, the generator being driven by Pelton water-wheels and working under a potential of 3000 volts. During the past year this plant has been increased by the addition of an 800-horse-power generator, from which a current is transmitted 8 miles to a 250-horse-power motor, driving a 120-stamp-mill of the same company. The excess of power is to be utilized in operating other stamp-mills in the vicinity.

After consultation with Mr. W. F. C. Hasson, electrical engineer, of San Francisco, it was decided to adopt this single-phase synchronous system as the cheapest in copper and the simplest for the work required, and the contract for the electrical apparatus was let to the Westinghouse Electric and Manufacturing Company. At that time, neither the three-phase system (requiring three wires) nor the Tesla two-phase (requiring four wires) had reached their present state of development in this country. In the Niagara Falls transmission, the above company is to use this two-phase system, which is undoubtedly better adapted to a variety of work than the single-phase, since the motors are self-starting.

An interesting installation of a three-phase plant has recently been made by the General Electric Company at Redlands, San Bernardino county, California; though the distance of transmission is but five miles. There are two A. C. generators of 250 K. W. each, driven by four 30-inch Pelton water-wheels at a speed of 600 revolutions, double nozzles to each wheel. The differential governor described later on in this paper, is used. The generators carry a

* "Long-Distance Transmission," by Prof. George Forbes, in *Electricity* (N. Y.), September 20, 1893, p. 115.

Fig. 1.

STANDARD CONSOLIDATED MINING COMPANY

Water and Electric Power Plant,

at Green Creek,

Near Bodie, Mono County, California.

Scale: 1 Inch = 1800 Feet

DATA:

DITCH 4560 ft. long; section, [illegible]; grade, 0.2 ft. per 100 ft.; capacity 3,500 miners' inches.
PENSTOCK. 12 ft. x 12 ft. x 6 ft., capacity, 540 miners' inches (1.6 cubic ft. to the inch.)
PIPE LINE. 1,571 ft. long.
655¼ ft. of 18 inch diam., No. 12 iron C and S joints.
502¾ " 20 " " 14 " slip joints.
413 " 22 " " 16 " "
RECEIVER Drum of ¼ths Steel, 9' 6" x 40", with 18" Gate Valve, Safety Valve, etc.
EFFECTIVE HEAD 350 feet.
WATER WHEELS. 4 21-inch special Pelton Wheels, 230 H. P. Max., 865 Revs.
GOVERNOR One Doolittle Governor, one No. 2 Pelton Motor, 1,410 Revs.
DYNAMO One No. 2, 120 K. W. Westinghouse A. C. Generator, coupled direct to wheel-shaft, E. M. F., 3,530 volts.
EXCITER. One Type C Exciter with rheostat 8,050 Revs., driven by One No. 3 Pelton Motor, 910 Revs.
COST OF WATER POWER per H. P. $14.00.

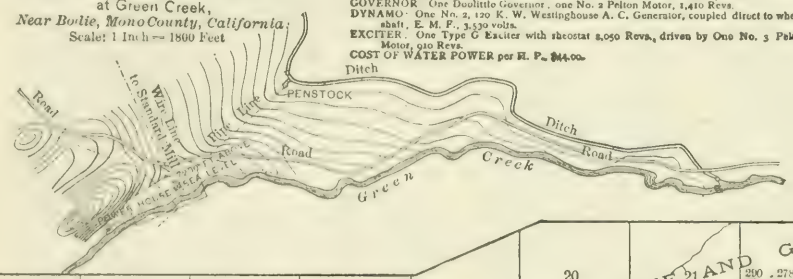


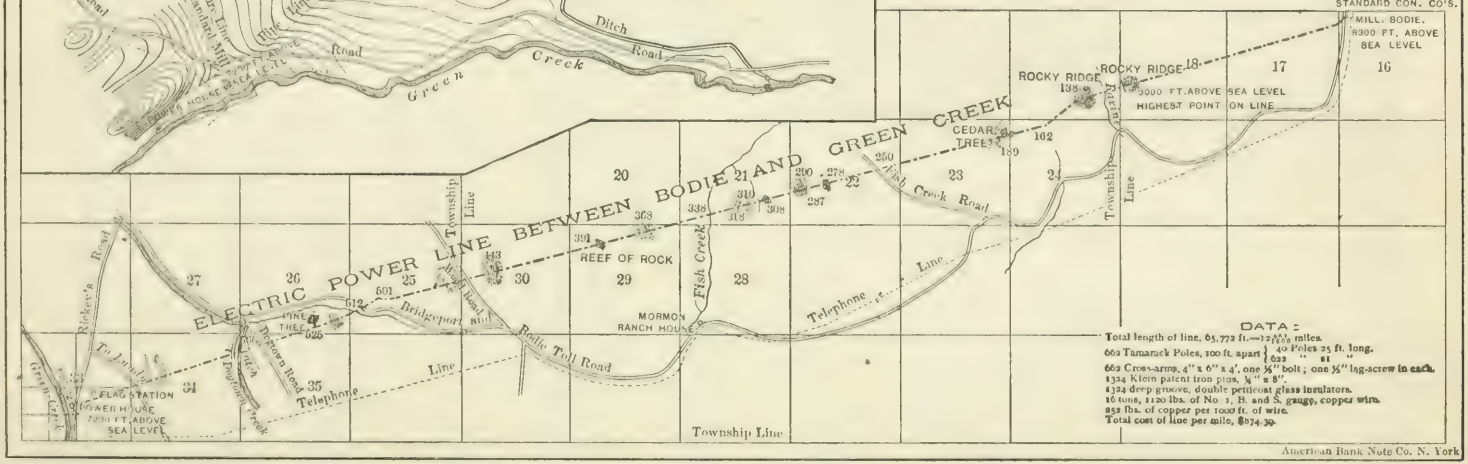
Fig. 2.

ELECTRIC POWER LINE BETWEEN BODIE & GREEN CREEK

Mono County.

California.

Scale: 1 Inch = 900 Feet



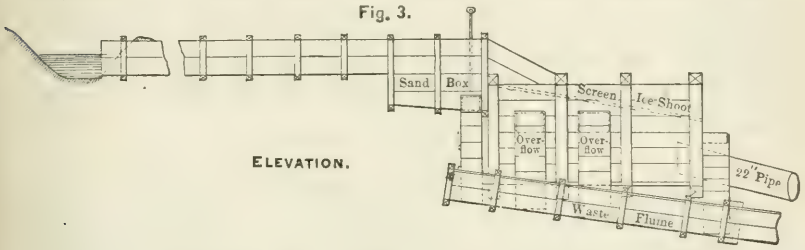
DATA:

Total length of line, 65,772 ft.—12 $\frac{5}{280}$ miles.
662 Tamarack Poles, 100 ft. apart { 40 Poles 25 ft. long. 622 " 21 "
662 Cross-arms, 4" x 6" x 4', one ⅝" bolt; one ½" lag-screw in each.
1324 Klein patent iron pins, ¾" x 8".
1324 deep groove, double petticoat glass insulators.
16 tons, 1120 lbs. of No. 1, B. and S. gauge, copper wire.
852 lbs. of copper per 1000 ft. of wire.
Total cost of line per mile, $874.39.

American Bank Note Co. N. York

potential of 2450 volts and the motor about 2150, the line-loss being approximately 12 per cent. There are three No. 0, bare copper wires (insulated within the city limits) carried on deep-groove, double-petticoat glass insulators. This size of wire permits a subsequent enlargement of the plant. The line-poles are 35 feet long, 6 feet in the ground, and set 110 feet apart. The one motor at present in operation, is a synchronous high-potential machine, of 150 horse-power, and has continuous work to perform in driving the ice-machines of the Union Ice Company. The initial current in the fields of the motor is generated by a small exciter, and the motor is

Fig. 3.

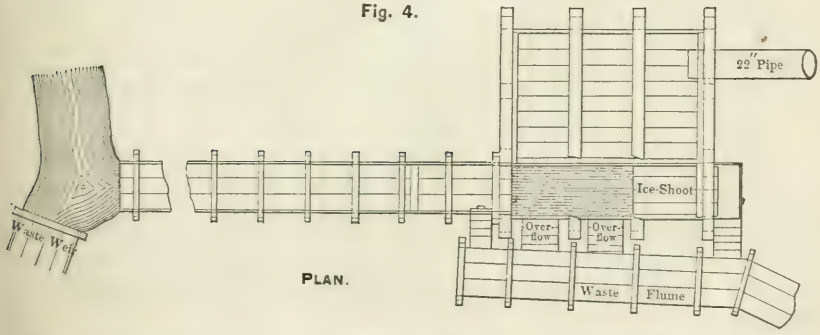


PENSTOCK AND FLUME.

Scale ¼ in. = 3 ft.

1893.

Fig. 4.



self-starting only under light load, the full load being thrown on after the machine is up to speed. The operation of this plant is stated by the manager, Mr. H. H. Sinclair, to be extremely satisfactory in every respect.

The General Electric Company has also a plant at Hartford, Conn., consisting of one 300 K. W. tri-phase generator, transmitting power 11 miles to one 300 horse-power synchronous, self-starting tri-phase motor. This motor in turn drives 500-volt direct-current

railway-machinery. The voltage of the tri-phase generator and synchronous motor is 500, and the line-voltage 5000, obtained by means of step-up and step-down transformers. The Company states that this plant has been in operation now something over eight months, and has given excellent satisfaction.

At Bodie, the distance of transmission (12½ miles) is just at that intermediate point where the cost of transformers about equals the difference in cost between a No. 1 and a No. 6 copper wire (it is not advisable to use any lighter wire than No. 6, on account of its liability to rupture during storms). Hence it was deemed better not to use converters, since they would only complicate the apparatus, without effecting a saving in cost. This conclusion seemed perfectly correct at the time ; but experience with the plant in operation tends to favor the use of potentials not exceeding 2000 volts in machines of such small capacity.

Water-Power Plant.

An excellent water-power was found in a mountain stream on the north slope of Castle Peak in the Sierra Nevada, known as Green creek, and forming one of the chief sources of the East Walker river. This stream carries 400 inches of water during the dry season, and ten times that amount during the time of melting snows.

An old ditch was cleared out and rebuilt for a length of 4570 feet and a site was selected for a power-house, 355 feet vertically below its lower end. The ditch was made larger than necessary for power-purposes alone, with the object of supplying other parties, when there was an excess of water.

The maps, Figs. 1 and 2, give the data with regard to the ditch and pipe; and Figs. 3 and 4 show the connecting flume, pressure-tank and waste-weirs. The arrangement of the screen adopted, while it occasions a loss of head of a couple of feet, is greatly to be recommended where "anchor-" and slush-ice form in a ditch during cold weather.

The pipe is of large diameter, in order to permit subsequent enlargement of the plant, and also to reduce loss of head by friction. It is fitted with three 2½-inch air-valves, to prevent collapse in case of sudden rupture, and is anchored at proper intervals with straps of 1¼-inch round iron. The slip-joints extend to a vertical head of 220 feet, the remainder of the pipe being laid with collar-and-sleeve lead joints.

The pipe leads into a receiver, 40 inches in diameter and 9½ feet

long, from which four taper-pipes lead the water on, under pressure of 152 pounds per square inch, to as many 21-inch Pelton water-wheels, each wheel being fitted with two nozzles, and rated at 60 horse-power under the largest sized tips of $1\frac{1}{3}$-inch diameter.

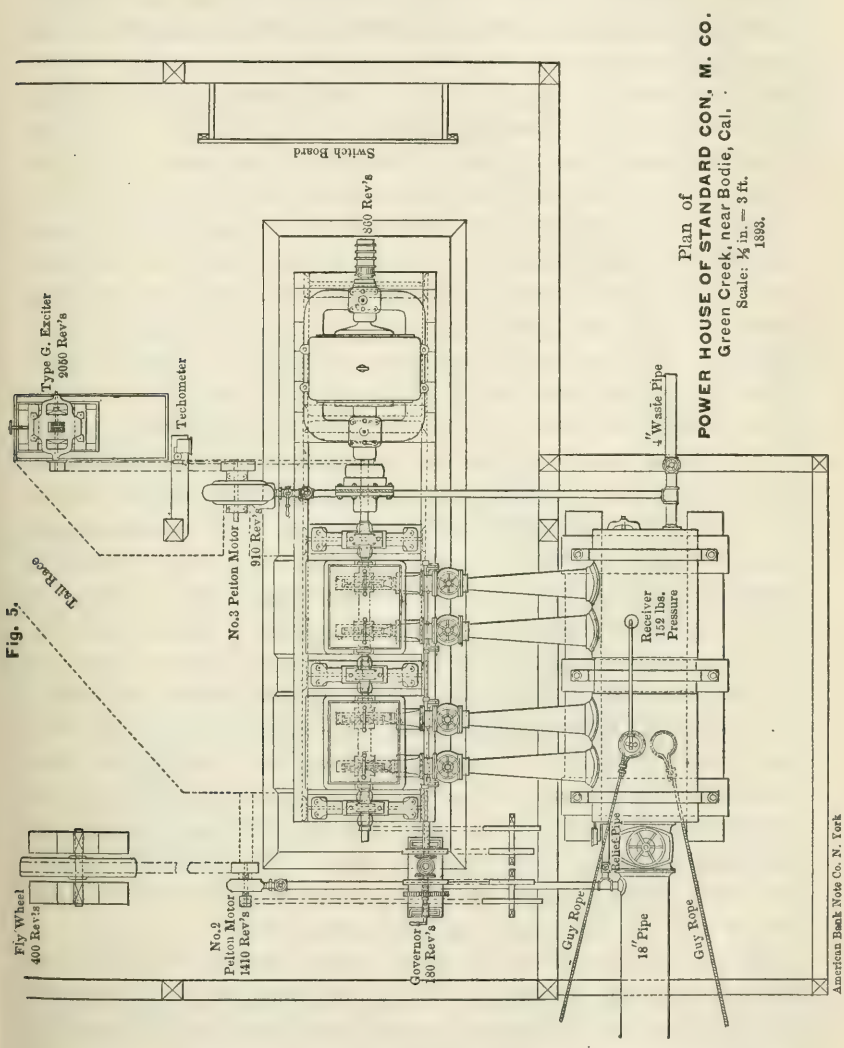


Fig. 5.

The speed of the wheels is 860 to 870 revolutions, and their shaft is connected by an insulated rigid coupling to the armature-shaft of a 120 K. W., A. C. generator. Plate I. shows the generator and water-wheels in operation.

Power-House.

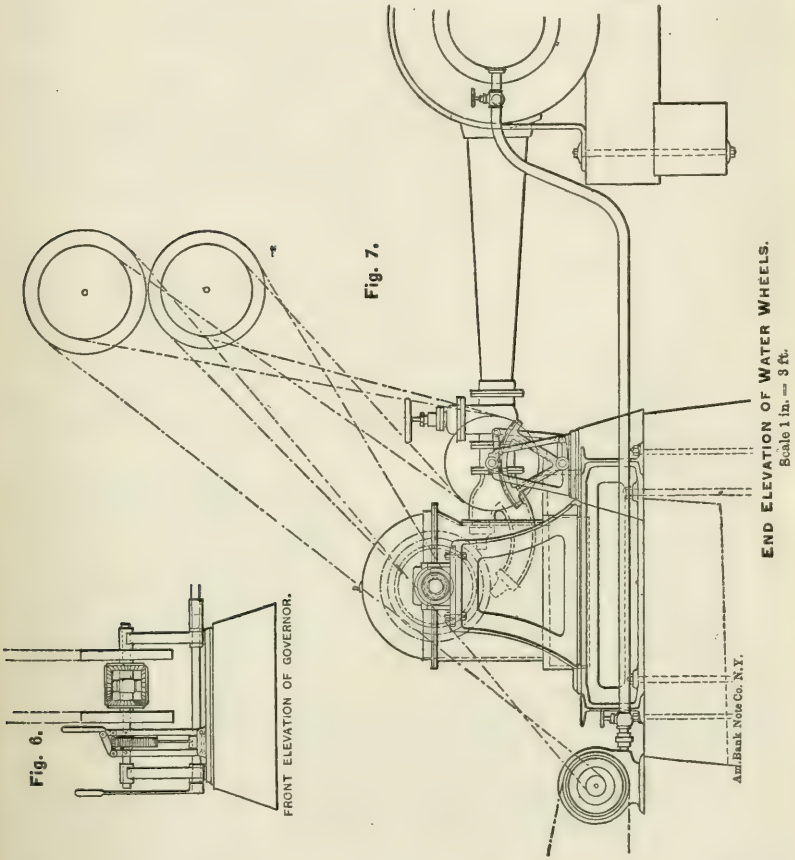
The accompanying plan, Fig. 5, shows the arrangement of the plant, one of the most interesting features of which is the water-governor formerly known as the "Doolittle," and now called the Pelton differential governor, Figs. 6 and 7. It operates butterfly-valves placed in the 5-inch pipes between the gate-valves and the diverging nozzles; and though this form of valve invariably "throttles" the water to a greater or less extent (according to the position of the valve), it is a most satisfactory way of controlling the power where the same is ample, and the loss due to this cause is of slight consequence. The governor operates as follows:

Two 18-inch pulleys revolve loosely and in opposite directions on a shaft, one being driven from the water-wheel shaft and the other by a No. 2 Pelton motor. These pulleys have gears on their hubs which mesh into two other gear-wheels carried on an axis at right angles to the shaft and keyed fast to the latter. Beyond these wheels is a pinion, loose on the shaft and with ratchet-teeth cut in opposite directions on either side of its hub. Into these ratchet-teeth mesh corresponding circular ratchets, which are keyed to the shaft but free to move longitudinally along the same, and are thrown in or out of gear by a short lever and spring. The pinion engages a sector, which is fastened to the rod and levers that operate the butterfly valves, and on the same rod is a hand-lever, by means of which the valves may also be opened or closed by simply throwing out of mesh the circular ratchets alluded to and thereby detaching the governor. It is evident that when the two pulleys are revolving in opposite directions at exactly the same rate of speed, there will be no motion of the central gear-shaft, and none will be communicated to the pinion and sector and thence to the valves, to open or close them; while, on the other hand, a difference in speed of these pulleys will have the opposite effect. The belts driving them are therefore so arranged that a decrease in speed of the water-wheels will open the valves, and an increase will close them.

In starting up from rest, the governor is detached by throwing out the springs on the ratchets, and the valves are operated by the hand-lever. After the wheels are at normal speed and the load is on, the ratchets are sprung into gear with the pinion, and the governor takes care of any and all variations, even to a complete throwing off of the load by pulling the main-current plug-switch at Bodie. The speed of the governor-pulleys, as first designed, was 60 revolutions. This was found to be too slow, and it was increased to 180 revolutions

with most beneficial effects, developing a greater sensitiveness to small changes of load, and much quicker action, especially when all the load was thrown off at once. In the latter case, the increase in speed of the water-wheels did not at any time exceed 12 per cent. before the governor began to close the valves.

It was further found necessary to furnish a constant resistance for the water-motor, that drives one side of the governor, to work against.



In the original plan this was to be done by the exciter which furnishes current for the fields of the generator; but on trial it appeared that the load on the exciter was too variable, and at times too great for the little motor to take care of. The exciter was then placed so that it could be driven by either a larger size (No. 3) motor or by the water-wheel shaft-coupling (see plan of power-house); and a fly-wheel of about 1500 pounds' weight was set to be driven by the smaller motor and insure its constant speed.

The great drawback to the use of water-power for the generation of electricity has hitherto been the lack of a good water-governor, sufficiently sensitive and quick-acting to insure the vital factor of constant speed without bringing dangerous strain on the water-pipe. In fact, in the Westinghouse plant at Telluride, and in several others of which the writer is aware, the "one-man automatic regulator" had to be used; *i.e.*, a man sat with his hand on the lever of a deflecting-nozzle and his eye fixed on a voltmeter or a tachometer. The above-described governor is so great an improvement over this system that its operation has been given in detail.

The generator is a Westinghouse 120 K. W., constant-potential 12-pole machine, and its armature-shaft is attached to that of the water-wheels by a rigid coupling, insulated by a disk of oak, one inch thick and soaked in oil, while the bolts are surrounded by bushings and washers of insulating-fiber.

The initial current in the lower half of each field-coil, or the winding nearest the armature, is installed by means of a type "G," D. C. exciter. The secondary winding on the armature-spokes of the dynamo generates current when the machine is under load, which is led to a 12-bar commutator on the armature-shaft and thence to the compensating-winding which occupies the upper half of each field-bobbin.

As the load on the generator increases, more current flows through its armature-coils, and through a primary winding on the armature-spokes, thereby inducing, in the secondary winding, a heavier current, which, being led to the magnetic field as described, proportionately strengthens the same. When the generator is running without load, there being little or no current in its armature-coils, none is induced in the secondary winding, and the compensating-winding on the fields is without magnetic effect until the latter is required by work to be performed.

The potential of the generator under full load is 3530 volts, but at present it is operating with about 3390. The exciter carries a voltage of 105 to 112. A D. C. voltmeter, recently placed on the switchboard to the left of the ground-detector and above the small rheostat, is in the main circuit of the exciter, recording the tension of its current and serving as a speed-indicator when the machine is driven by the No. 3 motor. This is not necessary when driving from the wheel-shaft, as is sometimes done in winter, when pieces of ice give trouble in the small nozzle of the motor.

Plate II. shows the generator-switch-board at the power-house. The generator-current is led from the collector-rings on the ex-

treme end of the armature-shaft to the plug-sockets on the switch-board; and when the line-plugs are in these, the current follows the line to two similar sockets on the motor-switch-board. The small converter in the upper middle of the switch-board has a transforming ratio of 30 to 1. Its primary coil is attached to the main-current wires from the generator, and its secondary to the A. C. voltmeter, immediately below it. A potential of 113 volts on the voltmeter is, therefore, equivalent to 3390 volts on the dynamo-current, which is the tension under normal load. The voltmeter does not, however, read 113 volts, but records 100 to 102 volts, the difference being due to the compensator (the instrument showing in the upper left-hand corner of the switch-board and connected with the voltmeter), the object being to reduce the reading by an amount about equal to the line-loss. This loss is estimated at 15 per cent. under maximum load, and is but from 8 to 10 per cent. under normal load, as will be shown later on.

The ammeter, and just below it the aluminum fuses, all of which are in the main circuit, are shown to the left of the voltmeter in the view of the generator-switch-board.

Immediately to the left of the main-line plug-switches is the ground-detector with two lamps, one for each leg of the line, and each lamp with its converter behind it. This instrument has not as yet shown itself to be of any great usefulness, since the static effect of the line is sufficient to burn the lights with a dull red glare on making the ground-connection with the press-button, while any more serious ground on the line than one sufficient to give such a red light on the filaments would, in all probability, make itself felt quickly enough in the machines, without necessitating recourse to the detector to discover it.

The two-pole jaw-switch to the left of the switch-board is in the circuit from the exciter to the generator-fields, as are also the two fuses and the rheostat immediately below it. The small rheostat to the right of the fuse-blocks and the single pole-switch below it are in the shunt-field-circuit of the exciter. By means of these two rheostats the potential of the generator is governed and the voltmeter kept at its proper reading, the large rheostat in the exciter and generator field-circuit permitting a quick regulation over a wide range, and the shunt-rheostat a finer and closer adjustment of the voltage.

When starting up the plant, one attendant stands at the lever, controlling the admission of water to the wheels through the butterfly-valves and the other at the switch-board, handling these two rheostats

(most of the regulation is done by the large one), until the motor is in synchronism and at work, when the governor is thrown into gear, the voltage is finally adjusted, and the mechanism is then practically self-regulating for all ordinary changes of load. If, for instance, 10 of the 20 stamps are to be hung up, or any or all of the 8 continuous-pans in the mill are to be stopped, it is never necessary first to give word to the attendant at the power-house. The governor takes charge of such changes, even to the entire throwing off of the load, as before remarked. All the bearings of the generator and water-wheel shafts and of the exciter are self-oiling. The attendant has merely to keep on the *qui vive* and see that all is running smoothly. Any change in tone of the hum of the dynamo warns him at once of a change of conditions, the tone rising or falling according as the speed increases or diminishes, though ever so slightly.

To insure the all-important factor of constant speed, a tachometer, registering to 1200 revolutions, is belted to the water-wheel and dynamo-shaft. Its dial faces the water-wheels, so that the attendant at the valve-lever can readily maintain a uniform speed during the operation of "synchronizing" the motor and starting the mill, at which time the load is constantly varying.

In front of the two jaw-switches on the switch-board, there will be noticed, in the view of the latter, two steel springs and also two cords attached to the handle of the large rheostat. These cords are led around the side of the building to the attendant's place at the valve-lever, as are also those that release the catches of the springs. A pull on these cords opens the exciter main- and shunt-circuit instantly, and puts in the entire resistance-box, thereby "killing" the fields of the generator and preventing any dangerous rise in electromotive force, should the load be suddenly thrown off by a break in the wire-line, or other accident causing a sudden increase in the speed of the armature-shaft. It should be explained that this arrangement was devised by the writer, before the speed of the governor was trebled, the constant-resistance fly-wheel was put in and other changes were made, giving more sensitive and perfect control of the water-power; and it is left in place because it might still be of use in case of emergency. The power-house is lit by a small 10-light converter attached to the generator-circuit.

Wire-Line.

The length of the line is 67,760 feet, or 12.46 miles. The poles are of round tamarack timber, 21 feet long, 6 inches in diameter at

Fig. 8.

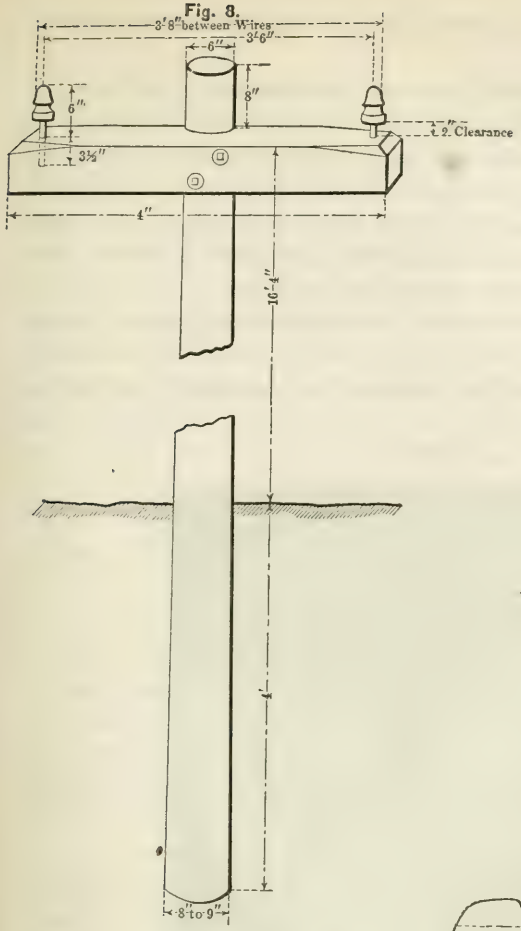
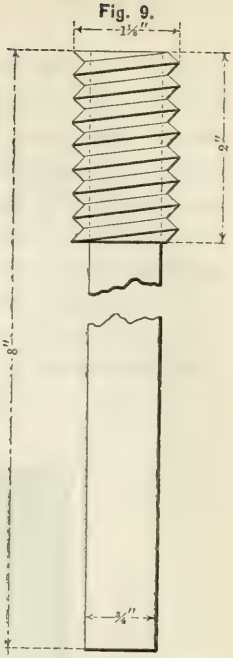
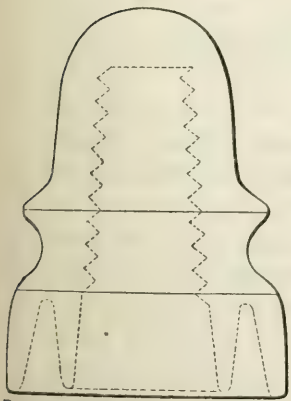


Fig. 9.



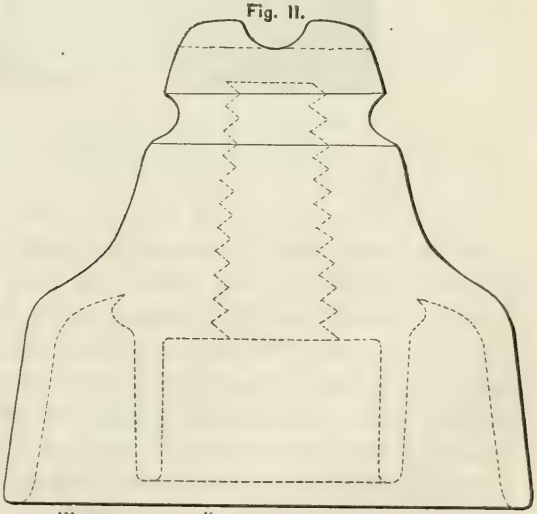
KLEIN PATENT IRON PIN.
LEAD THREAD.

Fig. 10.



DEEP GROOVE DOUBLE-PETTICOAT
GLASS INSULATOR. HALF SIZE.

Fig. 11.



WESTINGHOUSE "POMONA" DOUBLE-PETTICOAT
GLASS INSULATOR. HALF SIZE.

LINE DETAILS.

the top, set 4 feet in the ground ; poles 25 feet long being used through the town, and along the line wherever there is danger of deep snow-drifts. They are placed 100 feet apart, and fitted each with a 4- by 6-inch cross-arm, boxed into the pole, and held by one bolt and one lag-screw. The accompanying sketch, Fig. 8, shows the detail. The object of chamfering the ends of the cross-arms is to leave less room for the lodging of snow under the insulator.

The line crosses extremely rough country, not 500 yards of which is level beyond the town-limits. Most of the ground is very rocky, over 500 pounds of dynamite being used in blasting the pole-holes.

Fig. 12.

Pole No. 40; 4000 feet from Mill.
(Wire is 17 feet above ground at pole. Snow-drift 15 feet deep, March, 1893.)

The wire is of No. 1 (B. & S.) gauge, soft-drawn bare copper, and is attached to standard double-petticoat deep-groove glass insulators, Fig. 10, carried on Klein patent iron pins, Fig. 9. The distance between the wires is 3 feet 8 inches, and there are over 16.5 tons of copper in the line. The only objection found to the iron pins is their liability to be withdrawn from the cross-arm during a gale of wind, wherever there is an upward pull on the wire. To obviate this, a number of pins were drilled with a $\frac{1}{8}$-inch hole near the end, and in all such places these were used, and held firm by driving a wire nail through them.

The wire was first attached to the insulators by tie-wires of No.

10 galvanized iron wire. Later it was found advisable to insulate the line-wire at the insulators, and for this purpose ordinary sheet-rubber $\frac{1}{8}$-inch thick, such as is used for gaskets, was cut into strips 1.5 inches wide and 12 inches long. These were wound spirally about the wire and held in place by two close wrappings of Manson's tape. The whole was then well daubed with asphalt paint, and the insulated wire re-attached to the insulators by tie-wires of No. 6, weather-proof copper wire.

The line crosses a number of very steep ridges (from 300 to 800 feet in height) and on these the wire necessarily pulls heavily on the top pole, and especially on its pins and insulators. In all such places the ordinary double-petticoat insulators were replaced by the large "Pomona" insulator, Fig. 11, on which the wire is carried in a groove across the top, and its weight is therefore directly down upon, and in line with the center of, the pin.

The line has given no trouble whatsoever, and has carried the high potential of 3000 volts without a leak, even during a severe storm of ten hours' duration, the rain changing to sleet and ice toward the end; but this severe test, it must be admitted, occurred after the wire had been wrapped at the insulators as described. In fact, one of the chief objects of this insulation was to render the line proof against just such a storm as this. Snow-storms have no effect whatever.

Motor-Room.

The motor that drives the stamp-mill of the Standard Consolidated Mining Company at Bodie, is an A. C. synchronous constant-potential machine of 120 horse-power. The mill contains twenty 750-pound stamps, 4 wide-belt (6 feet) Frue Vanners, 8 continuous-process amalgamating-pans (two of which are constantly grinding), 3 settlers, 1 agitator, 1 pan and settler devoted to the amalgamation of concentrates, a bucket-elevator, a worm-gear hoist and a rock-crusher. In order to determine accurately the capacity of motor required, a number of cards were taken with the Tabor indicator from the 20- by 36-inch steam-engine that drove the mill, showing an average of 90 and a maximum of $101\frac{1}{2}$ horse-power.

The fields of the motor are self-exciting through a secondary winding on the teeth of the armature, the current being led to a 12-bar commutator similar to that on the generator. In fact the motor is almost identical with the generator, the chief difference being in the compensating-winding on the field-bobbins of the latter.

On the armature-shaft of the motor is a friction-wheel, and beyond this a clutch, which is used to set in motion the driving-pulley and the machinery of the mill. On the same bed-plate with the motor is a small 10 horse-power Tesla starting-motor, with a wooden pulley on its shaft, that is brought to bear against the friction-wheel mentioned by means of a screw and hand-wheel. This Tesla motor consists simply of field-coils and an armature; it has neither brushes, nor commutator, nor sliding contacts of any kind. The alternating current is led directly into the fields, the stationary element, the coils of which, being connected in series, produce a rotating magnetic field, in that each pole is alternately positive and negative. The starting-torque of the armature is in consequence very low, and it has to receive several rapid turns by hand before putting on the current, after which it generally runs up to normal speed (1660 revolutions) within a minute. Plate III. shows the motor in operation.

Turning our attention to the switch-board, shown in Plate IV., the two plugs in the sockets on the right of the board are the line-plugs, and the two to the left of them, in their rests, are the starting-motor plugs. When the line-plugs are in their sockets, the current is led directly to the top of the upper jaw-switch, and this switch is never closed until the machines are in synchronism. The wires from the bottom of this switch lead directly to the collector-rings on the armature-shaft of the motor.

In the upper right-hand of the board is the Wurts lightning-arrester, consisting of 22 spools, 11 on a side, separated each by a distance of $\frac{1}{32}$-inch.

Both legs of the wire-line are attached to the arrester, one on each side at the top, while the ground-wire leads from the bottom spools to a water-pipe in the earth. The spools are made of a patent non-arc-ing metal, and it is claimed that the dynamo current will therefore not follow the path through them made by a discharge of high-tension atmospheric electricity.

To the left of this instrument are two converters of the ratio of 30 to 1, filled with paraffine oil. The primary coil of the right-hand one is connected to the main line just above the plug-sockets, and that of the left-hand converter is connected to the motor-circuit, *i.e.*, the wires leading from the collector-rings on the armature-shaft to the bottom of the upper jaw-switch; it being understood that the motor acts as a generator when being driven by the starting-motor.

The secondary of the line-current converter goes to the top posts,

marked G, of the synchronizer (the marble plate with four lamps on it to the extreme left of the board), one leg being first carried through the right-hand side of the lower jaw-switch. When the line-plugs are in, therefore, and this switch is closed, the top light of the synchronizer will always be burning while the generator at the power-house is in operation.

The secondary of the motor-current converter is carried directly to the bottom posts, marked M, of the synchronizer, the two middle lamps of which are connected in series with the motor- and generator-currents.

The field-circuit of the motor is carried to the switch-board, and in it are placed the large rheostat, the left-hand ammeter, and the left-hand side of the lower jaw-switch. The closing of this switch and an adjustment of the rheostat will therefore cause the lower light on the synchronizer to burn, whenever the motor is being run as a generator, as is the case when it is being driven by the starting-motor. The aluminum fuses showing below the converters are in the main line before it reaches the jaw-switch, as is also the ammeter just below them, which instrument should, and does, record the same volume of current as its fellow in the power-house.

To start the motor requires two men, one to handle the starting-motor, and the other at the switch-board. The line-plugs are put in, which leads the main current to the top of the synchronizing-switch, and the lower jaw-switch is thrown in, which closes the field-circuit of the motor, and the secondary of the main-line or generator-converter, thereby lighting the upper lamp of the synchronizer. The armature of the starting-motor is turned a few times by hand, and the two left-hand plugs are then pushed into their sockets, leading the current from the main line to the fields of this motor.

Immediately upon doing this, the main-current ammeter records 30 amperes, and the needle stays at this reading until the starting-motor is up to speed, when it drops quickly to 18 to 20 amperes. It takes from 50 to 70 seconds for the starting-motor to reach full speed, after which its friction-wheel is brought to bear against that of the main motor, and the armature of the latter begins to revolve. During this time the synchronizing-switch (the upper jaw-switch) is open, and all the resistance-coils of the rheostat are left in the field-circuit, in order that the armature may more easily be brought up to speed, by preventing the flow of current in the same.

As soon as the armature is above speed, about two-thirds of the rheostat is thrown out, permitting 40 to 50 amperes of current to

dow and the lower lamp of the synchronizer to burn. The pushing on to its button of the little switch on the bottom of the synchronizer, now connects the two central lamps in series with the motor- and the generator-currents, and they begin to flash in accordance with the phases, and therefore the speeds, of the two machines. As the speed of the motor approaches that of the generator, the wave-phases come nearer coincidence, and those lamps brighten and darken almost simultaneously.

The attendant stands with one hand on the rheostat and the other on the open jaw-switch, watching these waves of light intently, and just as the two lamps darken in unison, he throws in the switch and pulls one of the starting-motor plugs. The lamps only remain "out" for a second or less, while the speeds are together, and then flash up brightly again as the motor speed drops off: there is therefore but a fraction of a second during which the jaw-switch should be closed, though this time can be lengthened slightly by a proper handling of the starting-motor.

If this switch has been thrown in at the right moment, the series lights remain "out," while the top and bottom, or "pilot" lights, burn brightly, and so continue all the while the machines are in operation.

If the switch is thrown in a second or so too soon, the main-current ammeter will fly up to 40 or 45 amperes, and quickly drop down to less than 10 as the motor-speed decreases, and it falls into step with the generator; while the series-lamps will remain dark, and the pilot-lamps burn as usual. On the other hand, if it is closed several seconds too soon, or a fraction of a second too late, it is impossible for the machines to get into synchronism. In such event all the lights on the synchronizer go out at once, and a heavy flow of current sets in, the main ammeter showing 45 amperes, which is as high as it can record. By the extinction of the lights the attendant sees at once that he has missed the synchronizing-point, and immediately pulls the main-line plug, opens the jaw-switch, and starts over again. The second trial, however, will not consume as much time as the first, since the starting-motor is still revolving at a high speed, and more quickly comes up to its normal rate, while the motor-armature is also running yet at several hundred revolutions per minute.

It will be noticed that in order to break the circuit and stop the motor it is necessary to pull the line-plug, in doing which a brilliant arc, sometimes two feet in length, if 25 amperes are flowing, follows

out from the socket to the plug-tip. Any attempt to open the jaw-switch while the line-plugs are in, would doubtless result in the death of whoever tried it, since the distance is too short in which to break the arc, and the current would likely follow down the arm in spite of one's standing on an insulated floor.* These floors are used around both generator and motor, and in front of both switch-boards.†

The entire operation of starting up the motor from a state of rest occupies from three to five minutes, and when once in synchronism, the clutch can be thrown in and the mill-shafting brought to normal speed in from one to two minutes more, after which the load may be thrown on as fast as desired. The clutch is always thrown in slowly in order to prevent too heavy a flow of current, and consequent sparking of the commutator-brushes.

By means of a single counter-shaft, fitted with self-oiling boxes, the high speed of the motor (600 revolutions) is reduced to the necessary 80 revolutions of the battery line-shaft; the reductions being 2 feet to 8 feet, and 3 feet to 8 feet. Light steel-rim balanced pulleys are used, and an endless 16-inch double leather belt runs from the motor to the first 8-foot pulley. The speed of this belt is 5400 feet per minute, and it is kept tight by levers which, acting through screws, move the entire motor and its bed-plate along 4 grooved cast-iron slides.

The motor is separated from the underlying brick foundation by means of 8- by 10-inch timbers, which are bolted to the latter and covered by three layers of 1-inch boards; and to this wood foundation the slides referred to are fastened by lag-screws that pass through the boards into the timbers. The generator is insulated from the I-beams that carry both it and the water-wheels, by timbers 4½ inches thick, to which it is likewise secured by lag-screws.

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* Several years ago, when high-tension currents were not so commonly in use as now, on a 2000-volt circuit installed in France, large jaw-switches were used set high up on the switch-board, so that the attendant had to spring upwards to reach them, and was therefore always in the air when either breaking or closing the circuit.

† The acoustic synchronizer described on page 106 of the *Engineering and Mining Journal* February 5 1894, is probably a more perfect apparatus than the phase-lamp device. It is claimed for it that "so sensitive and distinct is the indication of approach of synchronism, that it is possible to throw two machines together with hardly a quiver upon the ammeter at normal travel." This synchronizer is in use on the 3-phase machines of the Eastlands plant already mentioned.

The mill and offices of the company are lit by 100-volt incandescent lamps, taking current from a large 100-light converter, ratio 30 to 1, which is attached to the main line in the motor-room, before it reaches the switch-board. The light is very satisfactory, even to read or write by, although at times the lamps flicker slightly, due to small changes of load. This variation of intensity is of course unavoidable where the lighting-current is taken direct from a power-circuit; but, in the present case at least, is not sufficiently noticeable to cause inconvenience.

During normal operation of the plant, the field-ammeter of the motor is, by means of the rheostat, kept steadily at 52 amperes, while with full load on the mill the main-current ammeter registers from 23 to 25 amperes. The needle oscillates over a range of 4 to 6 amperes, showing considerable variation of load, due undoubtedly to slipping of the belts, unequal resistance of the grinding-pans, rock-crusher, etc., so that it is difficult to read this ammeter closely, either at the generator or at the motor.

The average amperes of current can be very closely approximated, however, as was done in the tabulated readings given below, which were taken with the object of determining the line-loss, and with the aid of Mr. H. M. Reed, the efficient engineer of the Westinghouse Electric and Manufacturing Company, who installed and first operated this apparatus.

The readings were taken simultaneously at power-house and motor-room, by means of the telephone, and the figures given are the averages of five or six consecutive observations. There being no voltmeter on the motor-switch-board, a Weston portable voltmeter was used, the wires being attached to the lower posts on the synchronizer.

The table is merely a rough approximation of the efficiency of the transmission, there being no instruments at hand for close work, such as the measurement of the wheel-shaft energy, or the determination of the true efficiency of the motor and generator. This efficiency is taken in the table at 90 per cent., the Westinghouse Company claiming as high as 96 per cent. for its machines under full load. This load it is impossible to give them at present, as full mill-load represents but 75 to 80 per cent. of their capacity.

It will be noticed that, as the dynamo and motor approach their rated capacity, the efficiency of the transmission increases. The line-loss is undoubtedly light for even so moderate a current; but the fact that the tests were made in the winter, when the temperature

Approximate Efficiency of the Transmission.

Date.	Generator.			Mech. Horse-Power given out by Generator.	Horse-power given out by water-wheels, allowing 90 per cent. efficiency for Generator and adding 5 H. P. for Exciter.	Motor.			Mech. Horse-Power delivered to Motor.	Line-Loss, per cent.	Mech. Horse-Power given out by Motor, allowing 90 per cent. efficiency.	Approximate per cent. of full Mill-Load driven at time of Test.	Approximate per cent. of Water-Power obtained at Motor-Pulley.
	Volts.					Volts.							
	Voltmeter Reading.	Equivalent to Volts.	Amperes.			Voltmeter Reading.	Equivalent to Volts.	Amperes.					
December 22d.....	100	3390	20	90.9	106.0	103	3090	20	82.8	8.9	74.5	82	70.3
December 29th.....	100	3390	21	95.4	111.0	$103\frac{1}{3}$	3100	21	87.3	8.6	78.5	85	70.7
January 16th.....	100	3390	23	104.5	121.1	104	3120	23	96.2	8.0	86.6	95	71.5
February 1st.....	101	3420	25	114.6	132.3	105	3150	25	105.5	8.0	95.0	105	71.8

Note.—During the above tests the exciter was being driven from the wheel-shaft, instead of by its separate water-motor; hence the allowance of 5 horse-power.

along the line was below freezing, may have some bearing upon this point.

From the foregoing description, it will be seen that the operation of starting the motor is one requiring considerable practice. The proper setting of the brushes on the commutator of the motor-armature is also a knack acquired only by experience; and for a a while considerable trouble was caused by undue sparking at these brushes. Experience on the part of the attendants has entirely overcome this; but it has been found necessary to use two commutators, keeping one always turned and polished ready for use, and changing them usually after 25 to 30 days of steady operation.

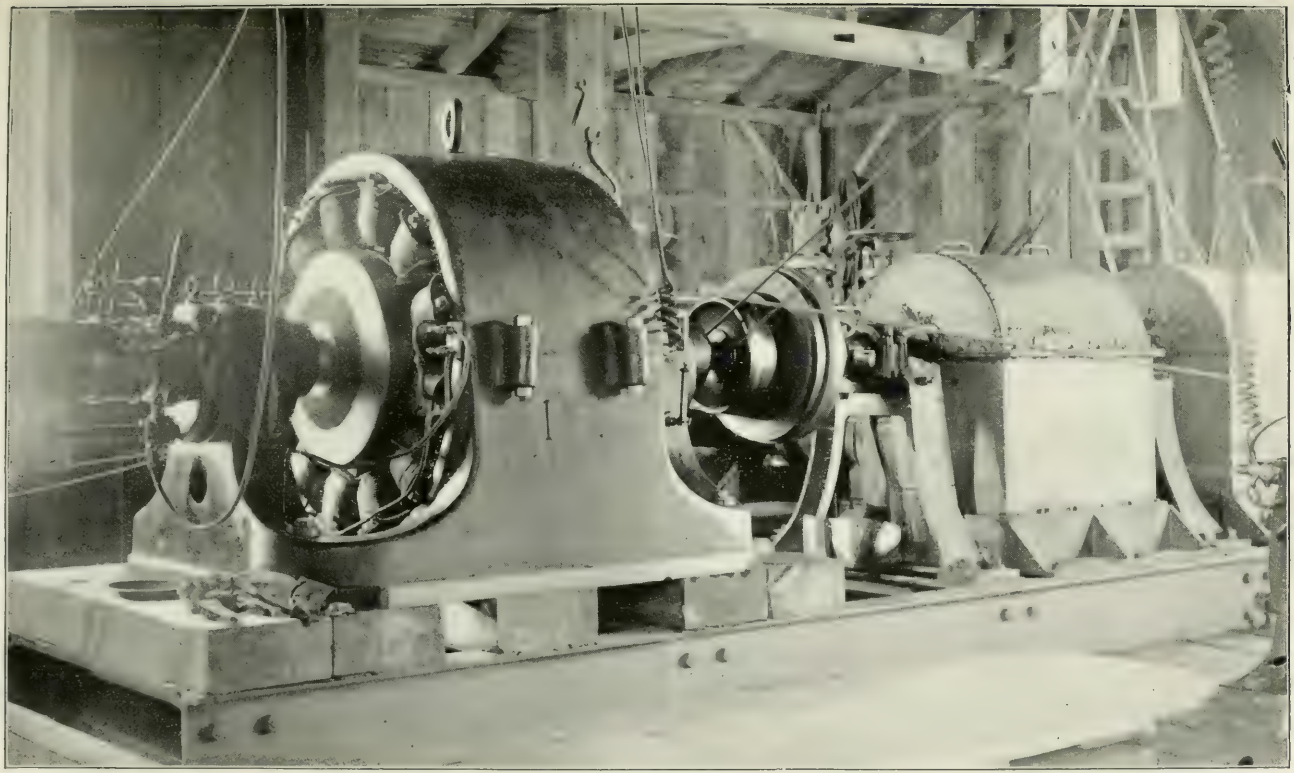
In order to stop the motor, the load is thrown off by means of the clutch, and the line-plug is then pulled. Should the plug be pulled without first throwing off the load, a momentary rise in electro-motive force may follow, sufficient to damage an armature-coil. This has happened once in our experience; and the very high potential was vividly shown by the discharge through the lightning arresters at both ends of the line.

The dependence of the motor-speed upon the alternations of the generator is very prettily shown, when, without pulling the line-plugs, the machines are stopped by shutting off the water on the wheels. The motor then slows down in exact accordance with the generator, and is at rest within half a minute or so; whereas, when the plug is pulled in the usual way, the motor-armature will revolve for several minutes from its own momentum, before coming to a stop.

The first trials of this plant last summer developed electrical defects resulting in accidents more or less serious to both machines; and Messrs. Reed and Winslow, Westinghouse engineers, deserve much credit for the manner in which they met and conquered these difficulties. There was no trouble with the wire-line,—it has carried the 3000-volt current through all kinds of weather—but the current succeeded in escaping at the machines themselves, through A. C. brush-holders, and armature-coils, and even at the synchronizing jaw-switch. In almost every instance, the remedy was heavier insulation at the point of escape, resulting almost invariably in a cure of the evil.

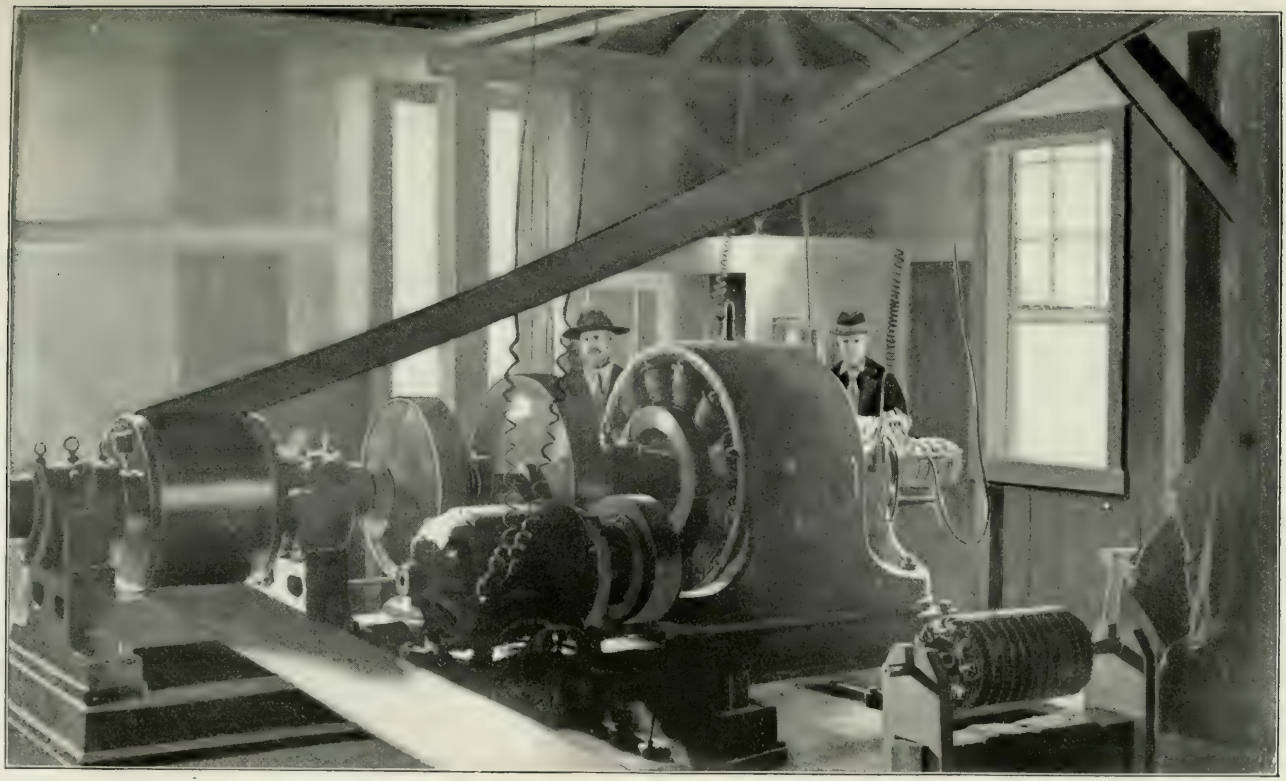
It is probably more advisable on machines of this size to use a potential not exceeding 2000 volts, or, if the length of the transmission necessitates a higher tension in order to avoid excessive cost of copper,* then to use step-up and step-down transformers.

* For amounts and cost of copper per mile and per horse-power transmitted, see



Generator and Water-Wheels in Operation.

(Exciter in foreground, driven from Wheel-shaft.)



Motor in Operation.

Motor-Switchboard. Clutch-wheel in left foreground. Motor in Operation.

This plant has accomplished several unbroken runs, day and night, one of thirteen days' and another of twenty days' duration, but latterly it has been operated more intermittently, on account of the mill being run upon only half-time. During December, 1893, the plant was started twenty-three times in twenty-one days, and in January eleven times in as many days (in accordance with the requirements of the milling work), these daily startings being an excellent test on both the starting-motor and machines, as at such times the differences in potential, and consequently the strain on the insulation, are likely to be a maximum.

The entire cost of this plant does not exceed $38,000, while its operation during the month of October alone effected a saving of $2100, equivalent to $1.46 per ton of ore crushed, and reducing the total milling-cost to $2.32 per ton; a fairly low figure for a high-priced camp (wages $4 per day), such as Bodie.

At present the plant is operating most smoothly, and is successfully demonstrating the effectiveness and simplicity of the single-phase synchronous system for such work and distances, while the daily saving over the use of steam, on twelve-hour runs, is from $35 to $40.

The writer takes pleasure in acknowledging the aid of his assistant, Mr. R. C. Turner, E.M., in the preparation of the accompanying drawings.

Postscript.

Since the presentation of the foregoing paper in February, 1894, four months ago, this plant has continued in successful and satisfactory operation. An acoustic synchronizer has been added to the phase-lamp device on the motor switch-board, and has proved a great aid in the operation of synchronizing the machines. In fact, it is an extremely rare occurrence for the motor-attendant to miss the moment of synchronism.

A few minor changes have been made, such as the replacing of the oak insulation, in the coupling of the generator and water-wheel shafts, by a hard-rubber disk, projecting about an inch beyond the flanges of the coupling. This was found necessary on account of the splitting of the oak disk, giving *surface* for the passage, through the coupling to ground, of high-tension electricity, with which, during

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paper by Mr. Hasson before cited; also pamphlet on "Long Distance Transmission of Power," by L. B. Stillwell, E.E., issued by the Westinghouse Electric and Manufacturing Company.

wind-storms, the wire-line is sometimes charged from the atmosphere.

By the use of lower-voltage lamps in the ground-detector on the generator switch-board, and a change in the ground-wire attachment, this instrument has been greatly improved; and, while no "grounds" have yet occurred on the line, there is no doubt that the detector is now in shape to indicate such with accuracy. Without the ground-connection made, the lamps now show a red light on the filaments, due to the difference in potential of the two sides of the line; and, should a "ground" occur on either leg, the corresponding lamp immediately burns at full candle-power, while the other lamp proportionately diminishes.

The only trouble now experienced with this plant, comes from an extraneous source, common, in a greater or less degree, to all electrical plants throughout the world, namely, occasional incursions of lightning during thunder-storms; or from another, more local cause, already alluded to, namely, discharges of static electricity, due to a gradual charging of the line from a highly charged atmosphere, during wind-storms.

These have several times caused the burning-out of armature-coils; but this matter is not as serious as it may seem, since but a couple of hours are required to repair such damage. To put in a new coil, the top-half of the field of the machine, weighing about two tons, is swung off by means of differential blocks and an overhead trolley; the burnt coil is cut out with a hack-saw; the new coil is slipped over the tooth and squeezed into place by means of specially-made geared clamps; the connections are soldered, taped, and painted; and the top-field is then replaced in position.

A proper arrangement of lightning-arresters and impeding or "choke" coils, now being introduced, bids fair to protect the machines entirely against all danger from lightning; while the grounding of the wire-line at each end through inductive resistance, sufficiently high to prevent any appreciable loss of current, will prevent damage from these static charges, as they will be leaked to earth as fast as accumulated, and without the occurrence of disruptive discharges.

SOLIDS FALLING IN A MEDIUM—II.

A Critical Examination of the Theory and Method Applied by Rittinger, and the Resulting Formulas.

BY F. M. F. CAZIN, HOBOKEN, N. J.

(Bridgeport Meeting, October, 1894.)

In my first paper, relating in general to the movement of solids in a medium, I stated a newly-discovered natural law, and explained its application to mechanical ore-concentration. This law, as applied primarily to the motion of ships in water, had been announced, demonstrated and defended by me in the *Journal of the Franklin Institute* for March, April and May, 1893, and has not been controverted since then. I therefore contented myself in my first paper with the simple statement of it; and in order to avoid unnecessary repetition, I refer the readers of this paper to that statement (*ante*, p. 81) and to the earlier publication already mentioned.*

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* I may be permitted to recommend the following simple ocular demonstration of this newly-expressed law of the displacement of a medium, in inverse direction and in measurable quantity, by the movement of a solid therein. The illustration primarily applies to a ship's motion.

Cut in the face of a planed board, representing the medium, water, a triangular groove to represent the travel of the submerged part of a ship. Fit into this groove a removable triangular rod, restoring the surface of the board. Let the rod be cut into equal fractions of its length, one of these pieces being colored to distinguish it from the rest, and let this colored piece, representing the immersed part of the ship, be placed in one end of the groove, the remaining pieces filling the rest of the groove. The colored piece may now be shifted into the place of the next, interchanging position with it, thus representing the travel of the ship for a distance equal to its own length, and the restoration of the surface of the medium behind it by displacement and replacement. Such interchange between the piece representing the ship's bottom and the successive fractions of the medium confronting it in its progress may be repeated until the ship-piece has reached the other end of the groove.

Instead of a groove and rod of uniform transverse section, a series of adjoining indentions of uniform ship-shape may be made and filled with corresponding pieces so as to restore the surface, one of these pieces representing the ship's bottom, and the others fractions of the medium, as before, and the ship being made to interchange places with these fractions in its road of travel. On either plan, to represent the case truly, the fractions must all have the same volume (weight) and the same length (d), and when pieces of a triangular rod are used, the transverse section of

From the law thus stated, which is equally applicable to the motion of solids in any direction in a resisting medium, I deduced for the ultimate maximal velocity of a solid of any form, falling in water or any other medium, the formula,

$$C = \sqrt{d \cdot \delta \cdot 2g \cdot \frac{\delta - 1}{\delta - 1}}$$

in which C is the said velocity per second, expressed (like d and g) in meters; d, the length of the solid, measured in the direction of relative motion; g, the acceleration due to gravity per second; and δ the specific gravity of the solid, that of the medium being taken as the unit.

P. von Rittinger had already presented a different formula, for the case of a spherical solid falling in water, namely,

$$c = \sqrt{\frac{2\gamma}{3\, a_3} \cdot d \cdot (\delta - 1)}$$

in which c is the velocity; γ the metric ton of 1000 kilos; d, the diameter of the sphere (equivalent, therefore, for this case, to my d, the length of the solid measured in the direction of relative motion); δ the specific gravity of the solid, as in my formula, and a_3 the resistance in kilogrammes offered by the medium per square meter of the maximal cross-section of the sphere.

It is the purpose of the present paper to determine the difference between these two formulas, and to point out their origin and nature.

Criticism of Rittinger, heretofore directed to alleged defects in his results, has not offered any substitute for his valuable pioneer discussion or for his various formulas. Thus, to take as an instance the earliest, severest, and perhaps ablest of his critics, Julius von Sparre,* a royal Prussian mining counsellor, pointed out, that Rittinger's theoretical development of the law of motion in submersion was confessedly dependent upon experimental coefficients for any value it might claim in the art of ore-concentration; and that it omitted altogether to take into consideration the counter-pressure

the rod should represent B ÷ d volume divided by length in the direction of travel) of the immersed part of the ship.

A thorough mathematical digestion of the facts thus illustrated will render proof of the law as stated by me, so far as real final displacement is concerned (see paragraph 11 of my first paper).

* *Zur Theorie der Separation oder kritische Bemerkungen zu von Rittinger's Lehrbuch der Aufbereitungskunde*, Oberhausen, 1869.

of the medium on the rear of the solid, which amounts, according to Weisbach,* for a plane at right angles to the line of motion, to about one-third the total pressure in front. Von Sparre observed also that Rittinger's determination of a_3 (the pressure per sq. m. of maximal section against a sphere) as 0.5 times a, the similar pressure against a plane, conflicts with the conclusion of Weisbach, who gives 0.6, and that of Eytelwein, who gives 0.7886 as the coefficient for the case of a sphere. Moreover, he controverted Rittinger's assumption, that in practical ore-dressing the law of incipient fall need not be considered as important in comparison with the ultimate uniform maximal velocity, and he claimed an importance for the former which he omitted to establish by adequate proof. He also criticized the experimental methods of Rittinger, and doubted their value as supports of doubtful theoretical results. Without going further into the details of his argument, it is sufficient to say that he practically accepted the general theoretical method of Rittinger, objecting only to details, and offering nothing better as a substitute. In fact, he frankly declared (*op. cit.*, p. 9), that as the complicated nature of the equation for incipient fall as evolved and proposed by him (without changing any of Rittinger's fundamental assumptions), admitted of no algebraical solution, it appeared to him impossible to find by direct calculation a value correctly expressing the relation between the diameters of spheres of different density, falling equal distances within a stated period.

A similar statement may be made concerning other critics of Rittinger. They have offered no opposition to his fundamental assumption that the resistance to a solid's motion in submersion should be measured in analogy with the measurement of the mechanical force of a water-current, striking a solid face, or on the basis of the so-called percussion or water-jet theory. The present writer has been so far alone in the claim, that the percussion-theory is not applicable, not necessary, and not adequate for solving all the questions relating to resistance to submerged motion, but that they may be solved correctly, with greater facility and with a general application to forms of solids and kinds of medium by substituting for the percussion-theory the fact of the displacement of the medium in inverse direction by the moving solid, and the logical consequences of that fact.

Rittinger starts† with the expression $W = f \frac{v^2}{2g} \gamma$ for the pres-

* *Lehrbuch der Ingenieur-und Maschinen-Mechanik*, ed. 1850, vol. i., 429, 430.

† *Lehrbuch der Aufbereitungskunde*, Berlin, 1867, p. 166. "Ein mit der Geschwindigkeit v vertical aufsteigender Wasserstrom übt gegen eine ebene und horizontale

sure (W) exerted by a water-current rising vertically with the velocity of v meters per second against a horizontal plane, f square meters in area. In this formula, g is the acceleration of gravity, and γ is the weight of a cubic meter of water, or 1000 kilogrammes. Substituting this value of γ, and $g = 9.809$ meters, the expression becomes

$$W = f \frac{v^2}{2g} \gamma = 50.97 f v^2, \text{ or say, } 51 f v^2.$$

Assuming $f = 1$, the pressure against one square meter becomes $51 v^2$, and if $v = 1$, the pressure becomes 51 kilogrammes. This value Rittinger designates as a, which he defines (p. 167), as the pressure exerted perpendicularly against a plane of one square meter area by a current having the velocity of one meter per second.

The introduction of this symbol, applicable only to a special case, involving a particular area, a particular velocity, and a particular value of g, has rather confused than simplified Rittinger's subsequent demonstrations. But apart from this consideration, the essential question is, whether the resistance of the medium can be properly measured in this way. It is practically impossible, that a plane without a third dimension, which would change the plane into a volume, should oppose the movement of a water-current, because a plane opposing a water-current is in nature supplemented automatically by a portion of the medium, which adapts the form of the matter moving in the medium to the molecular division-lines (adhesion-lines, wave-lines) of the medium—such portion of the medium participating in the relative movement of the solid and not in that of the medium at large. Therefore, a correct measurement of resistance to immersed motion rests on the assumption, either of the solid being of a form closely adapted to the division- or adhesion-lines of the medium, or of the solid being measured in its length in the direction of relative motion, inclusive of the part of the medium that automatically moves with it.

In modifying his values for f on account of form of face, Rittinger indirectly accounts for volume as fundamental to the measurement of resistance to motion in a medium: but his method of accounting therefor is an obstacle to correct results, because this method leaves the actual nature of the occurrence unconsidered.

Platte von geringer Oberfläche ($= f$ Quadratmetern) einen Druck aus, welcher dem Gewicht einer Wassersäule gleichkommt, deren Grundfläche $= f$ und deren Höhe $= \frac{v^2}{2g}$, d. i. der Geschwindigkeit-höhe gleich ist."

Proceeding to modify his expression for resistance, by calculations upon the effect of the form of the front of the solid, to which the water-current is opposed, Rittinger obtains $a_1 = \frac{a}{2} = 25.5$ k., for a wedge with a solid angle in front of 90° and a base-plane of one square meter; $a_2 = \frac{a}{2} = 25.5$ k., for a cone, the height of which is equal to the radius of the base, and the circular area of the base of which is one square meter; and $a_3 = \frac{a}{2} = 25.5$ k., for a hemisphere or sphere, with maximal transverse section of one square meter. His formula for the pressure of an ascending current against a sphere is, in consequence,

$$W = a_3 f v^2 \delta \text{ kilogrammes},$$

in which a_3 represents the resistance in kilogrammes experienced on each square meter of transverse sectional area by a spherical solid moving in water with the velocity of one meter per second. If the total transverse sectional area be one square meter, the diameter of the sphere is evidently not $= 1$ but is $= \frac{2}{\sqrt{\pi}}$, a fact which it will be opportune to consider, when the values obtained under the two different methods shall be compared; such a comparison offering also the desired opportunity to compare the methods and elucidate their relative merits.

On p. 177 of his book, Rittinger developes as the formula for the velocity of uniform ultimate fall of a sphere having the diameter d,

$$v = \sqrt{\frac{2 \gamma d (\delta - \triangle)}{3 a_3 \triangle}}$$

or, $\triangle$ being the density of the medium, the formula becomes for water ($\triangle = 1$) as the medium:

$$v = \sqrt{\frac{2 \gamma d (\delta - 1)}{3 a_3}}$$

On p. 193 he develops as the formula for the velocity of an ascending current keeping a spherical solid in suspension:

$$c = \sqrt{\frac{2 \gamma d (\delta - 1)}{3 a_3}},$$

and since $r = c$, he concludes that to hold a body in suspension, the water must have an upward velocity equal to the ultimate uniform velocity with which the body would, after a certain period, sink in still water.

It will be seen that in this expression, by the use of a_3, a particular value of g is assumed. But g, the acceleration of gravity, varies with the altitude and latitude, and the formula should provide for this variation. Going back to the original development, we find that $a = \frac{\gamma}{2g}$ and $a_3 = \frac{a}{2} = \frac{\gamma}{4g}$; and substituting this value, we have

$$c = \sqrt{\frac{2\gamma d\,(\delta - 1)}{3\,a_3}} = \sqrt{\frac{8}{3} g d\,(\delta - 1)} = \sqrt{\frac{8}{3} g \,.\, d \,.\, \delta \,.\, \frac{\delta - 1}{\delta}}.$$

In this form the expression may be compared with my own for the ultimate maximal velocity of a sphere falling in water,

$$C = \sqrt{2gd\delta\left(\frac{\delta - 1}{\delta + 1}\right)},$$

as it results from the displacement law; and we then find

$$c : C \text{ as } \sqrt{\frac{8}{3} \,.\, g \,.\, d \,.\, \delta \,.\, \frac{\delta - 1}{\delta}} : \sqrt{2g \,.\, d. \,\delta \,.\, \frac{\delta - 1}{\delta - 1}},$$

or

$$c : C \text{ as } \sqrt{\frac{4}{3} \cdot \frac{\delta - 1}{\delta}} : \sqrt{\frac{\delta - 1}{\delta + 1}}.^{*}$$

* This relation between c and C may also be expressed as

$$c : C = 1 : \sqrt{\frac{3}{4}} \times \sqrt{\frac{\delta + 1}{\delta}} = 1 : 0.866 \sqrt{\frac{\delta + 1}{\delta}},$$

whereby it is seen that Rittinger's value for

$$c = \sqrt{\frac{2gd\,(\delta - 1)}{3a_3}} = 5.11 \sqrt{d(\delta - 1)}$$

is in all cases in excess of the value

$$C = \sqrt{2g} \times \sqrt{d\delta \frac{\delta - 1}{\delta + 1}} = 4.43 \sqrt{d\delta \frac{\delta - 1}{\delta + 1}}.$$

The value c, as quoted, relates exclusively to solids of a spherical form, and Rittinger, recognizing the practical insufficiency of this theoretical value, introduced another experimental value relating to solids of irregular form, in his equation (91):

$$v = 2.44 \sqrt{D\,(\delta - 1)}.$$

Rittinger's reasoning in justification of this experimental and averaging formula

The origin of the coefficient $\frac{\delta - 1}{\delta + 1}$ is clearly shown in my first paper. It is a modification of the acceleration, g (*in vacuo*), consequent to the resistance by a medium of $\frac{1}{\delta}$ density to the fall of the solid. And, as I shall demonstrate hereafter the origin of $\frac{4}{3}$ in Rittinger's value, c, to be in a claimed reduction of resistance by form of

is arbitrary and faulty on its face, and, therefore, need not be refuted in detail. D, which has taken the place of d in this equation, is no longer the diameter of a sphere, but of a circular perforation through which the solid has passed. Such solid, being of an irregular form, is of less volume than the sphere of the same diameter with the circular perforation, because of all particles, any dimension of which is not in excess of d, the sphere is the greatest in volume, and all particles having any one of their dimensions in excess of d will slide on the sieve face with their longest dimension parallel with the dimension d of the perforation, and, therefore, will not pass through the same, but to the end of the trommel or other sizing contrivance, as which in no case a sorter by equal falling-velocity can be considered, such sorting of mixed material never resulting in a grade of equal or similar size in mixed particles. As ultimate velocity of fall in submersion depends, under the displacement-law, on d as length in the direction of fall, and not on d as measuring transverse section, it is in conflict with facts, to assume reduced final velocity of fall as the result of decrease in volume, when the value, d, as length in the direction of fall, remains the same, though D be substituted for d.

The different interpretations of d and D make it worth noticing that with d, in the value C, as the length of the solid of any form, but measured in the direction of relative motion (the greatest length being automatically turned into such direction), the former relates neither exclusively to the sphere nor exclusively to solids of irregular form, but to all solids of any form, and it appears that one of Rittinger's values—the theoretical one, namely—is in excess of the value C, while the other, or experimental value, is a fraction only of C.

Applying both to the cases often used in illustrating actual results—namely, of quartz and galena, with δ as 2.6 and 7.4—we find by

$$v : C = 1 \times \frac{2.44}{5.11} : 0.866 \sqrt{\frac{\delta}{\delta + 1}} = 1 : 1.8 \sqrt{\frac{\delta}{\delta + 1}},$$

the concrete cases for Rittinger's two equations, as

$$c : C = \begin{cases} 1 : 0.736 \\ 1 : 0.700 \end{cases}$$

as relating to the sphere, and

$$v : C = \begin{cases} 1 : 1.530 \\ 1 : 1.700 \end{cases}$$

as relating to the smaller volumes; the ratio between c and v being as 1 : 0.4775.

Compare R. H. Richards and A. E. Woodward: "The Velocity of Bodies of Different Specific Gravity Falling in Water."—*Trans.*, xviii., 644.

solid, his $\frac{\delta - 1}{\delta}$ might be compared with $\frac{\delta - 1}{\delta + 1}$ as the modification of g, which Attwood ascertained by experiment. Such comparison would rest on the definite assumption of the displacement theory as preferable to the percussion theory. And it is therefore but proper that these two so essentially different theories, both relating to resistance to submerged motion, be further compared.

Weisbach, speaking of the theory of percussion as measuring resistance to submerged motion, says that the pressures upon the rear surface of the solid are opposed to those on the front, and the resultant percussion or resistance of the water is equal to the difference between the two, and adds:

"Although we cannot state *a priori* the amount of these pressures, we may nevertheless assume, on account of the great similarity of the conditions with the percussion of isolated jets, that at least the general law for percussion by unlimited water does not vary from the law for the percussion of isolated jets."*

These words, in the light of later discovery, bear the character of an intuition, because, although the percussion of isolated jets does not present the conditions of submerged motion, and although the whole percussion-theory must be deemed an unsound basis for measuring the effect of submerged motion, and although the theory of continuous displacement applied to the measurement of resistance to submerged motion is so thoroughly and entirely at variance with this percussion-theory, yet the formulas resulting from either are not only very similar, but virtually identical, while there remain differences only in the interpretation of some of the values constituting these formulas.

Under the metrical system the work done by the water-jet striking squarely an opposing face is as

$$P = f \cdot v \frac{v^2}{2g} \text{ kilogramme-meters,}$$

f being the transverse uniform area of the nozzle in one-thousandths of a square meter, when the result is kilogramme-meters.

Under the same system the resistance to submerged motion, as evolved under the law of continuous displacement, is

$$R = f \cdot v \frac{v^2}{2g},$$

* *Op. cit.*, vol. i., p. 640, § 429 (or edition of 1875, pp. 1179, 1180).

f signifying the transverse uniform or average section of solid $= \frac{B}{d}$,* with the same unit as above stated.

Yet neither Weisbach nor Rittinger perceived either the unsoundness of their fundamental issue or the true facts supporting their results in part.

While Rittinger compares resistance to submerged motion with the force with which a solid strikes, a force being the product of mass by distance, and the action being instantaneous, this resistance is, in fact, not force only, but continuous work or applied work, the velocity, v, per second being, in the case of the jet as well as of resistance to submerged motion, in fact, an element of the mass; that is to say, the amount of water moving inversely in one case, and coming into contact with the opposing face in the other case per second, is $f\,v$, and its weight $f\,v\,\gamma$. Consequently, the work performed by the water per second must be $f\,v\,\frac{v^2}{2g}\,\gamma$. This is as against Rittinger's

$$a = f\,\frac{v^2}{2g}\gamma,$$

(an expression of mere pressure), which must be multiplied by v, the path traversed per second.

But this employment of work instead of pressure, as the starting-point of the calculation, does not explain the difference between the resulting formulas of the two methods. For Rittinger (and Weisbach) omits v also on the other side of the equation; that is, in the measurement of the reaction of the solid. The true source of differing results is found in the two-fold modifications introduced by Rittinger, of which the one relates to a, which is reduced by reason of the form of the front of the solid, and the other relates to the transverse section of the solid, which, with Rittinger, is the maximal cross-section at right-angles to the line of motion. He makes a_3 (the pressure against this transverse section, if the solid be a sphere) to be only one-half a (the pressure, if the opposing surface, f, be a plane).

* Only by the author's introduction into theoretical mechanics of this new value, $\frac{B}{d}$, that is, the transverse average section, has it become possible to formulate the natural laws expressed in the first paper.

The purpose of expressing $\frac{B}{d}\,v$ or $f\,v$ in kilogrammes, is achieved either by expressing $\frac{B}{d}$ or f in units of 0.001 square meter, or by making use of the coefficient $\gamma = 1000$, as practiced by Rittinger.

This is equivalent to reducing the area, f, by one-half, for the case of a sphere. Stated in proportion to the square of the diameter of the sphere, the pressure, measured by transverse maximal section, would be equivalent to that against a plane:

$$f = \frac{1}{2} \cdot \frac{d^2\pi}{4} = 0.3927\, d^2.$$

On the other hand, the value of f_1, according to the new law of displacement, is determined as transverse average section by dividing the volume of the solid by its length measured in the direction of motion. That length being, of course, for a sphere, equal to the diameter, we have f_1, expressed for a sphere, in proportion to the square of the diameter as follows:

$$f_1 = \frac{d^3 \times \frac{\pi}{6}}{d} = \frac{\pi}{6} d^2 = 0.5236\, d^2,$$

showing f_1 to be, under the newer theory, as the ratio of the volume to the cube of d, taken as a coefficient of d^2.

The proportion between these two values for f is,

$$f : f_1 = \frac{1}{2} \cdot \frac{d^2\pi}{4} : \frac{\pi}{6} d^2 = 3 : 4 = 1 : \frac{4}{3}.$$

The coefficient $\frac{4}{3}$ appearing under the radical in our former proportion,

$$c : C = \sqrt{\frac{4}{3} \cdot \frac{\delta - 1}{\delta}} : \sqrt{\frac{\delta - 1}{\delta + 1}},$$

has thus been traced to its origin.

If the value f_1 is determined for a sphere that has, as originally assumed by Rittinger, a maximal section of 1 sq. m., and therefore a diameter

$$d = \sqrt{\frac{4}{\pi}},$$

and a volume

$$B = \left(\sqrt{\frac{4}{\pi}}\right)^3 \cdot \frac{\pi}{6} = \frac{2}{3}\sqrt{\frac{4}{\pi}},$$

then we find

$$f_1 = \frac{B}{d} = \frac{2}{3} \cdot \frac{\sqrt{\frac{4}{\pi}}}{\sqrt{\frac{4}{\pi}}} = \frac{2}{3} \text{ sq. m.} = 0.66 \ldots d^2,$$

0.66 being again the ratio of the volume of the sphere under consideration to the cube of d.

It is noteworthy that this corresponds to a value for a_3 of 0.666 (instead of 0.5 a, which is Rittinger's value), and it will be remembered that Weisbach has given 0.6, and Eytelwein 0.7886, as the experimental coefficient for the case of a sphere; so that it is evident that the displacement theory more closely agrees with the determinations of the best investigators than does the calculation of Rittinger. In fact, the correspondence would be still closer than the above figures indicate, for my theory requires that the volume to be divided by d in determining f shall be the volume of the solid, plus an allowance for the portions of the medium which automatically take part in the motion of the solid, as above stated. (Compare *Journal of the Franklin Institute*, 1893, p. 206.)

I may be permitted to direct attention, also, to the fact that, from the establishing of the effects of immersion or submersion to establishing the effects of submerged motion, there is but an insignificant step; both steps being in the same direction, and of much similarity. And yet how long an interval there has been between the two!

The unsatisfactory nature of the substitutes of presumption accepted in place of facts, relating to submerged motion, caused the author to investigate. Weisbach's quoted words were not convincing as to the methods so far exclusively applied. Neither did it appear a necessity to make use of the higher mathematics for digesting mentally so extremely simple an occurrence in nature as the moving of a solid in water. I set out to find the true nature of so simple an occurrence, and, thinking of the discoverer of the effects of immersion, called out "Eureka!" when I had found this true nature of the most simple occurrence of immersed motion. Nearly eighteen years have since passed, and I have, at sundry times, given publicity to what I had thus ascertained, amongst others, in "Dynamical Metallurgy" (*Mining Record*, January to June, 1882), in *Van Nostrand's Engineering Magazine*, of September, 1886, and later, as above stated, in the *Journal of the Franklin Institute;* but at no time, except in one case, controverted in the last-mentioned journal, has the fundamental fact, on which the entire new system of necessity rests, been contradicted. Therefore it appears, that the law, as claimed by me to exist in nature, has found a general recognition, and that the time has come after so many years since its discovery, and after thousands of years of error, when the school-boy, as well as the professional engineer, should learn about it.

While thus it has been shown that Rittinger's fundamental value for maximal velocity of fall in submersion may be, by employing a different method of evolving the same, not only corrected* but also expanded in its applicability, the essentially meritorious character of his work is thereby in no wise lessened. But it may be truly said that the most important fruit of his work has been, even to the present date, neither recognized nor utilized by either practical or theoretical ore-concentrators generally, the latter even wiping his merits off the table of respectful consideration by speaking of "*pure mathematics telling what ought to happen but does not.*"†

The essential merit of Rittinger's work must be found in his uncontradictable demonstration of the necessity, in practical ore-concentration, of alternating action as between sorting by equal falling-velocities in a medium and a specific grading by size, the latter action producing a different effect from the action of sorting, and it being immaterial which one of these actions, of absolutely different effect, shall precede the other, so long as an alternation between them takes place.‡ *Separation* by density of mixed particles never results

* The correction of this fundamental value of maximal velocity of fall involves as a necessary consequence the correction of all ratios of d for equal-falling particles of different densities. For quartz and galena this ratio, according to Rittinger, is as 1 : 4, while under the law of continuous displacement, the same values of δ being assumed (7.5 for galena) it is as 1 : 5. The superior accuracy of the latter ratio is confirmed by the following statement of Prof. Richards (Discussion of the paper of James Douglas on "Summary of American Improvements and Inventions in Ore-Crushing," etc., *Trans.*, xxii., 651):

"The discussion of the jigging of water-sorted products for a two-mineral separation has been well done by Prof. Munroe, of New York, showing that jigging can really separate particles of greater disparity in size than Rittinger's formula allows."

The reference is to Prof. Munroe's paper (*Trans.*, xvii., p. 637), on "The English *vs.* The Continental System of Jigging." But by neither Prof. Richards nor Prof. Munroe is the discrepancy considered as evidence of error in Rittinger's formula, or as a justification of my correction of the same. On the contrary, Prof. Munroe explains it by the assumption (approved by Prof. Richards, but in my judgment erroneous), that particles movable in a medium act upon one another as the firm walls of a confining solid tube would act, and that to this reciprocal action the observed effect is attributable. Thus the facts support the new teaching, though it be ignored by these savans, who allege other causes for the practical results observed.

† See Prof. Richards, in the Discussion of Mr. Douglas's paper, already cited (*Trans.*, xxii., 651).

‡ Alternation of action relates to the material subjected thereto, and not to a plurality of apparatus. Both kinds of action may be applied in practice to the mixed particles treated in the same apparatus in succession, in which case exclu-

from either the one or the other of these two actions when applied to the exclusion of the other. To call an apparatus effecting exclusively either one or the other of these essentially different actions a *separator*, or even "*positive separator*," indicates a looseness in terminology which is either the result of imperfect perception of the case under consideration, or an impediment to a rational treatment of the subject.

Rittinger's entire demonstration presses into consideration the necessity thus stated until it must be accepted as the first, undeniable life-theorem in practical ore-concentration. To discard it, leads, of necessity, to confusion; and, without it, the phenomena observed in practice must remain without satisfactory explanation, and experimenting loses the rational guidance without which it must remain a wanderer in the dark.

THE ELK GARDEN AND UPPER POTOMAC COAL-FIELDS OF WEST VIRGINIA.

BY JOS. D. WEEKS, PITTSBURGH, PA.

(Virginia Beach Meeting, February, 1894).

On the extreme fringe of the great Appalachian coal-basin is a long narrow detached coal-field, which is, in some respects, one of the most important in the United States. This field, about 90 miles long by 2½ to 16 miles wide, extends from the southwest corner of Somerset county, Pennsylvania, through Allegany and Garrett counties, Maryland, Mineral, Grant and Tucker counties, West Virginia, into Randolph county, West Virginia. In this distance four distinct sub-districts are recognized, the Wellersburg in Pennsylvania, the Cumberland-Georges creek in Maryland, and the Elk Garden and the Upper Potomac in West Virginia. The output of coal from the whole field including steam, domestic, smithing and coking-coal of the best quality, is about 4,500,000 tons annually. It is the nearest to tide-water of all the bituminous coal-fields which supply the great coal-markets of the Northern Atlantic seaboard;

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sively the apparatus becomes a *separator* proper. Jigs may act as graders as well as sorters, according to the method of their operation and according to the material fed into them.

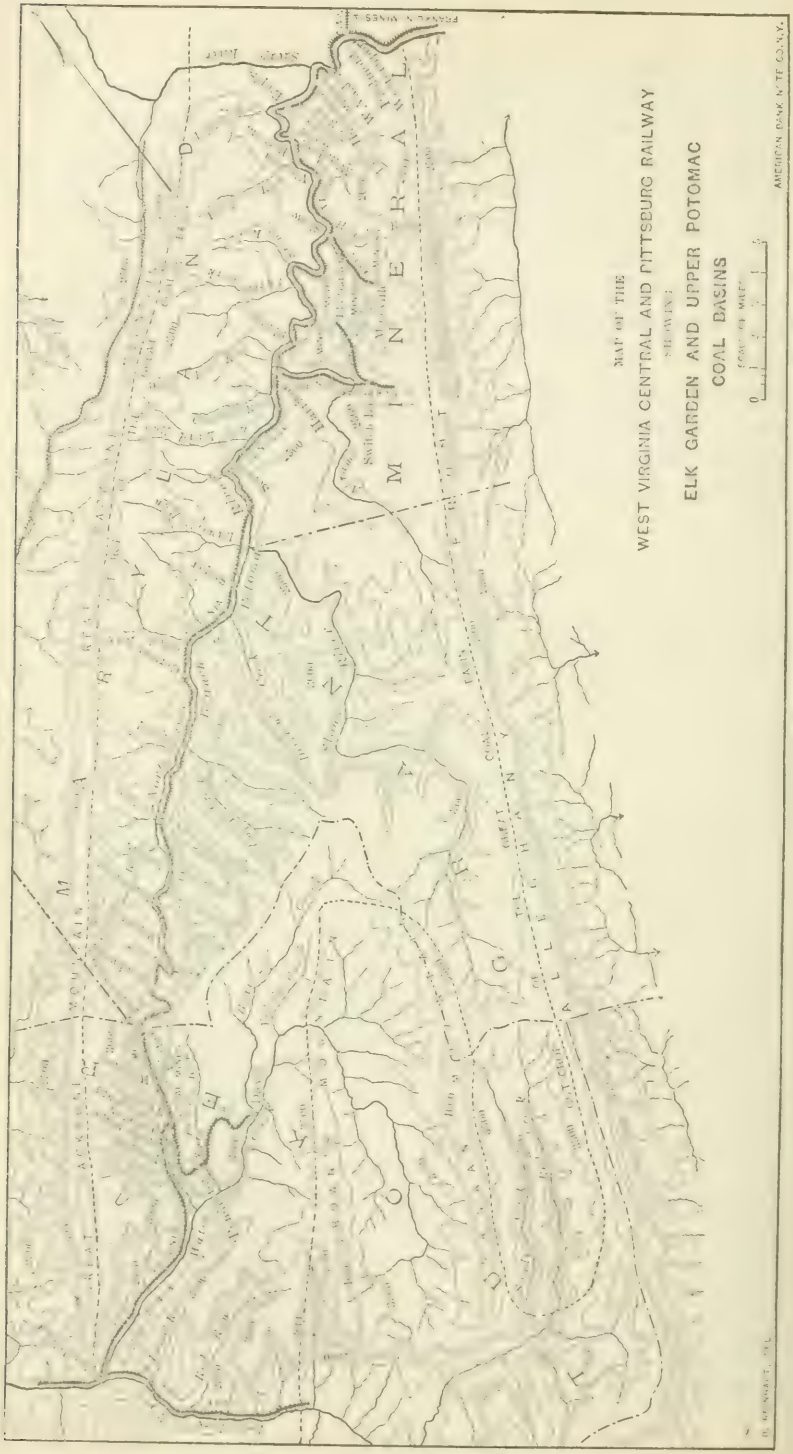

MAP OF THE
WEST VIRGINIA CENTRAL AND PITTSBURG RAILWAY
SHOWING
ELK GARDEN AND UPPER POTOMAC
COAL BASINS
AMERICAN BANK N.TE CO.N.Y.

Fig. 1.

and its coal-beds are so situated as to permit a well-nigh unlimited increase of production, should the trade of these markets demand it.

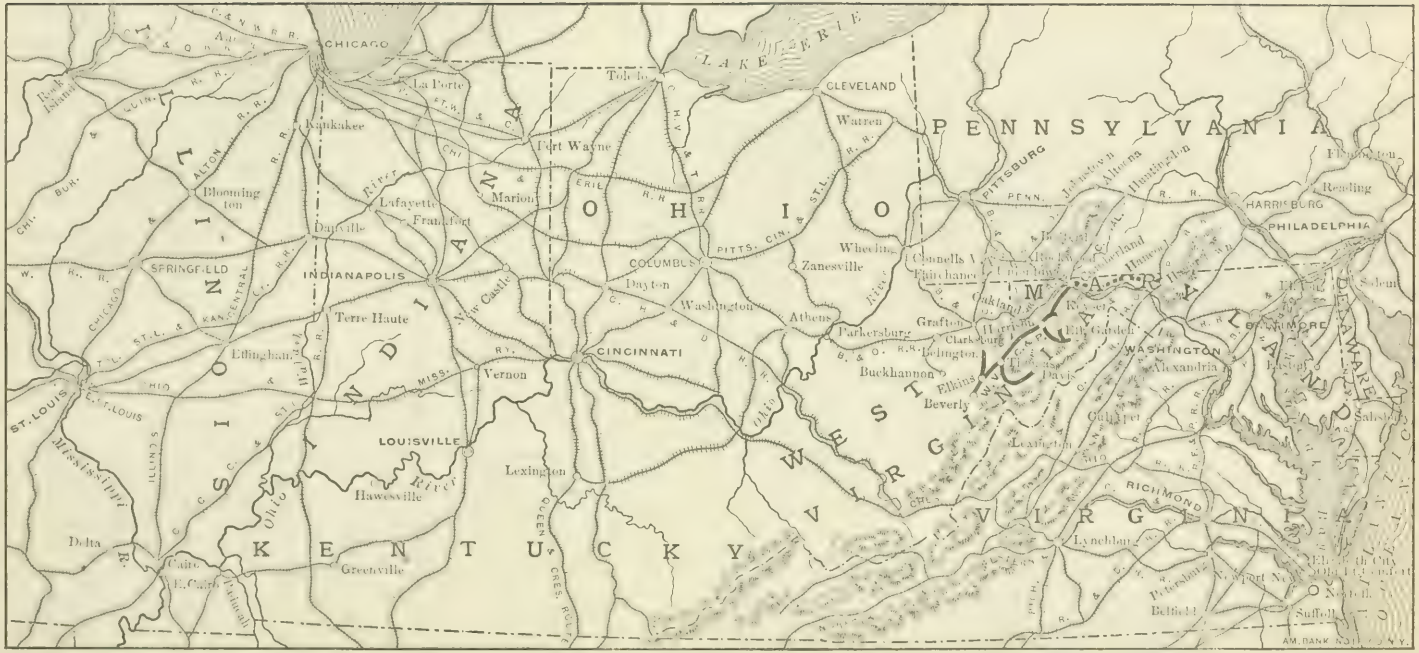
This great coal-field has sometimes been termed the Cumberland coal-field, from the fact that the Cumberland field proper, which, about half a century ago, began sending its unsurpassed steam-coal into market, was for a long time the only one of the sub-basins which produced coal; but as the name "Cumberland" is now more appropriately applied to a coal (that of the Big Vein) which is not mined throughout the entire district, and as the amount of coal in the beds below the Big Vein is vastly greater than that remaining in it, some other name would be more appropriate and less misleading. As the district is watered chiefly by the Potomac river and its tributaries, and as most of the mining is along the banks of that stream, the name "Potomac Basin" has been suggested for this entire coal-field; the distinctive and well-known names of the several sub-basins, however, being still retained.

The general course of this basin is northeast and southwest. It is hemmed in by the Allegheny Front mountains on the east and the Backbone mountains on the west. Its general shape from Pennsylvania to near the southern border of Tucker county, West Virginia, where it abuts on several parallel mountain-ranges, is that of a wedge, very narrow in Pennsylvania, only $2\frac{1}{2}$ miles wide at the State line, and widening as the mountains draw away from each other, until, at the point named in Tucker county, it is some 16 miles wide.

The Potomac river, which rises in Tucker county, West Virginia, after following, from its source, the general (northeasterly) direction of the mountains, turns abruptly to the east at a point where the Baltimore and Ohio Railroad crosses it, near Piedmont, West Virginia, and cutting across the coal-fields, as well as through the mountains, bends at Keyser to the northward again, and runs along the base of Dan's mountain, wholly outside of the coal-basin. Its course within the basin is shown in the accompanying map (Fig. 1). The Potomac thus divides this coal-field into two parts, a northern, which includes the Wellersburg, Cumberland and George's creek fields, and a southern, in which are the Elk Garden and Upper Potomac fields. It is of the southern division that the present paper especially treats.

The West Virginia Central and Pittsburgh Railway, to the construction of which the development of the Elk Garden and Upper Potomac fields is due, and which is their only outlet, extends at present from Cumberland, Maryland, to Belington in Barbour

FIG. 2.



Map Showing Railroad Connections to the Seaboard and the West of the West Virginia Central and Pittsburgh Railway.

county, West Virginia, a distance of 131 miles. For 74 miles it follows the Potomac to its headwaters at Fairfax, where it crosses the divide at Cheat river, which it follows to the Tygart's Valley river and down this stream to Belington.

At Cumberland the West Virginia Central and Pittsburgh Railway makes connection with the Baltimore and Ohio and Pennsylvania railroad-systems and the Chesapeake and Ohio Canal, and through these outlets to tide-water and to the great coal-consuming cities of the Atlantic seaboard. In addition to these, the Baltimore and Cumberland road, which is being built in the interest of the West Virginia Central Railway Co., will furnish a direct connection from Cumberland to the Pennsylvania and Reading systems in the Cumberland Valley. Indeed, one of the great advantages of the entire Potomac district, is its nearness to tide-water. One of the notable physical features of the Atlantic coast-line of the United States is the way it is broken by the tidal estuaries and bays which separate Maryland and Virginia, and thrust themselves so far inland bringing tidal water and the head of navigation very far towards the mountains. Another effect of the existence of these immense bodies of water on the border-line between the north and the south has been to determine the route of the leading highways of trade between these sections, deflecting them from the coast.

As a result of these physical features, the Potomac coal-field lies nearest to tide-water of all the important fields that supply the great coal-markets of the Atlantic border. The distance from the Elk Garden region to tide-water at Baltimore is 218 miles, while the competing field nearest to tide-water is the Clearfield, which is 253 miles from Philadelphia, its nearest tide-water port. By reason of the fact that the Chesapeake has deflected the lines of railroad inland, this region can reach the great eastern coal-markets by rail over remarkably short lines, 312 miles to Philadelphia *via* the Baltimore and Ohio, and 392 miles to New York *via* the Pennsylvania railroad. When the extension, by the Baltimore and Cumberland railway which has already been begun, shall have been completed to Hagerstown, these distances will be materially shortened. The railroad map (Fig. 2) shows the commercial relations of the region.

Most of the bituminous coals which are sent to the North Atlantic seaboard-markets are mined from four great coal-beds: the Pittsburgh of the Upper Measures, the Freeport and the Lower Kittanning of the Lower Measures, and the New River and Flat Top beds of the Pottsville Conglomerate, or Seral.

All four of these coals are found in the district here described; and at no other point in the Appalachian field do the first three exhibit greater size, or ease and cheapness of mining. The Pittsburgh bed is the Big Vein of this region, reaching at times a thickness of 16 feet, averaging 12 feet to 14 feet, and carrying 67 to 72 per cent. of carbon, 19.33 to 26.5 per cent. of volatile matter and but 5 to 6.75 per cent. of ash. The Upper Freeport (Thomas) measures 8 feet and gives from 4 to 6 feet of good workable coal, while the Lower Kittanning (Davis) bed measures 11 feet and works 6½ feet of valuable steam- and coking-coals, remarkably low in sulphur and phosphorus. Though the New River group has been found in this district, it has at present but little commercial importance.

The "Big Vein" or Pittsburgh seam, though it enjoys the widest reputation, is by no means the most important or valuable coal of this basin. Without wishing to detract from the well-established and widely-known reputation of this coal, I would simply indicate that in this district it is not as important or valuable as other coals. The "Big Vein" goes into the air just north of Stony river, and shows in but one place to the south of the Elk Garden district, namely, on the top of Fairfax Knob, at a height of some 3200 feet above the sea, where a few acres are still left.

As has been suggested, the Upper Freeport (Thomas) and the Lower Kittanning (Davis) are the important coals of this district. One or the other, sometimes both, are found throughout the entire length of both the Elk Garden and Upper Potomac districts, except here and there in a narrow valley, where they have been cut out. While, therefore, the beds are not as thick as the "Big Vein," yet, by reason of the great area underlain by these beds, the amount of coal remaining in them in the hills is vastly greater than that belonging to the "Big Vein." It is estimated at 90,000,000 tons in the Elk Garden, and 2,000,000,000 tons or more in the Upper Potomac district. The range of use of these coals is wide. The Freeport (Thomas) lump is shipped as a domestic fuel, and the "run of mine" as steam-coal, while the slack is coked, though the coke does not equal in physical structure or purity that made from the other bed. The Lower Kittanning (Davis) coal, which in this basin is remarkably free from sulphur, is, in the form of "run of mine," shipped as steam-coal; and the coarse slack has a large market in the west as a smithing-coal, while the fine slack makes a strong, bright, porous coke.

The Elk Garden District.

The Potomac river divides the Elk Garden district from the Cumberland-George's Creek field. This district extends from near Piedmont, in West Virginia, to Stony river, just south of Schell, West Virginia, a distance of 20 miles. The coal-measures, which are both the Upper and Lower Productive, lie on both sides of the Potomac, though nearly, if not quite, all that is left of the Pittsburgh seam is on the West Virginia side.

The Pittsburgh seam is mined to a considerable extent, and yields a coal with all the well-known characteristics of the "Big Vein" coal of this basin, being an excellent steam- and smithing-coal. The vein averages 14 feet, and it is estimated that some 10,000,000 tons of coal are still available in the bed within this district.

While the chief production of coal in the Elk Garden district is from the "Big Vein," some of the beds of the Lower Measures are also worked, viz., the Upper Freeport (4-foot vein) and Lower Kittanning (6-foot vein). At one place a 7-foot vein above the Pittsburgh is mined. In these smaller veins, which underlie a much larger area than the Big Vein, it is estimated that some 90,000,000 tons are still available, making, with the "Big Vein" coal still remaining, about 100,000,000 tons of available coal in the Elk Garden district.

An analysis of the coal from the Big Vein of this district, made by the United States Geological Survey in 1888, is as follows:

Analysis of Elk Garden Coal from Pittsburgh Vein.

	Per cent.
Moisture,	0.76
Volatile carbon,	19 39
Fixed carbon,	72.99
Ash,	6.86
	100.00
Sulphur,	0.880

Mining began in the district in 1881, and the first shipment of coal was made October 20th of that year. The total amount mined and sent to market up to December 31, 1892, has been 4,822,706 tons. The total shipment for each year since mining began and for each mine is given in the table on page 358.

The Upper Potomac Field.

The Upper Potomac field of the Potomac basin is at present re-

garded as extending from Stony river in Mineral county, West Virginia, where the Elk Garden district ends, through Grant and Tucker counties to the end of the basin in Randolph county. The important coal-workings, however, are just beyond Fairfax, at Thomas, Davis, Coketon, and Douglas, on the watershed between the Potomac and Cheat rivers.

The chief coal-beds worked in this district are the Upper Freeport and the Lower Kittanning in the Lower Measures. These are known locally as the Thomas and Davis veins, so named from the brothers, Henry G. and Thomas B. Davis, who were the pioneers in the development of this region. The Thomas vein is the Upper Freeport; the Davis vein the Lower Kittanning. The Thomas vein of this region has a thickness of nearly 8 feet; but a bony coal or slate near the center of the bed leaves only about 6 feet of merchantable coal. The coal is valuable for both steam- and coking-purposes. The Davis vein has a thickness of nearly 11 feet. With

TABLE I.—*Total Shipments of Coal from All the Mines in the Elk Garden Region on the Line of the West Virginia Central and Pittsburgh Railway, from October* 20, 1881, *When the First Shipment was Made, to January* 1, 1894.

Year.	W. Va. Central. Tons.	Big Vein. Tons.	Davis & Elkins. Tons.	Atlantic. Tons.	Virginia. Tons.	Hampshire. Tons.	Switchback. Tons.	Merrill's. Tons.	Total Tons.
1881	11,372	.....	.....	.....	.....	.....	.....	.....	11,372
1882	228,294	28,774	.....	.....	.....	.....	.....	.....	257,068
1883	261,075	68,942	.....	.....	.....	.....	.....	.....	330,017
1884	375,590	83,170	.....	.....	.....	.....	.....	.....	458,760
1885	268,780	65,792	.....	57,213	.....	.....	.....	.....	391,785
1886	211,852	50,686	.....	63,715	.....	.....	.....	.....	326,253
1887	259,354	59,801	.....	107,605	.....	.....	.....	.....	426,760
1888	356,259	30,407	.....	97,285	.....	.....	.....	.....	483,951
1889	353,216	.....	18,671	62,934	1737	.....	.....	.....	436,558
1890	414,547	.....	73,384	63,121	5926	.....	.....	.....	556,978
1891	420,503	538	88,576	92,192	193	502	5718	1872	610,094
1892	358,915	3,876	71,033	42,365	.....	17,326	39,421	174	533,110
1893	.....	.....	.....	.....	.....	.....	.....	.....	450,394
	3,519,757	391,986	251,664	586,430	7856	17,828	45,139	2046	5,273,100

respect to its tenor of sulphur, this is one of the purest coals mined. It is not only used largely for steam, but is especially valuable for blacksmithing purposes, having an extensive market in the west, where it is put in bags and sold to interior points off the lines of transportation. For coking, it is an excellent material.

The following (Table II., page 360) are analyses of these coals, made by the United States Geological Survey from full sections.

This field is of great extent; how great, has not been determined. In 1882, when the explorations were exceedingly limited, Prof. Pumpelly, who prepared the report on coal for the Tenth Census, estimated that there were 250 square miles of coal-land in the Upper Potomac and Stony River basins, after allowing for the erosion of the valleys, and gave 2,000,000,000 tons as the minimum amount of coal in beds of workable thickness, half of which was above drainage and accessible from the valleys. As the 8-foot vein was the largest known at that time, it is evident that the amount of coal in the basin was much under-estimated.

At Coketon, only the Lower Kittanning, or Davis seam, is mined. A section of this coal, known here as the 11-foot seam, is as follows:

Section of Davis Vein at Coketon, West Virginia.

	Inches.	
Coal, upper bench,	12	Portion worked.
Slate,	2	
Coal, middle bench,	18	
Slate,	2	
Coal, lower bench,	44	
	78	
Slate,	18	Floor, not mined.
Coal,	32	
	128	

Of the 106 inches of coal in the seam, the 32 inches below the 18-inch slate is left in the floor. All of the vein above this 18-inch slate is mined, giving in actual working 72 to 78 inches of coal. For this reason the seam is sometimes called the 6-foot or 6½-foot coal.

This coal shows the columnar structure typical of all good coking-coals. It is so soft as to be easily broken by the hand, and breaks flaky and in slivers, even the finest slack showing under the microscope minute columnar flakes. The "run of mine" is used for steam, and the coarse slack is in high repute and good

TABLE II.—*Analysis of Thomas and Davis Coals, Upper Potomac Field, West Virginia.*

	Davis (Lower Kittanning).		Thomas (Upper Freeport).		
	No. 1.	No. 2.	Upper.	Middle.	Bottom.
Moisture	0.80	0.70	0.64	0.68	0.96
Vol. Matter	26.84	22.03	22.87	23.88	22.90
Fixed Carbon	67.18	70.53	65.60	65.99	72.76
Ash	5.18	6.74	10.89	9.45	3.38
Total	100.00	100.00	100.00	100.00	100.00
Sulphur	1.68	0.924	0.64	1.39	0.59
Phosphorus	.....	.....	0.06	0.02	0.01

demand in the west for blacksmithing, while the slack of the lower bench is coked.

The conditions at Douglas, where the same vein is mined, are similar.

At Thomas, but a mile north of Coketon, the Davis vein has gone below the surface, and the Thomas vein appears in the hillside above the railroad. The distance here between the two veins is 200 feet, though the shaft sunk to the Lower Kittanning is but 184 feet deep.

A section of the Thomas (Upper Freeport) vein, at Thomas, which is an average of thirty sections made by Mr. F. S. Landstreet, the General Manager of the Davis Coal and Coke Company, the chief operator throughout this entire region, is as follows:

Section of Thomas Vein (Upper Freeport) at Thomas Mine, West Virginia.

	Inches.
Coal,	18
Bone coal,	5
Coal,	6
Slate and bone,	4
Coal,	4
Bone coal,	3
Coal,	6
Slate,	3
Coal,	37
Total,	86
Coal,	71
Slate and bone,	15

Of the 86 inches shown in the section, some 78 inches is mined, the upper bench (18 inches) being removed first. The coal is harder than that of the Davis seam, breaks with a cubical fracture, and will stand transportation better. The lump is shipped for domestic purposes and the run of mine for steam purposes, while the slack is coked.

Shipments were begun from this field in 1885, since which time, up to December 31, 1892, the output has been 881,914 tons of 2240 pounds, as will be seen from the following table:

TABLE III.—*Total Shipments of Coal from All the Mines in the Upper Potomac Region on the Line of the West Virginia Central and Pittsburgh Railway from* 1885 *to December* 31, 1893.

	Davis. Tons.	Thomas. Tons.	Douglas. Tons.	Spring Garden Tons.	Fairfax Tons.	Bayard Tons.	Junior. Tons.	Total. Tons.
1885	8,434	1,131	.....	.....	.....	.....	.....	9,565
1886	17,904	2,772	.....	.....	.....	.....	.....	20,676
1887	18,287	3,774	.....	.....	.....	.....	.....	22,061
1888	69,297	10,350	.....	.....	.....	.....	.....	79,647
1889	99,771	23,320	.....	696	.....	.....	.. ...	123,787
1890	111,477	30,495	.....	1290	.....	.... .	.....	143,262
1891	141,142	55,888	15,146	1247	357	.....	.....	213,780
1892	107,877	95,160	62,860	.....	990	416	1833	269,136
1893	.....	.....	.....	.....	.....	.....	.....	423,610
	574,189	222,890	78,006	3233	1347	416	1833	1,305,524

The total production of the Elk Garden and Upper Potomac fields, from the beginning of shipments in 1881, have been as follows in tons of 2240 pounds. (See Table IV., page 362.)

COKE-MAKING IN THE UPPER POTOMAC REGION.

Though all three seams of coal mined in the Elk Garden and Upper Potomac regions are coking-coals, only two are coked, the Thomas (Upper Freeport) and Davis (Lower Kittanning), and chiefly the latter. In addition to its being more valuable as a steam- than as a coking-coal, the Big Vein coal is lower in volatile

matter than either the Thomas or the Davis vein, and does not coke as readily.

There are three coke-plants, with 393 ovens, in this field, all in the Upper Potomac district, no coal being coked at present in the Elk Garden district. Two of these, the plant at Coketon, with 327 ovens, and the one at Douglas, just south of Coketon, with 44 ovens, coke the Davis seam. The plant at Thomas, just north of Coketon, with 22 ovens, uses the Thomas coal.

TABLE IV.—*Total Coal-Production of Elk Garden and Upper Potomac Fields.*

Year.	Elk Garden.	Upper Potomac.	Total.
1881	11,372	.....	11,372
1882	257,068	.....	257,068
1883	330,017	.....	330,017
1884	458,760	.....	458,760
1885	391,785	9,565	401,350
1886	326,253	20,676	346,929
1887	426,760	22,061	448,821
1888	483,951	79,647	563,598
1889	436,558	123,787	560,345
1890	556,978	143,262	700,240
1891	610,094	213,780	823,874
1892	533,110	269,136	802,246
1893	450,394	423,610	874,004
	5,273,100	1,305,524	6,578,624

The ovens used are all bee-hives of the standard Connellsville type, 12 feet in diameter by 7 feet high, the batter of the ovens beginning 18 inches above the floor.

Slack or fine coal only is used, experience having shown that the run of mine or lump does not yield as good a coke. The charge is $5\frac{1}{2}$ tons for 48-hour and $6\frac{1}{2}$ tons for 72-hour coke. The actual yield of coke by weight at the Coketon plant, using the Davis seam, is over 67 per cent.

The coke is bright, silvery, porous, and hard, and has an excellent reputation for foundry uses because of its physical characteristics and low sulphur. It is shipped largely for this purpose to Mexican and South American ports. It is also an excellent blast-furnace fuel, and, when selected and crushed, has a large sale for domestic purposes.

An average of ten analyses of coke, made from the Davis seam at Coketon, West Virginia, is as follows:

Average of Ten Analyses of Coketon Coke.

	48-hour. Per cent.	72-hour. Per cent.
Water,	0.218	0.39
Volatile matter,	1.296	1.348
Fixed carbon,	89.352	91.291
Ash,	9.134	6 971
	100.00	100.00
Sulphur,	0.5946	0.566
Phosphorus,	0.0366	0.024

The first shipment of coke was made from this region in July, 1887. The total production to December 31, 1892, was 242,205 tons. The shipments by years have been as follows:

TABLE V.—*Statement Showing Total Shipments of Coke from All Ovens on the Line of the West Virginia Central and Pittsburgh Railway from July*, 1887, *when the First Coke was Made to December*, 31, 1893.

Year.	Thomas Ovens.	Coketon Ovens.	Douglas Ovens.	Custer Ovens.	Total Tons.
1887	2,211	.....	.....	.....	2,211
1888	5,432	.....	.....	.....	5,432
1889	12,657	.....	.....	.....	12,657
1890	7,409	48,208	.....	.....	55,617
1891	8,901	68,273	3,315	.....	80,489
1892	8,992	57,745	18,429	633	85,799
1893	.....	.....	.....	.....	84,186
	45,602	174,226	21,744	633	326,391

It is evident not only from the character of the coke and from its special adaptation to foundry and domestic uses, but from its nearness to tide-water and to markets, that the coke-industry in this region will have a prosperous future. The advantageous position of the mines to the trade, and the excellent quality of the coal, will enable both the coal and coke of this section to compete at all times with those produced in other districts.

NOTES ON THE RE-WORKING OF ANTHRACITE CULM-BANKS.

BY ARTHUR W. SHEAFER, POTTSVILLE, PA.

(Virginia Beach Meeting, February, 1894.)

Mr. Eckley B. Coxe, in his able and interesting paper on "The Preparation of Small Sizes of Anthracite,"* read and discussed at the Glen Summit Meeting, October, 1891, refers to the reputed immense amount of coal contained in the culm-banks, which are the unsightly accompaniment of every mining operation in the anthracite coal-regions. Mr. Coxe thinks the available amount and value greatly exaggerated, giving among his reasons (1) the spoiling of the culm by mixing with other refuse; (2) its deterioration; (3) its destruction by fire; and (4) frequent failures to realize expectations as to the size of the banks. These points are undoubtedly important factors in determining the advisability of working any individual bank, but they have a varying influence in the several regions, some prevailing more in one than in another.

Although the amount of fuel lying waste in these banks is still an uncertain and undetermined quantity, yet no one can visit the Schuylkill region without being impressed with the fact that a considerable amount of culm, too valuable to be left merely to the mercy of the elements, surrounds and encumbers each site of an old or present colliery operation.

The rapid increase in the number of washeries, and the demand for leases on old culm-banks, shows that, in the Schuylkill region at least, the question of deterioration does not play an important part. The proportion of coal to refuse, the relative percentage of the sev-

* *Trans.*, xx., 613.

eral sizes won, the cost of handling and washing, and the market price obtained, are the elements which here most concern the owner of a washery-plant. As bearing upon the question of proportion of coal to refuse, and percentages of the several sizes won, I append the shipments of three plants, especially constructed for the re-working of culm-banks. Each of the banks in question is an old bank; that is, was deposited between 1865 and 1880, before the day of "buckwheat" and coals of still smaller size. The culm is almost exclusively from the Mammoth bed. The large rock and other refuse from the mines had been in these cases deposited, in the main, separate from the breaker-waste.

The sizes of screen-mesh for each of the washeries are:

Stove-coal, over $1\frac{1}{8}$ inches.

Chestnut-coal, through $1\frac{1}{8}$ inches over $\frac{6}{8}$ inch.

Pea-coal, through $\frac{6}{8}$ inch over $\frac{1}{2}$ inch.

Buckwheat-coal, through $\frac{1}{2}$ inch.

Dirt, through $\frac{1}{4}$ inch and over $\frac{3}{16}$ inch.

The percentage of No. 2 buckwheat (rice) shipped is so small that no separate account has been made of it, except in Table VI. Elsewhere it is included under the head of buckwheat.

Tables I. to IV. show the result of the Stanton washery for the years 1889 to 1892. I regret that I am unable to record the number of wagons of culm dumped in the washery each month, thus giving the data upon which to determine the number of cubic feet of bank necessary to yield 1 ton of coal; but careful tests, made in 1889 and 1890, proved that 70 per cent. of the bank thus worked was marketable, and that actually 60 per cent. was shipped from the breaker.

The bank then washed was, as the table shows, an exceptionally good one. In the years 1891 and 1892 the percentage was not determined, but, judging from the tables, it could not have yielded more than 45 per cent.

Tables V. and VI. show the results of a much newer operation, which has just completed its second year, and is not yet working the oldest portions of the banks.

Tables VII. and VIII. show the percentages obtained in a still more recent operation, working a poorer bank, much mixed with large slate and rock. There is, of course, a large amount of culm smaller than buckwheat, which is now allowed to waste on account of the limited demand for such sizes, but which, in the near future, will be utilized as a fuel, and thus materially increase the percentage of coal to be won from these so-called refuse banks.

TABLE I.—*Stanton Culm-Bank: Shipments for* 1889.

Size of Screen-mesh: Stove, over $1\frac{1}{8}$ in.; Chestnut, through $1\frac{1}{8}$ in., over $\frac{3}{4}$ in.; Pea, through $\frac{3}{4}$ in., over $\frac{1}{2}$ in.; Buckwheat, through $\frac{1}{2}$ in.

	Wagons dump'd in breaker.	Stove.			Chestnut.			Pea.			Buckwheat.			Total.	
		Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.
January		110	07	18.20	107	07	17.70	74	00	12.20	314	12	51.89	606.06	
February		103	09	16.83	116	07	18.92	82	15	13.46	312	03	50.78	614.14	
March		14	01	20.36	18	01	26.16	04	18	7.10	32	00	46.37	69.00	
April		194	13	21.46	180	17	19.95	123	14	13.64	407	11	44.95	906.15	
May		449	12	20.15	469	00	21.02	309	15	13 88	1002	16	44.95	2231.03	
June		506	08	19.63	589	12	22.85	370	18	14.38	1112	09	43.12	2579.07	
July		689	10	23.88	471	08	16.43	405	06	14.03	1319	00	45.66	2888.04	
August		635	02	20.80	525	18	17.22	484	19	15.88	1407	12	46.19	3053.11	
September		594	16	22.94	436	03	16.82	356	12	13.75	1204	16	46.47	2592.07	
October		640	08	20.31	419	14	13.31	413	01	13.10	1679	15	53.27	3152.18	
November		652	13	19.62	458	11	13.78	509	07	15.31	1705	19	51.28	3326.10	
December		611	01	18.85	433	03	13.35	462	07	14.25	1734	19	53.52	3241.13	
Total		5202	03	20.59	4229	01	16.74	3597	12	14.24	12,233	12	48.42	25,262.08	

TABLE II.—*Stanton Culm-Bank: Shipments for* 1890.

Size of Screen-mesh: Stove, over $1\frac{1}{8}$ in.; Chestnut, through $1\frac{1}{8}$ in., over $\frac{3}{4}$ in.; Pea, through $\frac{3}{4}$ in., over $\frac{1}{2}$ in.; Buckwheat, through $\frac{1}{2}$ in.

	Wagons dump'd in breaker.	Stove.			Chestnut.			Pea.			Buckwheat and culm.			Total.	
		Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.
January		748	15		562	06		499	14		1392	09		3203.04	
February		475	11		345	12		304	14		898	01		2023.18	
March		345	06		561	01		360	15		1030	10		2297.12	
April		822	12		1449	18		929	16		3084	10		6286.16	
May		950	11		1632	10		1179	04		3294	00		7056.05	
June		1079	08		1436	04		794	01		2648	06		5957.19	
July		802	03		1268	00		761	02		3222	02		6053.07	
August		811	00		1094	19		860	11		3003	06		5769.16	
September		725	18		892	13		732	15		2908	15		5260.01	
October		1014	09		937	04		890	02		2703	18		5545.13	
November		736	17		1417	18		1288	09		3593	03		7036.07	
December		416	16		1183	19		1162	03		3554	04		6317.02	
Total		8929	06	14.21	12,782	04	20.35	9763	06	15.55	31,333	04	49.89	62,808.00	

TABLE III.—*Stanton Culm-Bank: Shipments for* 1891.

Size of Screen-mesh: Stove, over $1\frac{1}{8}$ in.; Chestnut, through $1\frac{1}{8}$ in., over $\frac{3}{4}$ in.; Pea, through $\frac{3}{4}$ in., over $\frac{1}{2}$ in.; Buckwheat, through $\frac{1}{2}$ in.

	Wagons dump'd in breaker.	Stove.			Chestnut.			Pea.			Buckwheat and culm.			Total.	
		Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.
January	.....	423	15	.....	945	18	.....	1197	00	.....	4228	03	.....		6794.16
February	.....	329	02	.....	877	01	.....	1329	17	.....	3807	01	.....		6343.01
March	.....	215	16	.....	824	03	.....	1617	14	.....	4061	05	.....		6718.18
April	.....	156	14	.....	915	11	.....	1359	06	.....	4139	03	.....		6570.14
May	.....	267	07	.....	986	06	.....	1326	13	.....	3706	19	.....		6287.05
June	.....	234	15	.....	732	02	.....	1275	17	.....	4397	19	.....		6640.13
July	.....	216	13	.....	758	00	.....	1260	07	.....	3594	06	.....		5829.06
August	.....	261	07	.....	958	06	.....	1390	18	.....	3308	17	.....		5919.08
September	.....	260	03	.....	856	07	.....	1161	10	.....	2631	16	.....		4909.16
October	.....	316	04	.....	918	06	.....	1357	18	.....	3253	04	.....		5845.12
November	.....	228	07	.....	720	12	.....	1278	09	.....	2903	18	.....		5131.06
December	.....	151	07	.....	519	10	.....	1279	13	.....	3132	17	.....		5983.07
Total	.....	3061	10	4.25	10,012	02	13.89	15,835	02	21.97	43,165	08	59.89	72,074	02

TABLE IV.—*Stanton Culm-Bank: Shipments for* 1892.

Size of Screen-mesh: Stove, over $1\frac{1}{8}$ in.; Chestnut, through $1\frac{1}{8}$ in., over $\frac{3}{4}$ in.; Pea, through $\frac{3}{4}$ in., over $\frac{1}{2}$ in.; Buckwheat, through $\frac{1}{2}$ in.

	Wagons dump'd in breaker.	Stove.			Chestnut.			Pea.			Buckwheat and culm.			Total.	
		Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.
January	.....	189	18	.....	443	05	.....	834	03	.....	2452	00	.....		3919.06
February	.....	321	15	.....	616	05	.....	1162	15	.....	3251	17	.....		5352.12
March	.....	187	1	7	435	04	.....	1204	04	.....	3223	06	.....		5050.11
April	.....	189	05	.....	380	02	.....	836	03	.....	2291	19	.....		3697.09
May	.....	258	18	.....	569	17	.....	1321	19	.....	3971	00	.....		6121.14
June	.....	239	07	.....	562	13	.....	940	15	.....	3575	02	.....		5317.17
July	.....	120	07	.....	350	14	.....	599	10	.....	2629	19	.....		3700.10
August	.....	166	16	.....	308	08	.....	1267	16	.....	2837	05	.....		4580.05
September	.....	332	12	.....	353	08	.....	1282	08	.....	3312	13	.....		5281.01
October	.....	624	02	.....	497	02	.....	972	13	.....	2471	13	.....		4565.10
November	.....	61	04	.....	196	04	.....	565	15	.....	1741	06	.....		2564.09
December	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
Total	.....	2692	01	5.37	4713	02	9.40	10,988	01	21.91	31,758	00	63.32	50,151	04

TABLE V.—*Draper Culm-Bank: Shipments for* 1891–92.

Size of Screen mesh: Stove, over $1\frac{1}{8}$ in.; Chestnut, through $1\frac{1}{8}$ in., over $\frac{3}{4}$ in.; Pea, through $\frac{3}{4}$ in., over $\frac{1}{2}$ in.; Buckwheat, through $\frac{1}{2}$ in.

	Wagons dumped in breaker.	Stove.			Chestnut.			Pea.			Buckwheat.			Total.	
		Tons.	Cwt.	Per ct.	Tons.	Cwt.	Per ct.	Tons.	Cwt.	Per ct.	Tons.	Cwt.	Per ct.	Tons.	Cwt.
December, 1891	1642	219	18	.....	400	19	.....	361	19	.....	557	02	.....	1569	18
January, 1892	2319	219	12	.....	276	18	.....	227	14	.....	900	11	.....	1624	15
February	3586	112	05	.....	279	03	.....	245	01	.....	1850	17	.....	2487	06
March	5605	258	06	.....	306	00	.....	315	14	.....	3294	19	.....	4174	19
April	3467	278	08	.....	553	19	.....	356	00	.....	2147	18	.....	3336	05
May	5616	315	00	.....	842	03	.....	834	10	.....	2849	06	.....	4840	19
June	4357	256	07	.....	623	11	.....	741	17	.....	2710	06	.....	4332	01
July	4087	192	10	.....	597	08	.....	791	05	.....	2509	13	.....	4093	16
August	5832	492	15	.....	901	01	.....	1093	15	.....	3930	07	.....	6417	18
September	5583	701	13	.....	794	09	.....	645	01	.....	3423	19	.....	5565	02
October	3578	482	15	.....	587	12	.....	567	00	.....	2201	11	.....	3838	18
November	4737	216	00	.....	623	07	.....	843	03	.....	3656	00	.....	5338	10
	49,809	3775	09	7.93	6786	10	14.26	7025	19	14.76	30,032	09	63.05	47,629	07

Average contents of wagon, 90 cwt. 94 cubic feet of bank yields 1 ton coal (40 cubic feet).
42 per ct. of bank is marketable coal.

TABLE VI.—*Draper Culm-Bank: Shipments for* 1893.

Size of Screen-mesh: Stove, over $1\frac{1}{8}$ in.; Chestnut, through $1\frac{1}{8}$ in., over $\frac{3}{4}$ in.; Pea, through $\frac{3}{4}$ in., over $\frac{1}{2}$ in.; Buckwheat, through $\frac{1}{2}$ in., over $\frac{1}{4}$ in.; Rice, through $\frac{1}{4}$ in., over $\frac{3}{16}$ in.

	Wagons dumped in breaker.	Stove.			Chestnut.			Pea.			Buckwheat.			Rice.			Total.	
		Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.
January	1804	29	03	.....	233	19	.....	300	05	.....	749	02	.....	329	13	.....	1642	02
February	4084	48	07	.....	483	19	.....	701	04	.....	2098	09	.....	825	16	.....	4157	15
March	6082	11	16	.....	1086	18	.....	1208	15	.....	3319	05	.....	1637	05	.....	7263	19
April	6002	156	15	.....	751	01	.....	956	00	.....	4861	18	.....	469	06	.....	7195	00
May	5041	132	16	.....	763	08	.....	928	10	.....	3541	04	.....	828	13	.....	6194	11
June	5889	222	09	.....	781	10	.....	706	10	.....	4500	10	.....	447	17	.....	6658	16
July	4942	272	11	.....	822	18	.....	752	18	.....	2648	02	.....	747	09	.....	5243	18
August	3636	204	03	.....	824	03	.....	871	16	.....	1530	16	.....	532	11	.....	3963	09
September	4480	26	07	.....	922	12	.....	1076	16	.....	2523	11	.....	1569	03	.....	6118	09
October	6534	102	08	.....	1298	14	.....	1376	13	.....	4206	01	.....	533	09	.....	7517	05
November	6017	44	12	.....	1224	18	.....	1230	01	.....	3001	19	.....	1424	15	.....	6926	05
December	4999	40	16	.....	920	07	.....	1061	18	.....	2648	07	.....	1081	08	.....	5752	16
	59,510	1292	03	1.88	10,114	07	14.74	11,171	06	16.27	35,629	04	51.91	10,427	05	15.20	68,634	05

Average contents of wagon, 90 cubic feet. 78 cubic feet of bank yields 1 ton of coal (40 cubic feet).
50 per cent. of bank is marketable coal.

TABLE VII.—*Furnace Culm-Bank: Shipments for* 1892.

Size of Screen-mesh: Stove, over $1\frac{1}{8}$ in.; Chestnut, through $1\frac{1}{8}$ in., over $\frac{3}{4}$ in.; Pea, through $\frac{3}{4}$ in, over $\frac{1}{2}$ in.; Buckwheat, through $\frac{1}{2}$ in.

	Wagons dumped in breaker.	Stove.			Chestnut.			Pea.			Buckwheat.			Total.	
		Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.
February	1246	106	03	.....	89	17	.....	225	00	.....	662	00	.....	1083	00
March	1714	124	03	.....	204	09	.....	447	14	.....	816	03	.....	1592	09
April	1491	80	01	.....	104	18	.....	133	07	.....	553	00	.....	871	06
May	2243	152	01	.....	132	01	.....	407	05	.....	849	09	.....	1540	16
June	2739	165	08	.....	414	15	.....	602	09	.....	1336	01	.....	2518	13
July	3070	190	05	.....	101	15	.....	701	00	.....	1188	00	.....	2484	00
August	4409	242	11	.....	147	09	.....	779	17	.....	1722	02	.....	2892	02
September	4540	278	09	.....	169	15	.....	707	16	.....	2167	15	.....	3323	15
October	3370	144	05	.....	74	05	.....	410	05	.....	1581	16	.....	2213	11
November	3420	262	03	.....	112	03	.....	566	05	.....	1546	00	.....	2486	11
December	4000	291	02	.....	202	05	.....	539	08	.....	1695	05	.....	2728	00
	32,245	2036	11	8.58	1753	12	7.39	5523	06	23.27	14,420	11	60.76	23,734	03

Average contents of wagon = 72 cubic feet. 98 cubic feet of bank yields 1 ton of coal.
40 per cent. of bank is coal.

TABLE VIII.—*Furnace Culm-Bank: Shipments for* 1893.

Size of Screen-mesh: Stove, over $1\frac{1}{8}$ in.; Chestnut, through $1\frac{1}{8}$ in., over $\frac{3}{4}$ in.; Pea, through $\frac{3}{4}$ in., over $\frac{1}{2}$ in.; Buckwheat, through $\frac{1}{2}$ in.

	Wagons dumped in breaker.	Stove.			Chestnut.			Pea.			Buckwheat.			Total.	
		Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.	Pr. ct.	Tons.	Cwt.
January	.....	15	...	.....	26	05	.....	60	10	.....	138	17	.....	240	12
February	.....	.....	...	.....	.....	...	.....	14	13	.....	.....	...	.....	14	13
March	.....	.....	...	.....	.....	...	.....	.....	...	.....	.....	...	.....	.....	...
April	4684	338	12	.....	167	03	.....	711	08	.....	2121	15	.....	3338	18
May	5473	690	10	.....	225	19	.....	710	...	.....	2529	16	.....	4156	05
June	5634	486	06	.....	205	03	.....	953	07	.....	2146	01	.....	3790	17
July	5063	263	13	.....	247	09	.....	883	08	.....	2190	...	.....	3584	10
August	4423	193	16	.....	178	19	.....	928	...	.....	2130	...	.....	3430	15
September	3891	173	06	.....	168	15	.....	599	04	.....	1752	01	.....	2693	06
October	5160	324	04	.... .	151	16	.....	922	09	.....	2299	16	.....	3698	05
November	4478	229	16	.....	193	16	.....	780	11	.....	1733	11	.....	2937	14
December	.....	12	15	.....	24	07	.....	23	19	.....	114	12	.....	175	13
	38,806	2727	18	9.73	1589	12	5.66	6587	09	23.47	17,156	09	61.14	28,061	08

Average contents of wagon, 72 cubic feet. 99.5 cubic feet of bank yields 1 ton of coal (40 cubic feet).
40 per cent of bank is marketable coal.

*ARTESIAN WELL PROSPECTS IN EASTERN VIRGINIA, MARYLAND AND DELAWARE.**

BY N. H. DARTON, U. S. GEOLOGICAL SURVEY, WASHINGTON, D. C.

(Virginia Beach Meeting, February, 1894.)

In the coastal plain region of the middle Atlantic slope there are no large supplies of potable surface-waters. The great rivers by which the region is intersected are tidal estuaries, and the water of their local branches is often of bad quality. The cities situated at intervals along the western margin of the region obtain a plentiful supply of excellent water from the rivers above the tidal estuaries, but out on the coastal plain surface-waters are mainly used. A few artesian wells have been sunk which afford abundant water, but their number is at present very limited. Norfolk, Newport News, and the many smaller places use surface-waters, and all over the region there is great need for purer and more abundant supplies.

During the past five years I have been engaged in a study of the geology of the coastal plain for the U. S. Geological Survey, and have given special attention to the question of subterranean waters. In this communication I shall present a brief review of the geological conditions under which such waters occur in this region, and give an account of the wells which have been bored. Unfortunately the number of well-records is small, and some of the most important questions of subterranean geology are as yet far from settled. It is hoped that this paper may lead to further borings, especially as the cost of artesian wells has been much reduced by the introduction of new machinery and methods, and is now quite within the reach of the average community. In New Jersey there are many wells, and the water-bearing horizons are so definitely determined that they may almost everywhere be predicted with certainty. In Maryland and Virginia the data are less definite, but there is excellent evidence of a wide distribution of water at moderate depths.

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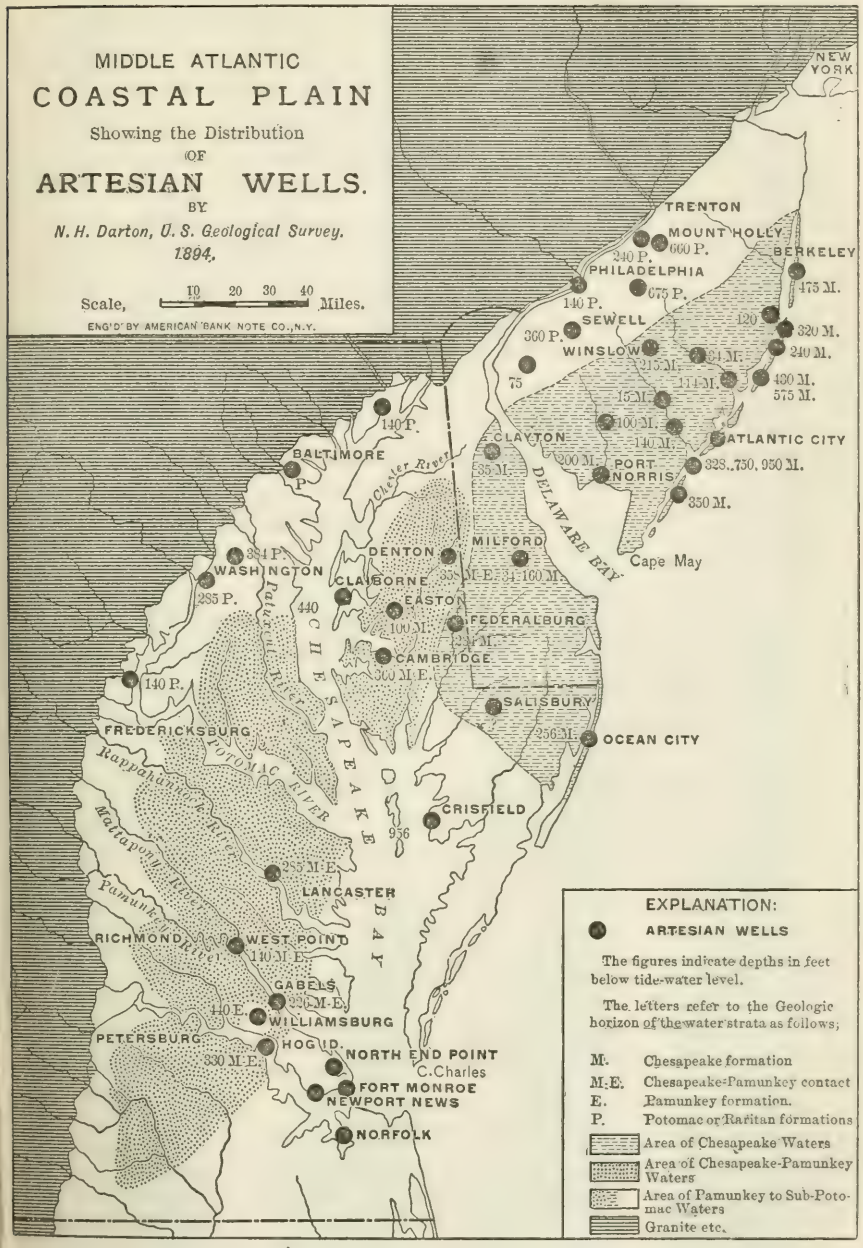
PLATE I.


MIDDLE ATLANTIC
COASTAL PLAIN
Showing the Distribution
OF
ARTESIAN WELLS.
BY
N. H. Darton, U. S. Geological Survey.
1894.
Scale, 10 20 30 40 Miles.
ENG'D BY AMERICAN BANK NOTE CO., N.Y.
NEW YORK
TRENTON
MOUNT HOLLY
240 P.
660 P.
BERKELEY
PHILADELPHIA
675 P.
475 M.
140 P.
SEWELL
120
320 M.
360 P.
WINSLOW
215 M.
84 M.
240 M.
75
114 M.
430 M.
575 M.
155 M.
100 M.
140 M.
ATLANTIC CITY
140 P.
CLAYTON
BALTIMORE
Chester River
35 M.
300 M.
PORT NORRIS
328..730, 950 M.
350 M.
DELAWARE BAY
MILFORD
DENTON
Cape May
341 P.
WASHINGTON
CLAIBORNE
285 P.
35 M.E.
160 M.
Patuxent River
440
EASTON
100 M.
FEDERALBURG
CHESAPEAKE
CAMBRIDGE
300 M.E.
140 P.
SALISBURY
FREDERICKSBURG
256 M.
OCEAN CITY
Rappahannock River
POTOMAC RIVER
CRISFIELD
Mattaponi River
Pamunkey River
285 M.E.
LANCASTER
RICHMOND
WEST POINT
110 M.E.
GABELS
220 M.E.
140 E.
WILLIAMSBURG
PETERSBURG
HOG I.
300 M.E.
NORTH END POINT
C. Charles
FORT MONROE
NEWPORT NEWS
NORFOLK
EXPLANATION:
ARTESIAN WELLS
The figures indicate depths in feet below tide-water level.
The letters refer to the Geologic horizon of the water strata as follows;
M. Chesapeake formation
M.E. Chesapeake-Pamunkey contact
E. Pamunkey formation.
P. Potomac or Raritan formations
Area of Chesapeake Waters
Area of Chesapeake-Pamunkey Waters
Area of Pamunkey to Sub-Potomac Waters
Granite etc.

Geology of the Middle Atlantic Coastal Plain Region.

It is proposed to give under this heading a brief outline of the general structure of the region in order that the relations of the water-bearing strata may be clearly understood.

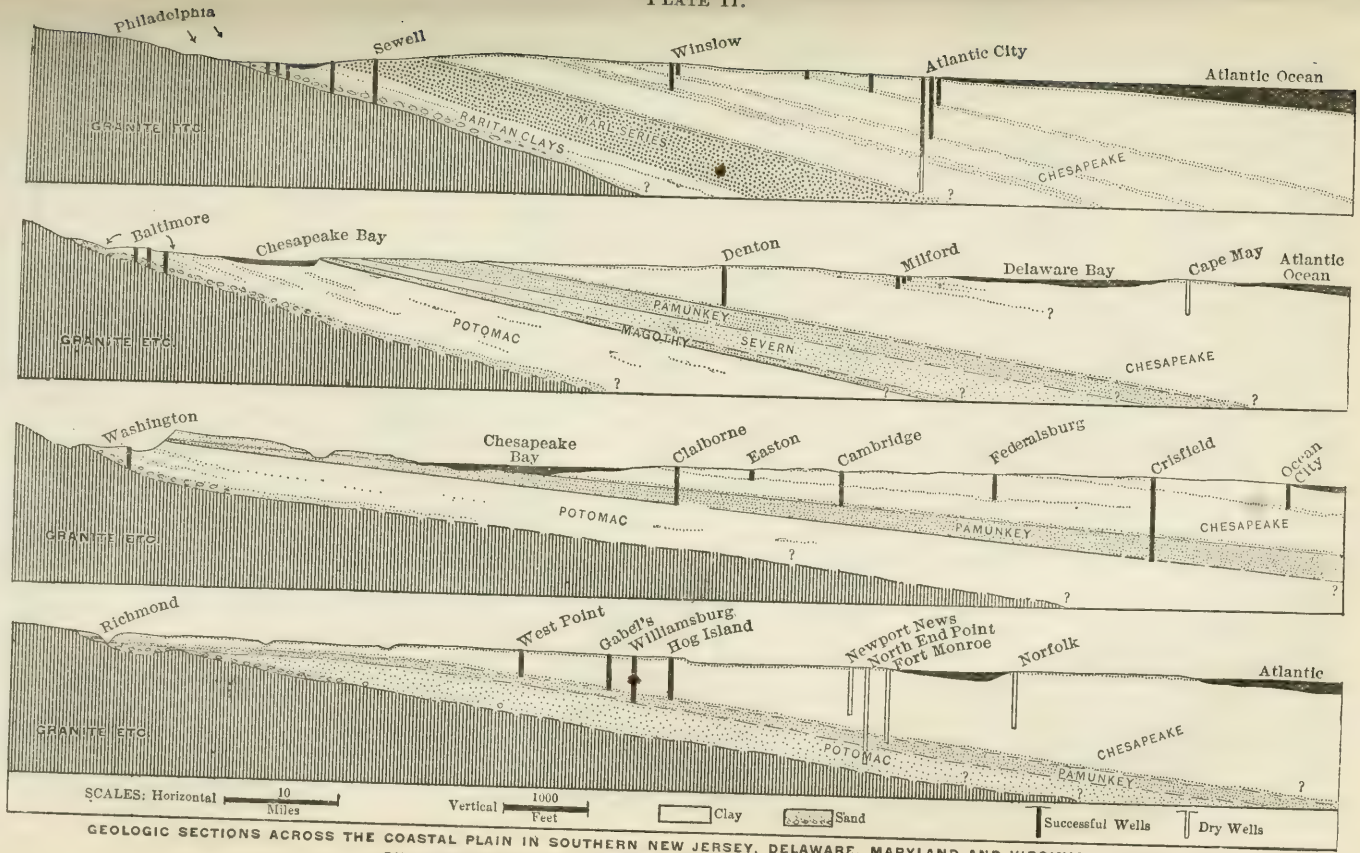
The coastal plain is a belt of country extending along the Atlantic slope from the ocean westward to a width averaging about 100 miles. The surface is, in greater part, smooth or gently rolling, and slopes up gradually to the westward to altitudes of from 300 to 400 feet. It is intersected by a series of bays and long, narrow tidal estuaries of rivers, which are surrounded by low lands to the east, but are flanked by moderately high plateaus and hills as the country rises inland.

The region is underlain by a series of great sheets of unconsolidated deposits, consisting mainly of sands and clays, which lie on an east-sloping floor of older rocks, predominantly granite and gneiss. These deposits constitute a great flat wedge, inclined to the east and southeast, and presenting its thin edge to the west and northwest. The underlying rocks come to the surface along a line extending from New York to Petersburg, passing near Trenton, Philadelphia, Wilmington, Baltimore, Washington, Fredericksburg, and Richmond, and they extend to the westward up the gentle slope of the Piedmont plateau to the base of the Blue Ridge. (See Plates I. and II.)

The great sheets of sedimentary deposits are comprised in a series of formations which are separated by unconformities. The designations of these formations and their general characteristics and ages are as follows:

	Formation.	Characteristics.	Palæontologic position.
Pleistocene.	*Columbia,*	Loams, sands, and gravels on terraces.	
Neocene.	*Lafayette,*	Gravels, sands, and loams,	Pliocene?
	Chesapeake,	Sands, clays, infusorial earth, and marl,	Miocene.
Eocene.	*Pamunkey,*	Glauconitic marls and sands,	Eocene.
Cretaceous.	*Severn,*	Black argillaceous, carbonaceous sands,	Cretaceous.
	Magothy,	White sands and brown sandstones,	Cretaceous.
	Potomac,	Clays, sands, and gravels,	Early Cretaceous.

The Columbia formation occupies terraces along the tidal estuaries, the entire area of the regions east of the Chesapeake bay, and much



GEOLOGIC SECTIONS ACROSS THE COASTAL PLAIN IN SOUTHERN NEW JERSEY, DELAWARE, MARYLAND AND VIRGINIA, LOOKING NORTH. SHOWING RELATIONS OF ARTESIAN WELLS, AND WATER HORIZONS.

of southern New Jersey. It is in greater part a thin sheet of loam merging downward into a gravel bed, and its total thickness rarely exceeds 25 feet. The basal gravel contains water in moderate amount, and is the general source of supply for dug wells in the lower lands of the region. The water is usually of excellent quality, but in thickly settled districts and where surface drainage is defective, it is very subject to contamination.

The Lafayette formation is a thin sheet of gravels and loams which covers the plateau region in Virginia and southern Maryland, but has been removed to the northward. It often has water-bearing gravels at its base, and they are the source of local water supply over wide areas in Virginia and Maryland; but the water is similar to that of the Columbia formation in being subject to surface contamination.

The Chesapeake formation is the most extensive member of the coastal plain series, although it is overlain in greater part by thin sheets of Columbia or Lafayette deposits. Its western edge extends to the crystalline rocks in Virginia, and it thickens rapidly eastward to over 1000 feet at the coast line. (See sections in Plate II.) It consists of a series of beds of sand and clay, which dip gently eastward at a rate of about 25 feet per mile in New Jersey, 15 feet per mile in Maryland and Delaware, and somewhat less in Virginia. These beds rise to the surface in succession on the dip, and extend longitudinally along the coastal plain with a northeast and southwest strike. Clays predominate, and the sands are in greater part of impalpably fine grain. The lower clay members contain a large proportion of diatom remains, and this diatomaceous series is a characteristic member of the formation over its entire area. Interbedded with the clays and fine sands at various horizons, there are beds of coarser water-bearing sands, some of which underlie wide areas and contain much water. These water-bearing beds have been penetrated by the greater number of wells northward, and in New Jersey their stratigraphic and areal distribution and rate of dip have been well established. At the base of the Chesapeake formation there are usually gravelly sands, of greater or less thickness, underlying the diatomaceous clays, and these basal beds are generally water-bearing.

The Pamunkey formation underlies the Chesapeake deposits in Maryland and Virginia, and possibly off to the southeastward in New Jersey and Delaware. It outcrops over the surface in a considerable area southeast of Baltimore and east of Washington, in

Maryland, and about Fredericksburg and on the upper portions of the tide-water depressions in Virginia.

It consists mainly of glauconitic sands and marls which are in greater part fine-grained. It usually contains less water than the Chesapeake sands, but it furnishes a moderate supply to certain artesian wells. The formation, which attains a thickness of about 150 feet in surface-outcrops, appears to thicken gradually to the eastward; but its precise thickness in that direction is not known. Some of its relations are shown in the three lower sections of Plate II.

The Severn formation underlies the Pamunkey formation in Maryland. It is the attenuated southern extension of the great greensand marl series of New Jersey, and terminates at the surface near Washington, D. C. In Maryland, it consists of dark carbonaceous clays which expand in New Jersey to a series of clays overlain by sands, of which several beds are highly glauconitic. It occupies the surface over a wide area in New Jersey, where it is overlain by the Chesapeake formation to the eastward. The thickness in Maryland is not over 75 feet, but in New Jersey, according to a recent estimate by Dr. W. B. Clark, the thickness is 470 feet. In Maryland this formation is not notable as a water-bearer, but in New Jersey many of the sand-beds contain more or less water.

The Magothy formation is a thin local series of coarse white sands with sandstone streaks which underlie the Severn formation in northeastern Maryland. It is undoubtedly a water-bearer, but has not as yet been penetrated by many artesian wells.

The Potomac formation, the great basal member of the coastal plain series, lies directly on the floor of crystalline rocks. In Virginia it consists of sands with interbedded clay sheets and lenses. In Maryland it is a great mass of clays with interbedded fine sands, but it has a basal series of coarse sands and gravels lying on the crystalline rocks. In New Jersey it is the Plastic clay series, or Raritan clay, which contains sand-beds and has a basal series of coarse fragmental deposits. From Trenton to Staten Island it lies on the Newark (Juratrias) sandstones and shales. The thickness of the formation is about 600 feet at Washington, apparently somewhat more than this east of Baltimore, and 350 feet in New Jersey (according to Smock's Clay Report). Its thickness and components out under the later Cretaceous and Tertiary deposits are not known, for it does not appear to have been reached by any of the eastern wells. The gravels and sands of the Potomac formation, notably those lying on the surface of the crystalline rocks, contain a large

supply of water, and a number of wells along the western edge of the coastal plain draw from this horizon. I have great confidence in the extension of this water-stratum eastward, for the crystalline surface is the floor for all the underground drainage, and should be expected to have supplied coarse permeable materials to the immediately adjacent sediments. The water-sheds of this horizon are not extensive in Virginia, owing to the overlap by the later formations, which restrict the outcrop of basal Potomac members to the river-depressions. From Washington to the head of the Chesapeake bay in Maryland it occupies the surface in a belt of considerable width; and although this belt is overlain by Columbia deposits throughout northern Delaware and the western part of southern New Jersey, a large amount of water passes into the formation in this region.

On Certain Conditions Affecting Subterranean Waters.

Before proceeding to the well records and considering their significance, it may be well to point out some conditions which affect the occurrence of subterranean waters. I shall notice only a few points here, and those who are interested in a more extended discussion should read the paper by T. C. Chamberlin, entitled "The Requisite and Qualifying Conditions of Artesian Wells."* The simplest condition of underground waters is shown in Section I. of Fig. 1.

The water enters the coarse stratum at its outcrop over the belt, AA, and flows down the dip, being confined to the coarse stratum by impervious materials above and below. When it is tapped at B it rises to a level approximately as high as AA. On the coastal plain there are probably two other qualifying conditions of greater or less importance. The first is shown in Section II. of the figure. This represents a diminution in the coarseness of the materials down the slope until finally the bed becomes so fine-grained as to be impervious to the water. Under this condition a well at C would afford water, but one at D would not. In case this fining of materials was mainly in irregular beds or in local areas, the districts underlain by water would be restricted to the areas underlain by the coarse materials.

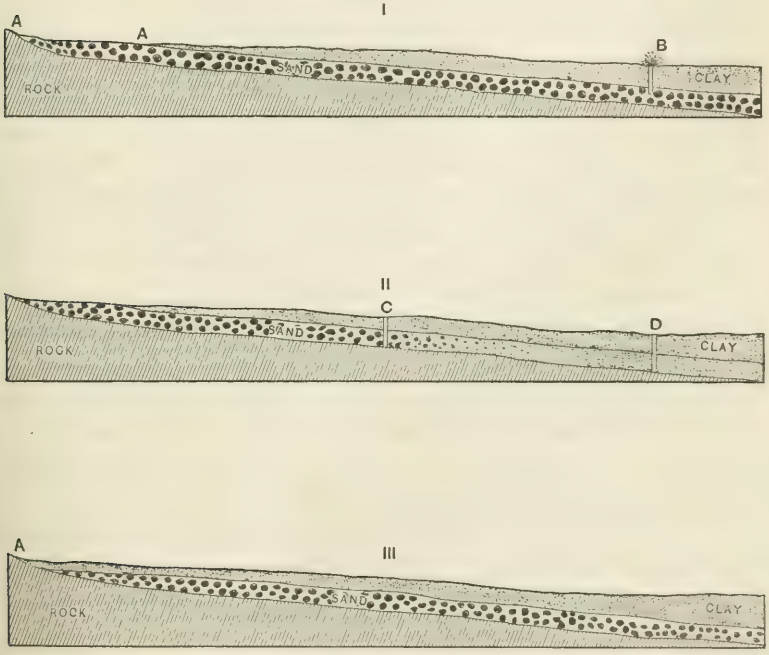
This relation is probable throughout the coastal plain, for the old shore of the deposits was near A, and the fineness of materials in-

* U. S. Geological Survey, J. W. Powell, Director, 5th Annual Report, 1883-84, pp. 125-174.

creases off-shore in most, if not all, of the formations. Beds which contain much water to the westward prove to be entirely fine-grained and barren of water to the east and southeast.

A third condition, which is quite widespread in Virginia, from Fredericksburg to Petersburg, is shown in Section III. This represents an overlap of fine materials across the catchment outcrop of the coarse beds, and this probably greatly diminishes the amount of water in the permeable bed. In Virginia the Pamumkey and Potomac formations are overlapped in this way by the clays of the Chesapeake and the loams of Lafayette and Columbia, and although they are more or less widely bared of these deposits in the river-depressions, they are cut off for considerable widths on the divides.

FIG. 1.



Ideal Sections Illustrating Certain Conditions of Underground Waters.

Section II. is introduced to illustrate the reason why some of the beds which hold water to the westward have not yielded or possibly may not yield water to the eastward.

Section III. has a somewhat similar bearing in practical result, but the same beds, which are widely overlapped by impermeable

deposits in Virginia, are widely bared to the northward, and could receive ample water-supply diagonally down the dip from that direction.

Well-Records.

In Maryland, Virginia and Delaware there are relatively few artesian wells, and the records of those which have been bored do not in every case afford definite information as to the underground geology. I have received borings from several of the wells, but for the others I have only second-hand data. I shall here present all the information which I have secured, not only for its bearing on the present discussion, but to serve for comparison by other observers. Some of the records, which have been published before, are here reproduced because I shall need to review all the evidence obtainable and wish to place all data on record in this paper.

Fort Monroe, Old Point Comfort, Virginia.—A well was started at this locality in 1845, but was abandoned at 168 feet. In 1864, another boring was started and a depth of 906 feet was attained. No water was obtained in these wells, but they both appear to have been badly managed, and the failure of the deeper one may be due to this cause. I believe, however, that it was not sufficiently deep and that the stratum in which water should be expected is at a considerably greater depth. This is a very important consideration, and as it rests mainly on the identification of the geologic horizon of the well-borings, I shall discuss at some length the basis of my opinion. During the boring of the well, samples of the borings were sent to Professor W. B. Rogers, formerly State Geologist of Virginia, who prepared the following descriptive list of the strata penetrated.*

At	
151 feet.	Fine yellowish-gray sand, with fragments of shells.
158 feet.	Buff argillaceous marl.
160 feet.	Fragments of Turritella and Pectens in gray marl mostly. Comminuted shells.
164 feet, 6 inches.	Fragments of Turritella and Pectens in gray marl mostly. Comminuted shells.
166 feet, 10 inches.	More sandy than last. Fragments of Arca.
168 feet, 10 inches.	More sandy than last, with Balanus, etc.
170 feet, 10 inches.	Gray sand, with Arca, etc.
192 feet.	Greenish-gray. Sandy. Turritella.
194 feet.	Lighter gray. Sandy. Turritella.
197 feet.	More clayey than 194 feet.
202 feet.	More clayey than 194 feet. Fragments of Venus, etc.
204 feet.	More clayey than 194 feet. Fragments of Venus, etc.
208 feet.	More clayey than 194 feet. Fragments of Venus, etc., with Perna, Venus. etc.

* This record is published in *The Virginias*, vol. iii., pp. 151–152, and in *Geology of the Virginias*, New York, 1884, pp. 731–736.

At	
390 feet.	Tenacious gray clay, with shelly matter.
400 feet.	Gray sandy clay. Artemæ, Venus, etc.
430 feet.	Yellowish sandy clay.
555 feet.	Light clay, gray brown in color. Foraminiferæ.
558 feet.	Infusorial earth containing many species.
570 feet.	Miocene down to about this depth.
574 feet.	Cetacean bone.
577 feet.	Concretions of sand, marl and shells, composed of loam sand, little pieces of milky and smoky, and sometimes rose, quartz often subangular, Between 577 feet and 583 feet is probably the base of the Miocene. Between 580 feet and 590 feet, shark's teeth, and Galeocerda lateralis.
583 feet.	Sandy clay, brownish.
590 feet.	Very sandy clay. About 5 per cent. sand. Shark's teeth.
604 feet.	Brownish-gray, sandy clay.
628 feet.	Brownish-gray, sandy clay. Less sand.
640 feet.	Greenish-gray, sandy.
670 feet.	Lightest gray clay.
699 feet.	Brownish-gray, sandy clay. Less sand than 604 feet. Same in bed as 628 feet.
784 feet.	Brownish-gray, sandy clay. Same as 604 feet.
815 feet.	Gray sandy clay.
835 feet.	Very coarse sand.
853 feet.	Conglomerate of clay, sand and pebbles.
863 feet.	Rather fine clay with a little sand.
865 feet.	Rather fine clay with a little sand.
870 feet.	Coarse sandy clay, brownish or reddish blotches.
877 feet.	Clay embedding fragments of granite.
885 feet.	Gray clay with occasional fragments of coarse sand.
890 feet.	Gray clay with occasional fragments of coarse sand.
900 feet.	Clay and sand in layers, with some coarse pebbles and reddish blotches.
901 feet.	Reddish mottled clay with quartz pebbles.
902 feet.	Reddish mottled clay with coarse sand.
903 feet.	Reddish mottled clay with coarse sand.
907 feet.	Total depth of boring below parade ground.

This table does not give the thickness of the beds, which is greatly to be regretted. The suggestion of Prof. Rogers that the base of the Miocene is between 577 and 583 feet was due to the supposition that the infusorial beds here characterized the very base of the formation and that the occurrence of shark's teeth indicated the underlying Eocene deposits. It was also suggested that the lower beds, from perhaps 835 feet to the bottom, may possibly be Jurassi-Cretaceous, or the Potomac formation, of later writers. This, I suppose, was based on the presence of reddish mottlings and coarse sand streaks in the lowest clays.

In 1880, Prof. Fontaine, of the University of Virginia, examined the old records of the borings, and an account of these, which he has published, adds materially to our knowledge of the strata penetrated.* The record, as compiled from Fontaine's notes and descriptions of the old records, is as follows:

* "The Artesian Well at Fort Monroe, Va."—*The Virginias*, vol. iii., pp. 18–19, 1882.

No. feet bored.	Thickness of Stratum.	Character of Borings from each Stratum.
0– 30	30	White beach sand.
30 - 40	10	Yellow beach sand.
40– 43	3	Small boulders.
43– 63	20	Mud or marl with shells.
63–155	92	Green sand with shells.
155–162	7	Impervious mud. No shells.
162–167	5	Green sand and shells.
167–185	18	Concrete sand, stones, shells, sharp grit.
185–228	43	Green sand with shells.
228–229	1	Concrete, hard shells and sandstones.
229–239	10	Marl. Shells abundant.
239–240	1	Concrete (shell-rock), hard.
240–248	8	Marl and shells.
248–249	1	Concrete sand.
249–254	5	Marl and shells.
254–256	2	Concrete sand.
256–265	9	Marl and shells.
265–280	15	Marl free from shells.
280–475	195	Stiff clay, very tenacious, of greenish-yellow color which changes to a light gray after exposure to the atmosphere.
475–480	5	Very fine sand.
480–572	92	Stiff clay of light greenish-yellow color, which changes to light gray on exposure.
572		Concrete sand with shark's teeth and cetacean vertebræ.
572–599	27	Clays more sandy.
599		Sands with 10 to 20 per cent. of clay. They were water-bearing, and the water rose above level of parade ground for several days. It was very saline.
599–669	70	Stiff clay, becoming sandy below.
669–830	161	Stiff pure clays, becoming sandy at base.
830–		Clays contain 80 per cent. of sand.
830–851	21	Very sandy clays.
851–		Few pebbles.
851–853	2	?
853–		Conglomerate of clay, sand and small gravel.
853–855	2	?
855–858	3	Hard crust.
858–874	16	Stiff clay.
874–885	11	Stiff clay, variegated in color.
885–890	5	Very dark stratum (clay?).
890–		Change in color.
890–906	16	Sandy clay.

Unfortunately, as Prof. Fontaine observes, this record is so meager that it yields only hints as to horizons. He expresses the opinion

that the Miocene and Eocene have a thickness of at least 800 feet and "that there is little doubt that at a depth of 851 feet the borings were in the Mesozoic (Potomac) strata. This is indicated by the presence of the pebbles" . . . . "and the variegated strata give us another proof that the beds at 874 feet are Mesozoic, for many of the Mesozoic strata along the eastern border are strikingly variegated."

In 1891 I secured an order from the chief engineer of the army, at Washington, and visited Fort Monroe for the purpose of examining the borings. Having spent several years in studying the outcrops of the beds, I believed I could recognize the strata penetrated by the well. It was found, however, that the greater part of the borings were lost, but fortunately a series from 600 feet to 877 feet were remaining. Portions of these were secured and have been studied with considerable care. They are in greater part light-greenish and brownish clays, and may represent the lower members of the Chesapeake (Miocene) formation or possibly the upper portion of the Pamunkey (Eocene). The 877 feet sample is a mixture of fine sand and clay, of dirty gray color, and contains moderately small, angular quartz grains. This is a material often found in the lower portion of the Chesapeake formation in the central part of the coastal plain region. The 865 feet and 811 feet samples were more argillaceous, and quite like typical Chesapeake clay. They were all entirely unlike Potomac clays. At Richmond, and along the western border of the coastal plain belt, the infusorial series is of moderate thickness, and lies in greater part near the base of the formation; but eastward it is now known to thicken, and to overlie a considerable series of dark, heavy clays of various kinds. Whether this relation is due to the overlap of older Miocene deposits or to an ascent of the diatom fauna through the formation is not yet definitely determined but the overlap is probable. The thickness of diatomaceous beds is at least 300 feet eastward. The occurrence of the thin rifts of pebbles mentioned by Rogers and Fontaine is no indication of any particular horizon; for I have observed them occasionally in both Miocene and Eocene at the surface. The local reddish mottling mentioned from 900 feet to 903 feet by Rogers is, it seems to me, of no import whatever; for it might occur restrictedly at any horizon. The sharks' teeth are not distinctive; for remains of Eocene fossils of this character often occur redeposited in the Chesapeake beds. With these facts in mind, it is my opinion that this well may not have penetrated to the base of the Chesapeake formation, although

it may possibly be in the Pamunkey (Eocene). It is thought that it would have penetrated to the Potomac formation at about 1100 feet and to the "granite" at the base of the series, a few hundred feet deeper. As neither the thickness of the Potomac in this region nor the precise depth to its surface is known, I can only give approximate figures. A supply of water should be expected on the surface of the granite and, as before explained, the depth to this at Fort Monroe is quite certainly less than 1500 feet.

About two years ago an allotment was made for a new well at Fort Monroe, and I was consulted by Lieut.-Col. Haines, of the U. S. Engineer Corps, in regard to its prospects. I predicted that water would probably be found between 1300 and 1500 feet and that I should expect its amount and quality to be satisfactory. Owing to some official causes, the boring has not as yet been commenced, and I do not know the plans of the War Department regarding it. A properly bored well at Fort Monroe would throw much-needed light on the underground geology of southeastern Virginia and would probably afford a general basis for water-predictions over a wide area of that region.

North End Point—Back River.—This well is about six miles north-northwest of Fort Monroe and was bored in 1886–'87 to a depth of 1172 feet. It did not yield a supply of water, owing, I believe, to insufficient depth. It is asserted to have reached granite; but no sample was obtained, and the nature of the rock was only surmised from its great hardness. The record of this well, kindly furnished to me by Mr. C. C. Knox, of Norfolk, is as follows:

Depth in feet.	Material.
0– 75	White sand and gravel.
75– 80	Blue clay.
80– 100	White sand.
100– 130	Thin layers of sand and blue clay.
130– 155	Blue or gray sand.
155– 165	Quicksand.
165– 170	Hard white sand.
170– 185	Loose white sand.
185– 187	Black marl.
187– 249	White sand and marl.
249– 250	Rock.
250– 267	Blue clay.
267– 268	Stone.
268– 275	Hard sand.
275– 530	Blue clay with thin marl layers.
530– 600	Hard blue clay.
600– 618	Quicksand.

Depth in feet.	Material.
618– 620	Stone or boulder.
620– 625	White sand.
625– 626	Yellow sand.
626– 695	Clay and sand mixed.
695– 696	Gravel and clay.
696– 740	Hard and soft layers of blue clay.
740– 748	Very hard clay.
748– 913	Soft blue clay.
913– 920	Hard blue clay.
920– 926	Coarse sand.
926– 939	Sandstone.
939– 955	Hard sandstone.
955– 973	Hard sand with few gravels.
973–1000	Hard sandstone.
1000–1007	Sandstone with two veins of gravel and some water.
1007–1015	Alternate layers of sand and sandstone, 3 to 4 feet thick.
1015–1080	Alternate layers of sand and sandstone, 5 to 15 feet thick.
1080–1100	Very hard white sand.
1100–1155	Sandstone.
1155–1160	Red sandstone.
1160–1170	Hard sandstone.
1170–1172	Hardest kind of stone or granite. Here drill rods broke and work was abandoned.

Unfortunately I was unable to examine any of the borings from the well, and from the above notes cannot identify the lower strata with certainty. The Chesapeake and Pamunkey formations certainly extend to 920 feet, and the soft blue clay of 748 feet to 913 feet strongly suggests Chesapeake beds. The sand and sandstones from 920 feet to 1170 feet are almost certainly the representatives of the Potomac formation. It is possible that this well penetrated to and through the Potomac formation to the underlying granite; and if this is the case, the probability of Artesian water supply in the region is not great; for no water will be found in the granite; if, on the other hand, boring stopped in a hard sandstone within the Potomac formation, it is probable that water would be found in gravels and sands which I should expect to find lying on the granites at no great depth below.

Lambert's Point near Norfolk.—A well was bored at Lambert's Point to supply water for the Norfolk and Western Railroad terminal. It attained a depth of 616 feet, and at 606 feet penetrated a water-bearing stratum which yielded 65 gallons per minute. The water was quite saline and the well is regarded as a failure. The railroad company kindly permitted me to examine some of the borings which are as follows:

Test.	Material.
0– 17	Beach sands. Shell fragments.
17– 44	Bluish clay. Shell fragments.
44–183	Gray micaceous sand. Fine. Shell fragments.
183–191	Greenish-gray sandy clay.
191–233	Gray, micaceous sands.
233–264	Fine sand and clay. Greenish-gray. Tough when dry. Shell fragments. Pectes, Venus, Pectans.
264–286	Gray clay.
286–355	Fine gray sand. Shell fragments.
355–377	Gray clay.
377–397	Fine gray sand. Shell fragments.
397–407	Dark-gray clay.
407–526	Gray sand, moderately coarse. Some glauconite.
526–534	Small gravel. Shell fragments.
534–540	Rock. (No sample.)
540–563	Gray clay.
563–564	Rock stratum. Shell fragments.
564–568	Gray clay.
568–603	Rocky strata with thin layers of clay, and shell fragments.
603–606	Small gravel in gray sand. Shell fragments.
606–610	Rocky stratum of 564–568.
610–616	Gray sand, moderately coarse. Micaceous. Oyster shells (?).

From 44 feet down, the well is in the Chesapeake formation.

Newport News.—I learn that a well was bored at Newport News to the depth of about 600 feet but without reaching water. I was unable to secure the well-record; but it undoubtedly did not penetrate to the base of the Chesapeake formation.

In the region northwest of Fort Monroe there are several artesian wells which are yielding large supplies of first-class water. They are at Williamsburg, Hog Island, West Point, Gabel's and near Tarpley's Point. I have obtained information as to their depths in part from Mr. Lewis Woolman, but have not seen any of the borings.

Williamsburg.—This well is 500 feet deep and its bottom is undoubtedly in the Pamunkey, possibly at its base. Its mouth is 65 feet above tide-level.

Hog Island.—This island is in James river, about 6 miles southeast of Williamsburg. The well has a depth of 330 feet and the water-bearing stratum is probably at the base of the Chesapeake formation. The water in the well occurs at a slightly higher horizon than that in the Williamsburg well, unless, possibly, the water in the Williamsburg well is not from the bottom of the boring.

West Point.—There are several wells here, averaging 140 feet in

depth, which yield a large supply of water under considerable pressure. This water probably comes from the base of the Chesapeake formation or possibly a short distance above.

Gabel's.—This well is on York river, 18 miles below West Point or nearly opposite Williamsburg. Its depth is 226 feet and the water-horizon is probably the same as at West Point.

Tarpley's Point.—This is on the Rappahannock river about 25 miles above its mouth. The well is 285 feet deep and its bottom is probably at the base of the Chesapeake formation.

Lancaster Court House.—This well is on the peninsula between the Potomac and Rappahannock rivers, about 8 miles from the bay-shore. The well was sunk to 300 feet and then, I believe, abandoned. Its mouth was about 60 feet above tide-water. The record is as follows:

0– 30	Bright orange sand, moderately coarse.—(*Lafayette*).	
30– 75	Gray sands, moderately coarse, with shell-fragments.	Chesapeake.
75–115	White and yellow sand intermixed, moderately fine.	
115–160	Gray sand, moderately coarse, some glauconite grains, few shells and thin ferruginous crusts.	
160–180	Greenish-gray fine sands, mud with some mica and many shell fragments. Some glauconite.	
180–250	Clay. Light brownish-gray in color, few sandy streaks. Shell fragments.	
250–300	Clay. Light greenish-gray in color.	

Crisfield, Maryland.—This locality is on the "eastern shore" in Somerset county. One well has been sunk to 1006 feet which yields a fair supply of excellent water and another boring is now in progress. The water occurs in beds of loose sand in the Severn formation. Through the kindness of Mr. J. H. Buxton, President of the Crisfield Water Company, I have been supplied with samples of the borings at frequent intervals of the well now in progress. The principal features in the record are as follows:

Depth in feet.	Material.
0– 13	Sands and loams with gravels at base.
13– 100	Buff and gray sandy clays with shell fragments.
100– 110	Dark gray sandy clay. Few shell fragments and small pebbles.
120*	Gray clay.
130	Gray clay. Many shells.
135	Clay. Light greenish-gray.
140	Clay. Shells and siliceous concretions.
147	Sand. Very fine grained. Greenish-gray. Glauconitic.

Depth in feet.	Material.
150	Tough clay. Light greenish gray.
160	Sandy clay. Greenish-gray.
170	Clay. Lead-gray.
177	Glauconitic sands. Loose. Greenish. Shell fragments.
185	Glauconitic sands. Loose. Greenish. Fragments of Perna.
190	Glauconitic sands. Moderately coarse. Shells.
230	Shells in sand.
235	Sand. Dark greenish-gray. Shell fragments.
240	Argillaceous sand. Gray.
240– 268	Clay. Greenish-gray with siliceous concretions and lignite fragments.
268– 270	Siliceous concretion.
280	Clay. Dark-gray.
285	Clay. Tough. Greenish-gray. Shells.
290	Clay. Light-greenish with siliceous concretions.
310	Clay. Dark-olive. Lignite, ferruginous crusts and shell fragments.
340	Clay. Light-greenish.
345	Sand with shell fragments.
350) 360)	Argillaceous sand. Light-green with fragments of Pecten Madisonius.
370	Fine sand. Greenish-gray. Fragments of Pecten Jeffersonius.
380– 385	Clay. Greenish-gray. Sandy below.
390– 420	Sand. Light greenish-gray, moderately fine.
420– 430	Clay. Light greenish-gray.
460	Clay. Very sandy. Light greenish-gray. Shell fragments.
465	Sand. Very fine. Dark greenish-gray. Micaceous.
466– 467	Sandy clay. Dark greenish-gray.
469	Clay. Dark greenish-gray. Shell fragments.
473– 480	Clay. Sandy. Greenish-gray, lighter below.
485	Clay. Bright greenish-gray.
490	Sand. Fine. Greenish-gray.
495– 510	Clay. Light-greenish.
515	Clay. Very light-greenish tint. Very diatomaceous.
520– 530	Similar to 495–510.
535	Clay. Light-greenish. Diatomaceous.
545	Clay. Dark-gray.
555	Clay. Greenish-gray. Diatomaceous.
565	Sand. Argillaceous. Shell fragments. Turritella plebia.
575	Sandy clay. Greenish-gray.
585– 595	Clay. Brownish-gray.
600– 605	Clay. Light-gray. Diatomaceous. Macoma.
610	Sand. Fine. Greenish-gray. Shell fragments. Glauconite.
620	Sand. Fine. Greenish-gray.
630	Clay. Sandy. Shell fragments.

Depth in feet.	Material.
640– 650	Clay. Light-gray. Diatomaceous.
660	Clay. Light brownish-gray. Diatomaceous. Macoma.
670	Clay. Very sandy. Dark greenish-gray.
675– 690	Clay. Light greenish-gray. Diatomaceous.
700	Clay. Gray. Sandy.
710	Clay. Light-gray. Diatomaceous.
720	Sand. Fine. Greenish-gray. Large glauconite grains.
740	Diatomaceous clay. Dark buff.
750	Diatomaceous clay. Light-gray.
760	Diatomaceous clay. Darker.
770	Sand. Fine. Greenish-gray. Some glauconite.
771– 775	Rock, with large grains of glauconite.
780– 850	Argillaceous sand. Dark olive-green, with large proportion of glauconite. Coarser-grained at 820. Finer at 800. Few small quartz pebbles and a shell fragment (oyster) at 810.
855– 960	Clay. Very light greenish-gray, micaceous, and slightly coarser near base.
961– 963	Dark, argillaceous, micaceous sand, containing plant-remains.
965	Sand. Fine, loose, light gray. Some mica.
970	Black clay, with pyrite fragments.

The base of the Chesapeake formation is clearly defined at 770 feet, and it will be noticed that the infusorial beds of this formation have a thickness of 260 feet. The coarse, highly glauconitic sands beginning at 780 feet are typical of the top of the Pamunkey formation, and the siliceous stratum and scattered grains of glauconite characterize the base of the Chesapeake formation in surface outcrops west of Chesapeake Bay. Mr. W. H. Dall has examined the shell-fragments, and says that the species from 60 to 230 feet belong to the St. Mary's fauna of the Chesapeake formation. The dark beds at 961 and 970 are apparently typical Severn deposits, and the water in the first well came from sandy streaks in this formation. The 100 feet of clay at the base of the Pamunkey formation is a noteworthy feature, not exhibited in surface-outcrops westward.

Cambridge.—This place is on Choptank river, about 40 miles north-northwest of Crisfield. There are four wells reaching a depth of 366 feet, and furnishing a large supply of water of excellent quality. The water occurs in sands underlying 100 feet of the diatomaceous clays of the Chesapeake formation, and probably near or at its base.

Federalsburg.—This place is 20 miles northeast of Cambridge, in the central-southern portion of the "Eastern Shore" of Maryland and only a few feet above tide-level. The well is 234 feet deep, and

the water rises to 30 feet above tide-level. From an outlet at 15 feet above tide, it flows 600 gallons an hour. The water-bearing stratum is a loose sand, containing glauconitic grains, and overlain by clays. I have not been able to obtain the record. The horizon is well within the Chesapeake formation, but it cannot be definitely correlated with other localities, unless, possibly, Milford, Delaware.

*Denton, Md.**—This place is 15 miles north-northwest of Federalsburg and near tide-level. The well is 358 feet deep through green clay containing shells. The water is of excellent quality and in fair supply. Judging from the statement of the driller, the well is in green clay all the way down to water; and if this is the case, the water-horizon is probably at the base of the Chesapeake formation. This would indicate an inclination of the beds of 15 feet per mile in this region.

Claiborne, Md.—This well is on the bay-shore, on the western side of the peninsula, nearly opposite Annapolis. The depth is 440 feet, and the water is good and in considerable quantity. The boring is in clay, with black sand-grains from 120 to 380 feet; then "coral-rock" for 40 feet, with plenty of strongly-alkaline water (16 grains to the gallon); then 10 feet of sand and a crust of sand-rock, under which the water occurs. The horizon is probably the Magothy formation; for the boring begins in the basal beds of the Chesapeake formation, and the combined thickness of the Pamunkey and Severn formations is about 400 feet. I have not seen the borings.

Ocean City.—This is a resort on the Atlantic coast of Maryland, a few miles south of the Delaware line. Its well, 256 feet deep, passes through green clays to a sand-streak yielding 130 gallons per minute of good water. This sand-streak is well up in the Chesapeake formation, and probably comes to the surface east of the other wells in Maryland and Delaware.

Easton, Md.—There are six 4-inch wells at Easton, reaching a depth of 110 feet, which yield a moderate supply of water. The horizon is about the same as at Federalsburg.

Salisbury, Md., has an artesian well; but I have no data regarding it.

Clayton, Del.—There is a well at this place only 35 feet deep,

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* For data as to the depth, nature of materials penetrated and results of the Denton, Ocean City, Federalsburg, Cambridge and Claiborne wells, I am indebted to Mr. J. H. K. Shannahan, well-borer, Easton, Md.

which yields 1800 gallons per hour, according to Mr. Woolman. It is in the Chesapeake formation.

Milford, Del.—Three 4-inch wells were bored at this place in 1891, near tide-water level. They are described by Mr. Woolman in the *New Jersey Report* for 1891, pp. 227–229. One at 160 feet furnishes 4000 gallons per hour, and the water rises 8 feet above the surface; another, to 150 feet, furnishes the same amount, with slightly greater pressure; and the third, at 34 feet, furnishes 3000 gallons per hour. They are all in the Chesapeake formation, far above its base.

There are a number of wells along the western edge of the coastal plain, and in greater part they yield large supplies of excellent water.

Baltimore.—In the eastern part of the city, and eastward to Sparrow's Point, there are several borings which extend to the lower part or base of the Potomac formation. I have been able to secure the record of only one of these, which is at the packing establishment of C. H. Pearson & Co., on Locust Point. It is as follows:

Material.	Thickness. Feet.	
Shells,	8	Recent.
Mud,	7	
Mud, with shells and gravel,	7	
Red clay,	8	
White clay,	15	
Sandy white clay,	15	
Sand-rock,	5	
White impervious clay,	15	
Water-bearing white sand,	22	
	102	

There are also in the Baltimore region a number of deep wells in the crystalline rocks, and several of these yield large supplies of fine water. This source of supply is usually a very uncertain one, but in the vicinity of Baltimore it proved remarkably satisfactory.

Washington.—At the gas-works, in the eastern part of Washington, there is a well through 290 feet of Potomac clays and sands to a white quartz gravel, which is probably at the base of the formation. There is another well at the brewery in the eastern part of the city, and there are several in sand-streaks at moderate depths on the shore at St. Elizabeth's asylum.

Barrow.—At the projected town of Barrow, on the west side of the Potomac, 25 miles below Washington, there is a well 195 feet deep, in which a large supply of water was found at 143 feet. Its

record, kindly furnished by R. P. Hughes, of Washington, is as follows:

Depth in feet.	Material.
0– 10	Clay and sand.
10– 20	Gravel.
20– 30	Clay.
30– 44	Sandstone.
44– 45	Coarse sandstone. Water.
45– 57	Sandstone.
57– 58	Bluish sandy clay.
58– 73	Brown clay.
73–113	Bluish sandy clay and fine sand.
113–143	Sandstone, large supply of water.
143–147	Yellow clay.
147–160	Sand, with pebbles.
160–165	Yellow clay.
165–195	Brown clay.

Bowie.—At Bowie Station, 18 miles northeast of Washington, a well is now in process of boring for the Philadelphia, Wilmington, and Baltimore Railroad Company, and I have received samples of the borings through the courtesy of Mr. E. T. Brooks, Superindent. They are as follows:

Depth in feet.	Material.
114	Sand. Light brownish-gray. Fine.
120	Clay. Buff and yellow, mottled.
135	Clay. Buff, with white streakings.
140	Clay. Light buffish-gray.
165	Sandy-clay. Pink and buff variegated, and ochre-yellow.
202	Sand. Fine, loose, buff-gray.
215	Clay. Bright pink, with white streakings, and layer of brown sandstone.
233	Sand. Fine gray, with few buff grains.
265	Sand. Pink-brown, moderately fine.
280	Sand. Light gray-brown.
300	Clay. Rich brown-pink tint.
314	Clay. Light pinkish-buff.
333	Sand. Coarse, gray, with some pink and yellow grains.
335–345	Sandstone. Red-brown, with fragment of lignite.
345–355	Sand. Very fine. Light reddish-brown.
355-384	Sand. Very fine. Buff, containing a large pebble of quartz.

This well is still in the Potomac formation, and probably about 150 to 200 feet from its base, where it is expected that abundant water will be found.

Philadelphia.—In the eastern part of the city, and at various points along and near the Delaware, and to the east and northeast through

New Jersey, there are wells in the Potomac formation (Raritan clay) mainly to its base, which yield water in large amount, and, in greater part, of unexceptionable quality. At Philadelphia, the wells average from 125 to 140 feet; farther east, the depths increase, with the east dip of the floor of the crystalline rocks, to 360 feet below tide at Sewell, and 600 feet at Mount Holly, N. J.

Wells in Southeastern New Jersey.—It is not my purpose to reproduce the records of the many wells in New Jersey, and I have no new data to offer. The New Jersey Geological Reports contain many records, and for the past few years Mr. Woolman has contributed records and discussions of the water-horizons.* I wish only to point out briefly the bearing of the New Jersey evidence on the prospects for the region southward.

The principal water-horizons in southern New Jersey, as shown in the upper section of Plate II., are constant over wide areas. It is well known, that the same geological formations extend southward through Delaware, Maryland, and Virginia; but, there are gradual changes in the stratigraphy, so that, as before explained, the Virginia section does not closely resemble that of New Jersey.

The principal horizons in southern New Jersey are in the sand series, interbedded in the clays of the Chesapeake formation, and they are abundant water-producers. Their extension southward is clearly indicated in several wells of Delaware and eastern Maryland, above described. There are, also, wells in the sands of the marl series, but they are not so noteworthy as the others, and are in beds which do not extend far southward.

General Review and Conclusions.

The success of a number of wells, scattered widely over the middle Atlantic coastal plain region, indicates in itself the strong probability of the existence of subterranean waters throughout the region. It has been shown that the geological relations are favorable to the wide circulation of waters at several horizons, and the vertical positions and general areal distribution of these are approximately indicated. To the northward, there are four prolific water-bearing sand-layers, interbedded in the great clay series of the Chesapeake formation, which furnishes water through the principal wells in southern New Jersey, Delaware, and a portion of Maryland. They have been

* *Geol. Survey of New Jersey, Report* for 1889, pp. 89–99. *Report* for 1890, pp. 269–276. *Report* for 1891, pp. 223–232. *Report* for 1892, pp. 273–311.

quite definitely correlated in the many wells in southern New Jersey, and their dip has been found to be about 26 feet per mile to the southeast. Their continuation southward may be relied on throughout central and southern Delaware, and to at least some distance south of Ocean City in Maryland. The area is approximately indicated by the horizontal broken-line shading on Plate I. The sands do not contain the same amount of water throughout, but, if one horizon is not satisfactory, the distance to the next is not great. The indications are, that to the southward the lower horizons contain less water; but this is suggestion and not established fact. The depth at which the water should be expected at these horizons, within the area indicated, increases gradually to the southeast down the dip, so that, along the ocean shore from Atlantic City southward, the deepest horizon is at a depth averaging about 1000 feet, and it reaches the surface about 25 miles to the northwest. The other horizons are at intervals above, as shown in Plate II. To the south and southwest, in southern Maryland and all of eastern Virginia, there appear to be no water-bearing horizons in the Chesapeake formation, and the successful wells are thought to extend to the bottom of the formation, or very near to it. The well at Lambert's Point reached water within the formation, but it was too saline to be of use. Probably, it was the same saline horizon penetrated at 599 feet in the Fort Monroe well. The success of the wells at Cambridge, West Point, Gabel's, Hog Island, Tarpley's Point, and Denton, indicates a wide extension of water-bearing beds on the surface of the Pamunkey formation; but at Crisfield, Claiborne, and probably Fort Monroe and Williamsburg, these beds yielded no notable amount of water. This would indicate that the water at this horizon does not extend far east of a line connecting Hog Island and Cambridge, and water may be absent in local areas within this district. The depth of the horizon, along the line mentioned, averages about 350 feet, and it gradually decreases up the westward dip. I believe the horizon may be relied on for water, at moderate depths, in eastern Virginia over the area shown by dottings on Plate I.

There is probably water at the base of the Chesapeake formation throughout southern New Jersey; but I do not know of any well which penetrates to it. Within the Pamunkey formation there are probably no water-bearing beds except, possibly, in a restricted area about Williamsburg. As the Pamunkey formation is mainly sand in the surface-outcrops, it was expected that extensive sheets of water-

bearing beds would be found throughout its area; but this has not proved to be the case.

In the Severn formation in Maryland there is probably no water that can be depended on; but the experience of the final well at Crisfield indicates that it may sometimes occur. In the coarse, loose sands of the Magothy formation water may be expected, throughout the area of the "eastern shore" of Maryland, lying between Chesapeake Bay and the Delaware line. They probably yield the water at Claiborne, and will be found to be satisfactory water-producers at Crisfield.

In the Potomac formation there are undoubtedly many water-horizons, and there is almost a certainty that the basal beds lying on the east sloping crystalline floor are water-bearing over a wide area. In eastern Virginia I should expect water throughout the coarser sandy members which occur at various horizons in the formation. Northward, the clays predominate to such an extent that there is less chance for water above, but in the coarse sands and gravels of the basal beds there is an abundant supply. These basal beds are bare over a wide extent in northern Virginia and Maryland and take in a very great amount of water which is generally retained by the clays above, and the crystalline rocks below. The wells sunk to this horizon at Baltimore and Philadelphia have been very successful; and there is every probability that the waters extend far to the eastward down the slope of the crystalline floor. It is to be expected that finally the basal materials may become too fine-grained to be water-bearing, as explained on page 378; but at present we have no knowledge as to where this takes place. The depth of the crystalline floor to the eastward is not definitely known, but as shown in the four sections in Plate II., it is thought to be not much over 1500 feet along the ocean-shore in Virginia, and somewhat deeper to the northward. The prospects for water in underlying crystalline rocks are very slight, and I should never advise sinking into them out on the coastal plain.

Prospects in the Several Districts.—In the vicinity of Norfolk, Fort Monroe, Cape Charles and Newport News, water will probably be found on the crystalline floor at about 1500 feet below the surface. There is also some possibility for water in the Pamunkey formation; but the North End Point and Fort Monroe wells indicate that this is improbable. It may be that the materials of the water-horizon westward and northward, are all too fine-grained or too much clogged with fine silts and clays to carry water in this district; but I believe

the chance is worth taking. The failure of Fort Monroe and North End Point wells is of course somewhat discouraging; but, as I have shown above, their evidence is not conclusive.

In the peninsula region of eastern Virginia and Maryland, west of Chesapeake Bay, water may be expected with a fair degree of certainty at the base of the Chesapeake beds, over the area shown by dottings on Plate I. The depths will vary from 100 feet, west of the longitude of West Point, to about 300 to 400 feet towards Chesapeake bay. This is the horizon of successful wells at West Point, Hog Island, Gabel's and Tarpley's point and will eventually prove to be an important water horizon. There is another chance in this region, from 100 to 150 feet below this horizon, down in the Pamunkey formation, as indicated by the success of the well to 440 feet below tide at Williamsburg. Below this, water may be expected again in the Potomac sands, particularly on the crystalline floor; but this is probably several hundred feet below the Pamunkey formation.

Along the western edge of the coastal plain, from Petersburg to Staten Island, water may be obtained from the basal members of the Potomac formation at moderate depths, as indicated by the successful wells of Baltimore, Philadelphia, etc.

On the "eastern shore" of Maryland there are many favorable prospects. From Federalsburg to Salisbury and Ocean City there are water-bearing sands in the Chesapeake formation at from 200 to 300 feet below the surface, and possibly others at greater depths. About Easton they are at 100 feet. There are waters on or near the surface of the Pamunkey sands in the area indicated by dottings on Plate I., which are reached by the Cambridge well at 360 feet and the Denton well (probably) at 350 feet. The next water-horizon is about 400 feet below the top of the Pamunkey, in the coarse sands of the Magothy formation. This water may be expected over the entire region south of Chester river, as confirmed by its probable occurrence at Claiborne at 440 feet.

In the northern portion of the peninsula there is almost a certainty, and to the southward a probability of finding water in the Potomac gravels and sands on the surface of the crystalline rocks.

In Delaware the conditions are similar to those in New Jersey, and the several water-horizons in the Chesapeake formation extend across the State. The lowest is at the surface a couple of miles north of Clayton and extends down the dip approximately southeastward to 1000 feet at the southeastern corner of the State. In the central

and southern sections, there is probably water also at the base of the Chesapeake formation and in the Pamunkey formation. In the northern section of Delaware the waters at the base of the Potomac will be found at moderate depth, and probably they extend far to the southward. At the southern margin of the State it is estimated that the depth of the basal Potomac beds would be about 1600 feet.

THE STRUCTURE OF THE RICHMOND COAL-BASIN.

BY E. J. SCHMITZ, NEW YORK CITY.

(Bridgeport Meeting, October, 1894.)

THE mining of the Triassic coals of the Richmond basin has been carried on, upon a larger or smaller scale, for more than a hundred years.

Notwithstanding the close proximity of the field to a large city and to tide-water, assuring both a good home-market and cheap transportation, and notwithstanding the good quality of the fuel and the great thickness of the principal bed developed and worked, operations in this field have been conducted with but limited success.

The basin comprises about 189 square miles, being from 24 to 31½ miles in length by 5½ to 10 miles in width. Not much more than one-tenth of this area has been explored in a practical and efficient way, and not over 1200 acres have been worked near and along the outcrops or edges of the coal.

The practical mistakes which have been committed undoubtedly in a number of the mine-operations of the past, do not alone explain the unsatisfactory success and progress of the industry of this field; nor can it be ascribed to the lack of local enterprise. Other reasons have to be looked for; and, as a principal one, the irregularity and the faulted conditions of the coal deposits must be pointed out. As a second cause, the indifference of the State government, which leaves the development of the field entirely to private enterprise, instead of aiding it by systematical and practical geological investigations, deserves to be mentioned. Nothing has been done by the State since the excellent work done more than fifty years ago by Professor Rogers, who could not at that time enter into the details of such

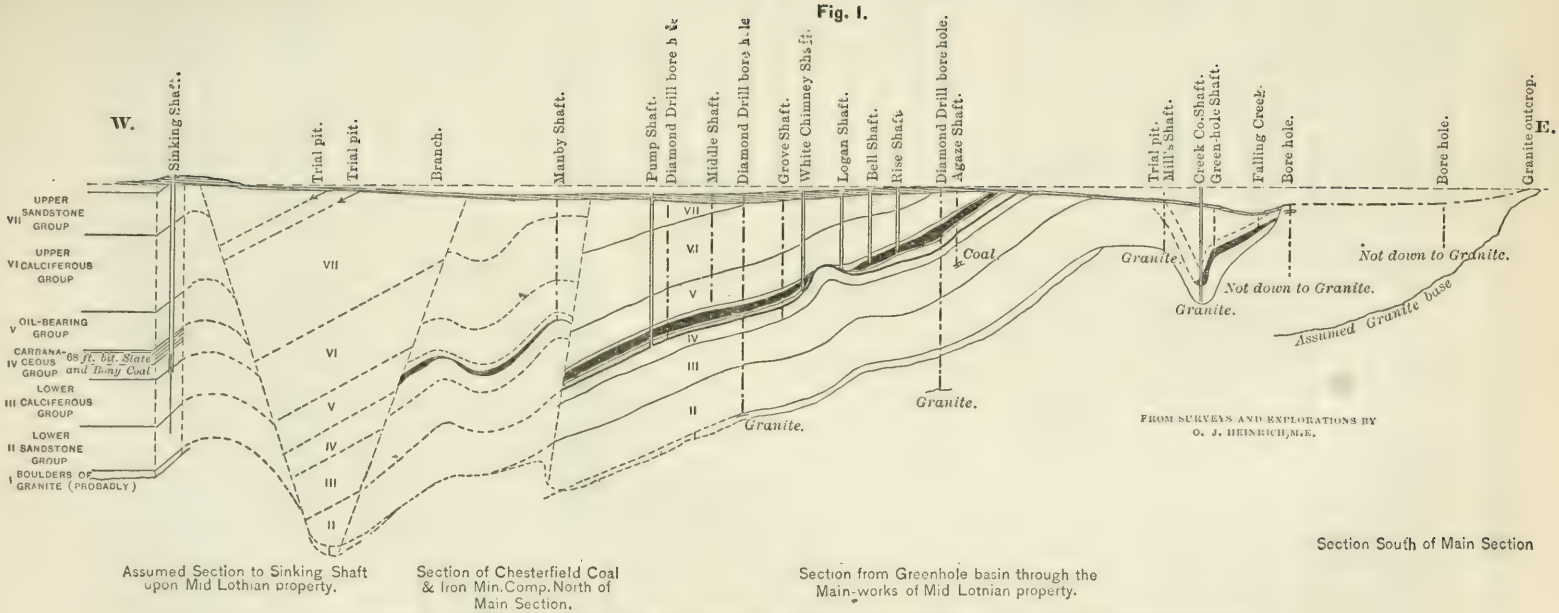
geological researches as the field requires. The paper of the late Oswald J. Heinrich, on "The Mesozoic Formation in Virginia" (*Trans.*, vi., 227), published sixteen years ago, based upon a number of practical investigations by the means of shafts and bore-holes, and dealing with the factors most vital to ultimate commercial success, is the best private monograph known to me. This paper gives the first complete section of the Triassic measures of the basin, and discloses also the faulty conditions of the field by a section across the strike at the Midlothian mines, from the outcrops to the Sinking shaft of the Midlothian colliery. Mr. Heinrich had evidently made detailed studies of the basin, and had conducted explorations and mining operations at the Midlothian mines for several years. My own examination, upon which the present paper is based, occupied but five weeks, and extended principally over the southern part of the field, near and west of the old Clover Hill workings. Since, however, my observations and conclusions differ in some respects from those at which Mr. Heinrich arrived, I lay the same before the Institute with the intention of throwing additional light on the subject, and of aiding in future developments.

The cross-section published by Heinrich (reduced and reproduced in Fig. 1) shows the measures for about $7\frac{1}{2}$ miles across the strike from the outcrop-line, and divides them into five distinct parts formed by four faults or vertical dislocations (upthrows). In the second division from the outcrops, the main seam is developed in great thickness, at moderate inclination and almost regular for about 2000 feet, while the same is indicated as thinning out in the third, and as changing into a carbonaceous slate stratum in the fifth division. His vertical geological section for the Midlothian district reaches down to the granite floor (1500 feet), and locates the carbonaceous group about 550 to 600 feet from the bottom of the Triassic measures, and about 900 feet from the surface at the most western point, the sinking-shaft.

The Clover Hill district, like the Midlothian district, is located along the eastern edge of the basin, but about 15 to 16 miles further southwest.

The Stratigraphy of the Clover Hill District.—Figs. 2 and 3 show approximately the outcrop-line of the Azoic base (the granite line), the outcrop-line of the carbonaceous group, and the lines of the so-called "Garret trouble," which represents a regularly developed fault or squeeze, parallel to the general strike of the whole formation, that is, about N.N.E. to S.S.W., or parallel to the general trend

Fig. I.



GEOLOGICAL SECTION THROUGH THE MID LOTHIAN COLLIERY FROM GREENHOLE BASIN TO SINKING SHAFT.

Scale. 1000 Feet = 1 inch

Section given by O. J. Heinrich (reduced from the plate accompanying his paper, *Trans.*, A. I. M. E., vol. vi.).

of the Appalachian chain in this latitude. This squeeze, which is indicated at the surface by a slight elevation of the ground, has been found regular for the entire length of the Clover Hill district, say for about 2½ to 3 miles, and is reported to have been traced from the Clover Hill district to Midlothian. It appears a downthrow of the carbonaceous group and the Triassic measures in general, different from the faults illustrated in Mr. Heinrich's section (Fig. 1), for the Midlothian district further N.E.

The edge of the basin has been prospected all along in the Clover Hill district for 4 miles or more (and at intervals also to Midlothian); but successful operations have been conducted only on a strip of about 2½ to 3 miles along the strike, from some distance north of the Cox shafts to a point south of the Raccoon slopes, and from the outcrop-line to some distance west of the Garret trouble, within about 1500 feet, or a little more, from the outcrop.

A flourishing mining industry existed in this district before (and, I think, to some extent after) the late war, upon the coal along the outcrops east of the Garret trouble, and to some distance west of it. Thus, it is reported that the Cox shafts, which were sunk several hundred feet west of the trouble, worked the ground for 400 to 600 feet further west, and also at the Bright Hope and other shafts mining was done west of the squeeze. At present, operations are restricted to a patch of forgotten ground of the outcrop, mined in the locality of the New Slopes No. 1, No. 2 and No. 3; but work is now in progress preparatory to the clearing of the Cox shafts and mining west of the Garret trouble.

There exist in the Clover Hill district three distinct beds of coal, as follows:

	Feet.
I. Top seam,	3 to 4.5
Interval,	10 to 30
II. Main seam,	7 to 20
Interval,	40 to 50
III. Bottom seam (reported),	4 to 6

The coals of the first two seams are hard bituminous coals of square cleavage and fracture, nearing the "splint" in hardness, and showing coking-qualities.

The upper seam carries the cleaner and harder coals, while the main seam sometimes contains considerable pyrites, and the third seam is reported often to contain pyrites in such quantities (especially in its lower bench) that its coals are rejected by the trade.

The main seam is worked in the New Slopes No. 1, No. 2 and No. 3, of which No. 1 has been driven on the main seam, intersecting it a short distance from the mouth of the slope, and following the inclination from this point. The top seam has been proved in the roof from No. 2 and No. 3.

In Fig. 4, the main seam is seen, regular to within a short distance from point A. The seam varies in thickness from 7 to 16 feet, containing from 6 to 13 feet of coal. About 400 feet from the mouth of the slope we have the following section:

Roof, Sandstone and Slate.	Feet.	Inches.		
Coal, . . .	2	...	with 1 slate parting	
Slate, . . .	...	10		Total, 3 feet 11 inches, generally left in roof.
Coal, . . .	...	9		
Slate, . . .	...	4		
Coal, . . .	3	10		
Black slate, . .	...	4		
Coal, . . .	1	1		
Black slate, . .	...	1		
Coal, . . .	1	1		Total, 11 feet, 8 inches.
Bony coal, . .	...	3		Coal, 10 feet, 10 inches.
Coal, . . .	2	1		
Black slate, . .	...	1		
Coal, . . .	1	10		
Whitish clay slate,	...	1		
Coal, . . .	...	11		
Total thickness, .	15	7		

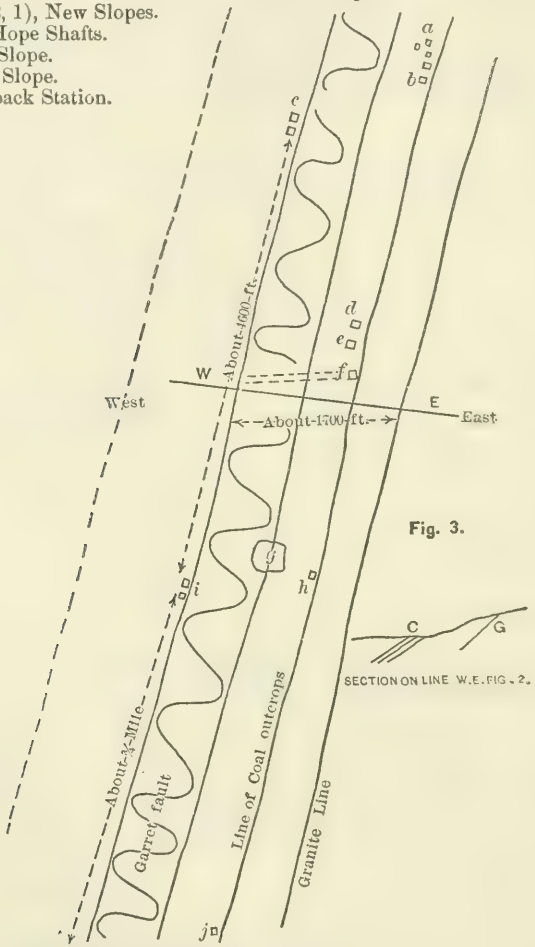
In the workings of the Bright Hope shafts and the Raccoon slopes, which I could not examine, the main seam is reported to me as 20 feet 2 inches to 20 feet 4 inches thick. A short distance above point A, Fig. 4, commences a squeeze of the main seam in the manner shown in Fig. 5. The thickness of the seam is gradually reduced to nothing, so that the hanging-wall rests directly upon the foot-wall at point A.

Point A marks the eastern line of the so-called Garret trouble, which, as appears from these sketches, is not a fault in the common acceptation of this term. From A westward the coal is mostly missing to the western line of the Garret trouble, which had not been reached yet in Slope No. 1, at the time of my examination (See Fig. 6). But in a number of cases we find the hanging-wall separating again from the foot-wall, and the intervening space of a few feet is then generally filled by a pocket of coal.

The last 25 to 30 feet of the first steep incline above B furnishes an instance. This pocket of coal is cut out again at B in the manner shown in Fig. 7. A second pocket of coal is found between C and D, continuing with small interruptions for about 70 feet.

c, Cox's Shafts.
d, *e*, *f* (3, 2, 1), New Slopes.
i, Bright Hope Shafts.
h, Beaver Slope.
j, Raccoon Slope.
g, Winterpack Station.

Fig. 2.

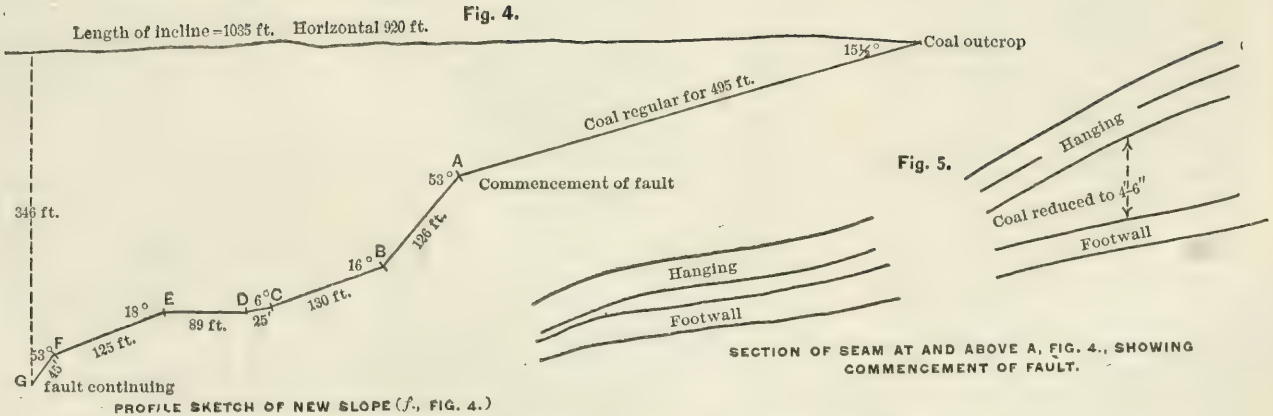


SKETCH-PLAN OF CLOVER HILL DISTRICT.

Fig. 3.

SECTION ON LINE W.E. FIG. 2.

At E we have a third pocket of coal (see Fig. 8), which has partly been changed into coke. Also, from E downward to the face of the slope, we have occasionally thin deposits of coal, which are mostly converted into coke.



Sketches from Clover Hill Workings, Richmond Field, Va.

Fig. 9 shows the section at F, that is, at the point of commencement of the last steep inclination.

The foot-wall of the seam is generally a pretty hard, siliceous and bituminous rock of dark to black color, but changes its character in the lower portion of the slope to a rounded, dark gray, pretty fine-grained and hard, and slightly calcareous concretion or band of small thickness. The hanging-wall is a fine-grained, lighter, siliceous and micaceous sandstone. The mica found in both floor and roof is mostly in fine silvery scales (muscovite).

Fig. 6 is a sketch taken about 95 feet above point B, Fig. 4.

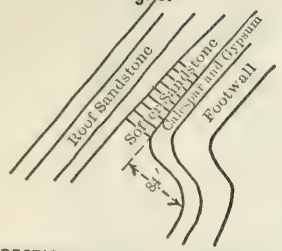
Fig. 10 represents a section noticed at several points of the fault, where the dip of the formation changes; and Fig. 11 represents a section obtained in a trial shaft on Honey Hill, 2 miles N.E. of Slope No. 1.

If we consider the form and details of the sections shown in Figs. 5, 7, 8, 9 and 11, it becomes at once evident, that the deposition of the coal in pocket-form or as an interrupted seam cannot have been due to secondary disturbances, but that the coal must have been deposited originally upon an uneven floor, which, as most of these sections indicate, was subjected to an upward movement during the deposition of the coal-beds. This upward movement seems to have been limited to, or to have been more pronounced at, certain zones, namely, those of the present squeezes or troubles, as the coal is regular to the east of the Garret squeeze. And, as this squeeze or trouble has a strike regular and common with the strike of the Appalachian chain, we are justified in supposing that this and similar fault-zones or ridges have been caused by the forces which raised the whole Appalachian chain, and represent smaller folding ridges or anticlinals of that chain.

Moreover, as the forming of the Appalachian chain was not due to a sudden movement, but must have occupied a considerable geological time-interval after the Permo-Carboniferous age, we may with great probability assume that the Triassic measures of the Richmond field were deposited upon the uneven floor of the Azoic rocks during the later period of the construction of the Appalachian chain, and that this uneven floor was under the influence, and took some part in the movements, of that chain.

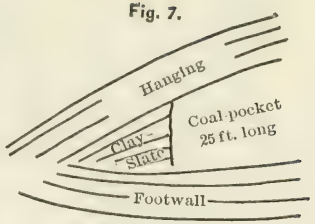
The irregular, or, rather, non-persistent forms of the Richmond coal-deposits must have been original, not only on account of the proofs already mentioned, but also for the reason that a later inter-

Fig. 6.



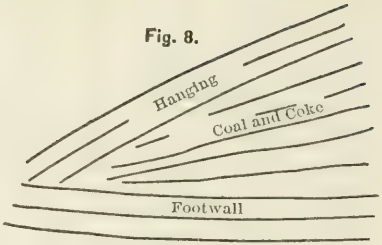
SECTION 95 FT. ABOVE B, FIG. 4

Fig. 7.



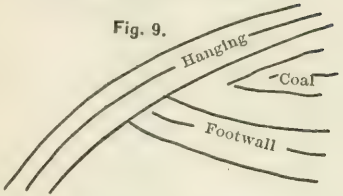
SECTION AT B, FIG. 4.

Fig. 8.



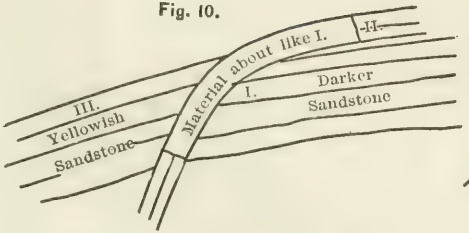
SECTION AT E, FIG. 4.

Fig. 9.



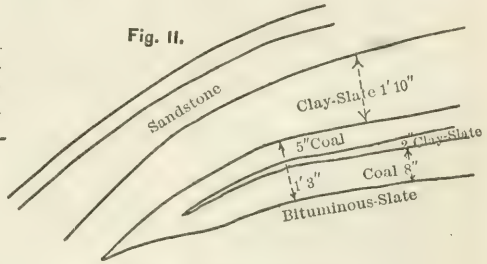
SECTION AT F, FIG. 4.
STEEP INCLINATION SHOWING COMMENCING.

Fig. 10.



SECTION OBSERVED AT SEVERAL POINTS.

Fig. 11.



SECTION ON HONEY HILL

Sections in Clover Hill Workings, Richmond Field, Va.

ruption or destruction of parts of the coal-seams, as, for instance, by plutonic action, is excluded. Such action—apart from dislocations or disturbances—would have either entirely consumed parts of the beds or converted the coal into coke. So far as disturbances come into consideration, I must call attention to the fact that the Garret trouble is too regular and continuous to have been caused by plutonic agencies. Nor could such a process have consumed the entire section of the seam without also destroying the walls, or without leaving other signs of its action, such as residue or ashes.

The conversion into coke, which is shown in several localities of the Garret trouble, and frequently in other localities in the field, must be due, however, to igneous action, the signs of which are also noticed in other forms, such as vertical upthrows of the measures and dike-openings, etc. But such action can have only been local, and must have taken place after the entire deposition of the coal-beds and the Triassic measures, and when the field or basin, as a whole, was already in its present stratigraphical form.

We may conclude with great probability that the formation of the Richmond basin and its coals took place in an estuary, inland lake or swamp, which was directly or indirectly connected with the ocean, and the floor of which was alternately partially raised above ocean-level for considerable periods during the deposition of the carbonaceous strata, and entirely submerged again by the movements of the Appalachian system.

This view is confirmed by the observations reported to me concerning the relations of the three principal coal-beds in the Garret squeeze of the old Clover Hill workings, namely, that the bottom seam was much more regular than the main seam, but was never cut out entirely; that it was smallest in the downthrows of the squeeze where the coal is mostly cut out in the main seam; and that the top seam followed the irregularities of the main seam, and was cut out in the downthrows with the main seam.

It has also been observed in the old workings that the bottom seam has been reduced to nothing in several localities outside of the Garret trouble, where the main seam was found to continue regular.

Of the two sections not yet mentioned, Fig. 6 seems to indicate a partial subsequent filling (Tertiary?) of the narrow space between the walls of the bed, while Fig. 10 indicates a regular deposition of

the different strata, one after the other, but under changing conditions.

Across the Basin from the Clover Hill District.—It is about 5 to 5½ miles from the eastern edge of the basin at the Clover Hill district to the western outcrop. The country is low, slightly rolling, and varies about 50 feet in elevation. The surface is almost entirely covered with drift and Tertiary deposits. Some flat-lying deposits of clay and soft sandstones, which can be observed, must be classified as later deposits, most likely Tertiary. The dip of the Triassic measures is mostly concealed by these layers.

The Azoic rocks of the western edge of the field and the lower

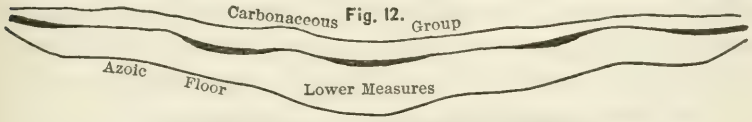


Fig. 12.

IMAGINARY SECTION OF COAL-FIELD AS DEPOSITED.

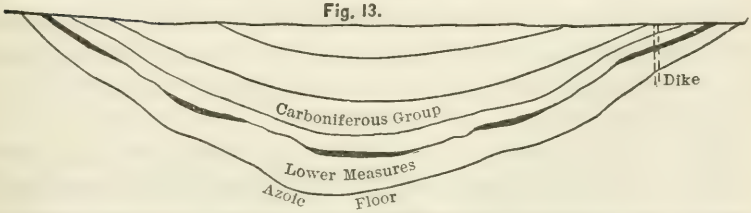


Fig. 13.

IMAGINARY SECTION OF COAL-FIELD AFTER SUBSEQUENT ELEVATION OF AZOIC FLOOR.

Sections Illustrating Supposed Formation of Richmond Coal-Field, Va.

Triassic rocks are well exposed in high inclined position, and as continuous low ridges near the Appomatox river, and further N.W. from the river. The researches for coal which were made in this locality in early years seem not to have been successful, as only very thin and small pockets have been opened there.

Turning from these outcrops eastward, one can observe the Triassic measures with a moderate eastern dip for about ¾ mile, and then changing to a slight western dip. It is doubtful whether this western dip is continuous from here to the eastern edge of the field, or is only the result of a local anticlinal; but probably the latter is the case.

Hypothesis of the Construction of the Richmond Basin and Its Present Form.—As already observed, much speaks for the deposi-

tion of the Triassic rocks and their coal-beds during the period of construction of the Appalachian chain, while the Azoic floor was affected by the upward and folding movements of this chain. Such movements would involve, of course, an irregular and pocket-like growth and deposition of the coal flora, varying for different seams, as well as for the area of each seam. I have tried to illustrate this deposition in Fig. 12.

Fig. 13 is intended as an ideal section of the field at the end of the above-mentioned period with the edges of the Azoic floor highly inclined. It is, of course, not to be inferred that the basin has such a regular and undisturbed form as is shown in this figure, with coal-bearing zones interchanging regularly with barren ones. The real section of the basin at the south end will most probably contain also vertical dislocations or faults, such as Mr. Heinrich's section indicates for the Midlothian district, and also faults and squeezes at right angles to the strike, and otherwise.

The secondary disturbances, caused by plutonic action and represented by dike-openings and dikes and the coking of the coal in the vicinity of such dikes, or in localities subjected to such influence, must have taken place mainly near or after the end of the Tertiary period, since Tertiary deposits have been removed from the surface at these dike-openings.

The Practical Value of the Field as a Coal-Producer.—The practical value of the field as a coal-producer depends, of course, principally upon the coal-areas which can be opened and made accessible to the shafts and slopes of each of the several mines; and it seems to me that the chances are better nearer to the center than to the edges of the field. It is to be regretted that no systematical boring-operations have ever been made across the field to the center of the basin.

It is hardly necessary to add that my investigations and deductions bear also against Mr. Heinrich's theory of a continuous connection for all Mesozoic deposits along the Atlantic coast.

CLOSE SIZING BEFORE JIGGING.

BY ROBERT H. RICHARDS, BOSTON, MASS.

(Virginia Beach Meeting, February, 1894.)

THE extent to which sizing by sieves should be carried, as a preliminary to the separation, by jigging, of minerals of different specific gravities, has been a matter of controversy for many years. The subject has been investigated by several authorities, yet the ground does not seem to have been completely covered, nor are the questions involved entirely settled. For my present purpose I shall refer to but three investigators—Rittinger, Munroe, and Hoppe.

In seeking additional light, I have gone over part of the old ground which has been considered satisfactorily settled; and since these preliminary tests have thrown light on some points, they have been included in this paper.

In the investigations here described, I have confined myself, for several reasons, wholly to small sizes—grains of 0.1 inch in diameter and less. Rittinger's work was mainly done upon larger sizes, and there is much need among millmen of information concerning the smaller sizes. Moreover, these sizes brought the investigation within the means at my disposal.

The laws that have been claimed as the laws of jigging by the several authorities are:

1. The law of equal-settling particles.
2. The law of interstitial currents.
3. The law of acceleration.
4. The law of suction.

The first of these has been considered by investigators, generally, to be the most important of all, and the larger part of the work of jigging is thought to be governed by it. But that it does not cover the whole of jigging is clear to all; and to account for the increased efficiency which we may call the extra jigging-catch, the other three laws have been advanced.

The investigation described in this paper was undertaken to determine, as far as possible, to what extent each of the four laws con-

tributes towards the results of jigging. The discussion will be taken up in the order indicated above.

I.—Equal-Settling Particles.

If we drop into a deep vessel filled with water an unsized product containing particles, say, of galena and quartz, which have been thoroughly wetted, and all of which will pass through a limiting-sieve of 10 meshes to the linear inch, we shall find that, after the short interval of acceleration, each particle will fall at its maximum velocity towards the bottom. These velocities, if the particles do not interfere with each other, and if they are all cubes or spheres, will depend upon two things, namely, the specific gravity, and the size of the grains. Of two particles of equal size, but different specific gravity, the heavier will fall the faster. Of two particles of like specific gravity, but different diameter, the larger will fall the faster. Evidently, therefore, any larger particle of the quartz will have the same velocity as a certain smaller-sized grain of galena. These two grains are said to be equal-settling particles.

Rittinger gives, in his treatise, four formulas to represent the relation between diameter of grains and rate of falling in water for irregular-shaped grains:

$$V = 2.73\sqrt{D(\delta - 1)}, \text{ for roundish grains.}$$
$$V = 2.44\sqrt{D(\delta - 1)}, \text{ for average grains.}$$
$$V = 2.37\sqrt{D(\delta - 1)}, \text{ for long grains.}$$
$$V = 1.92\sqrt{D(\delta - 1)}, \text{ for flat grains.}$$

In which V is the velocity in meters per second; D, the diameter of particles in meters, and δ the specific gravity of the minerals.

From the formula for the average, he computes the ratio of the diameters of quartz and galena particles that will be equal-settling in water. Taking the specific gravities, as determined by me, viz., for quartz, 2.640, and for galena, 7.586, and using his formula for the average grain, we should have,

$$\text{For quartz, } V^2 = 5.9536\, D_1 \times 1.64,$$
$$\text{For galena, } V^2 = 5.9536\, D_2 \times 6.586.$$

For equal-settling particles, we equate the two values of V^2, and deduce:

$$\frac{D_1}{D_2} = \frac{6.586}{1.64} = 4.015.$$

That is to say, the particle of quartz will have a diameter four times as large as the galena.

The minerals employed in the investigation described in this paper are given in Table I., with their specific gravity, and the multiplier which gives the diameter of the equal-settling grain of quartz, computed from Rittinger's formula for average grains.

TABLE I.

Mineral.	Specific gravity.	Multiplier for equal-settling quartz.
Anthracite,	1.473	.288
Quartz,	2.640	1.00
Epidote,	3.380	1.45
Sphalerite,	4.046	1.85
Pyrrhotite,	4.508	2.14
Magnetite,	4.987	2.43
Chalcocite,	5.334	2.64
Arsenopyrite,	5.627	2.82
Cassiterite,	6.261	3.22
Antimony (artificial),	6.706	3.48
Wolframite,	6.937	3.64
Galena (cubic),	7.586	4.01
Copper (Lake Superior),	8.479	4.56

Rittinger (1866) considers the law of equal-settling particles to be the law of jigging, and infers in consequence, that a jig should be fed with particles of galena and quartz, for example, that are in no case equal-settling. To prepare the sands for a series of jigs, then, there will be required a series of sieves graded according to the diameters of their meshes. The ratio for the diameters of these meshes for jigging quartz and galena, for example, will be 1 : 4, and for the other minerals, as the multipliers given in Table I.

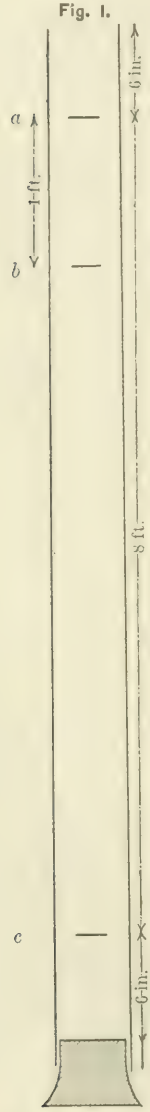
Upon this theory, the continental system of close sizing before jigging has been developed.

With a view to studying this question, experiments were instituted upon the rates of falling in water, and for this purpose, a sorting-tube of the size indicated in Fig. 1, and marked at *a*, *b* and *c*, was mounted vertically and filled with water to the top. Each mineral was sized by a series of sieves, the values of the holes in which were obtained with great care.

Table II. shows the manner in which the size of the grains delivered upon each sieve was computed. For convenience the sieves were arranged in a nest, see Fig. 2, so that when the nest was shaken, a sample of mineral placed on the upper sieve delivered all its dif-

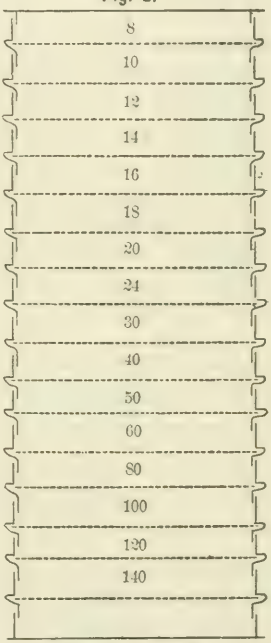
ferent sizes, each upon its own sieve. Fall velocities were obtained by allowing a number, perhaps 50, of grains of the same size to fall

Fig. 1.



Sorting-tube.

Fig. 2.



Nests of sieves.

the distance of 8 feet from a to c in the sorting-tube, and noting the period required for the passage of the fastest grain, and also the time required for approximately 90 per cent. of the grains to pass. This

proportion of 90 per cent. was preferred to the observation of the slowest grain, because the slowest often lags an indefinite distance behind. The results of these tests are given for fastest grains in Table III., for slowest, in Table IV.

TABLE II.—*Computation of Grain-Sizes.*

MESH.	Meshes per Inch One Way.	Diameter of Wire. In.	Net Linear Size of Hole. In.	Meshes per Inch the Other Way.	Diameter of Wire. In.	Net Linear Size of Hole. In.	Av. Linear Size of Hole. In.	Av. of Size and One Above It.
3	3.	.0503	.2830	3.	.0503	.2830	.2830	.....
4	3.75	.0471	.2196	$4\frac{1}{16}$	.0447	.1977	.2087	.2458
5	5.25	.0404	.1500	5.	.0404	.1596	.1548	.1817
6	6.	.0352	.1315	5.9	.0365	.1330	.1322	.1435
8	8.	.0280	.0970	7.5	.0279	.1054	.1012	.1167
10 old	10.	.0250	.0750	$8\frac{5}{8}$	.0250	.0909	.0830	.0921
10 new	10.4	.0250	.0712	10.	.0220	.0780	.0746	.0879*
12	12.	.0221	.0613	11.8	.0221	.0626	.0619	.0725 † .0683 ‡
14	14.	.0197	.0517	14.	.0197	.0517	.0517	.0568
16	16.	.0182	.0443	14.6	.0183	.0502	.0472	.0495
18	18.	.0170	.0386	18.4	.0158	.0386	.0386	.0429
20	18.8	.0161	.0371	20.	.0156	.0344	.0357	.0372
24	22.	.0133	.0322	24.	.0138	.0279	.0300	.0329
30	28.	.0124	.0233	30.	.0121	.0213	.0223	.0262
40	35.	.0100	.0186	40.	.0100	.0150	.0168	.0195
50	40.	.0090	.0160	50.	.0083	.0117	.0138	.0153
60	61.5	.0081	.0081	47.	.0076	.0136	.0109	.0124
80	67.	.0055	.0094	81.	.0055	.0068	.0081	.0095
100	101.	.0047	.0052	102.	.0043	.0055	.0054	.0068
120	116.	.0034	.0052	120.	.0034	.0049	.0051	.0052
140	128.	.0034	.0044	136.	.0034	.0039	.0042	.0046

* Average of 8 and new 10-mesh. † With old 10-mesh. ‡ With new 10-mesh.

TABLE III.—*Fall-Tube Results. Fastest Grains.*

			No. observations.	Copper.	No. observations.	Galena.	No. observations.	Wolframite.	No. observations.	Antimony.	No. observations.	Cassiterite.	No. observations.	Arsenopyrite.	No. observations.	Chalcocite.	No. observations.	Magnetite.	No. observations.	Pyrrhotite.	No. observations.	Sphalerite.	No. observations.	Epidote.	No. observations.	Quartz.	No. observations.	Anthracite.
Sp. gr.	...		...	8.479	...	7.586	...	6.937	...	6.706	...	6.261	...	5.627	...	5.334	...	4.987	...	4.508	...	4.046	...	3.380	...	2.640	...	1.473
Sieve.		Aver. diam of grains.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.		Veloc. In. per Sec.
10-12	...	.07246	11	17.601	20	17.085	10	16.032	15	16.180	10	16.066	10	15.421	10	13.307	20	13.427	10	13.218	6	10.472	8	10.529	20	8.746	10	3.743
12-14	...	.05683	10	16.410	12	16.000	10	14.970	12	14.287	12	14.558	10	13.543	10	12.149	20	12.559	10	12.075	10	9.746	6	9.365	20	7.314	10	3.194
14-16	...	.04947	10	15.238	20	15.186	10	14.090	10	13.226	10	13.738	12	12.962	10	11.160	17	11.412	10	10.909	10	9.121	8	8.817	20	6.579	10	3.026
16-18	...	.04291	10	14.091	20	14.054	10	13.175	12	12.320	10	13.018	10	12.229	10	10.830	20	10.641	10	10.247	6	8.515	10	8.348	20	6.168	3	2.704
18-20	...	.03716	10	12.590	20	12.862	10	11.962	10	10.955	10	11.601	10	10.621	8	9.283	12	9.375	10	8.984	10	7.773	10	7.320	20	5.597	3	2.360
20-24	...	.03286	10	12.467	20	12.229	10	10.785	10	10.448	10	10.831	10	10.294	10	8.706	20	8.707	10	8.291	10	7.138	10	6.724	20	5.219	3	2.194
24-30	...	.02615	11	11.583	20	10.855	10	9.708	10	9.388	10	9.624	10	9.197	10	7.983	20	7.983	10	7.398	6	6.261	10	6.085	20	4.596	3	1.901
30-40	...	.01954	11	8.892	20	8.914	10	7.986	10	7.413	8	7.641	8	7.019	10	6.529	15	6.530	10	6.358	10	5.224	10	4.989	20	3.525	3	1.631
40-50	...	.01530	11	7.217	20	7.314	10	6.621	10	6.503	10	6.405	10	5.872	10	5.259	20	4.761	10	5.167	10	4.133	10	4.050	20	2.879	3	1.280
50-60	...	.01235	10	5.818	20	6.061	10	5.549	10	5.472	10	5.533	8	4.986	10	4.579	...	.....	10	4.153	5	3.497	10	3.316	20	2.412	3	1.040
60-80	...	.00950	10	4.869	20	4.821	10	4.643	10	4.426	8	4.424	10	4.042	10	3.664	...	.....	10	3.337	10	2.783	10	2.731	20	2.040	3	.710
80-100	...	.00676	10	3.722	24	3.579	10	3.388	10	3.316	10	3.241	8	2.953	10	2.651	...	.....	10	2.441	10	2.118	10	1.955	20	1.378	3	.610
100-120	...	.00523	10	2.637	20	2.655	11	2.472	10	2.850	10	2.384	10	2.023	10	1.960	...	.....	8	1.912	10	1.561	10	1.453	20	0.951	2	.492
120-140	...	.00461	10	2.302	13	2.345	10	2.285	6	2.635	10	2.152	10	1.903	9	1.803	...	.....	10	1.744	5	1.437	10	1.348	17	0.795	1	.387*
140	...	.00416																										

* Measured upon 1-foot fall, instead of 8 feet (from *a* to *b*, Fig. 1).

TABLE IV.—*Fall Tube Results. Slowest Grains.*

Sieve.	Average diameter of grains.	Copper.		Galena.		Wolframite.		Antimony.		Cassiterite.		Arsenopyrite.		Chalcocite.		Magnetite.		Pyrrhotite.		Sphalerite.		Epidote.		Quartz.		Anthracite.	
Sp. gr.		8.479		7.586		6.937		6.706		6.261		5.627		5.334		4.987		4.508		4 046		3.380		2 640		1.473	
		Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.	Velocity, Inches per Second.	No. observations.
10–12	.07246	9.163	20	12.821	20	10.548	10	9.931	15	10.727	10	9.697	10	7.868	10	9.343	20	7.681	10	7.245	6	6.294	8	4.993	20	1.383	10
12–14	.05683	8.205	12	12.063	10	9.625	10	8.289	12	8.597	10	8.292	10	6.378	20	7.893	10	6.337	10	6.465	6	5.792	6	4.311	20	1.179	10
14–16	.04947	7.427	20	11.195	10	8.256	10	8.118	10	7.649	12	7.291	10	5.800	17	7.318	10	5.880	10	5.914	8	4.986	20	4.363	10	.991	10
16–18	.04291	6.931	20	9.667	10	7.442	12	6.827	10	7.046	10	5.800	10	4.872	20	6.249	10	5.154	6	5.327	10	4.247	20	3.845	3	.842*	3
18–20	.03716	5.783	20	9.057	10	6.296	10	6.079	10	6.550	16	5.208	8	4.401	12	5.377	10	4.614	10	4.788	10	4.016	20	3.398	3	.818	3
20–24	.03296	5.509	20	8.153	20	5.749	10	5.333	10	5.749	10	4.201	10	4.025	20	4.706	10	4.277	10	4.103	10	3.558	20	2.801	3	.750	3
24–30	.02615	4.363	20	6.579	10	4.572	10	4.433	10	4.496	10	2.962	10	3.345	20	4.000	10	3.153	6	3.064	10	1.943*	20	2.221	3	.560	3
24–30	.01654	3.125*	20	5.203	10	3.357	10	3.425	8	3.675	8	2.222	10	2.087	15	3.185	10	2.013	10	2.259	10	1.315	20	1.576	3	.413	3
30–40	.01530	2.243	20	4.000	10	2.513	10	2.596	10	3.077	5	1.655	10	1.621*	20	2.180	10	1.556*	10	1.630	10	1.057	20	1.261	3	.316	3
40–50	.01235	1.765	20	3.333	10	1.913*	3	1.397	10	2.167	5	1.154	10	1.299	...	.....	10	1.026	5	1.277	10	.728	20	1.026	3	.228	3
50–60	.00950	1.304	20	2.087	10	1.345	7	1.181*	8	1.435	3	.598	10	.749	...	.....	10	.650	5	.800	10	.390	20	.660	3	.151	3
60–80	.00676	.910	24	1.328	10	.768	10	.561	10	.....	1	.337	10	.367	...	.....	10	.405	2	.480	10	.206	10	.355	3	.082	2
80–100	.00523	.634	5	0.934	11	.532	10	.454	10	.....	1	.133	9	.245	...	.....	8	.247	1	.305	10	.155	3	.230	2	.061	2
100–120	.00461	.536	10	.828	10	.462	6	.388	10	.....	..	.....	7	.262	...	.....	1	.214	1	.291	10	.138	2	.210	1	.045	1
140	.00416	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...

* This value, and all below it in the same column, was measured on 1-foot fall instead of 8 feet.

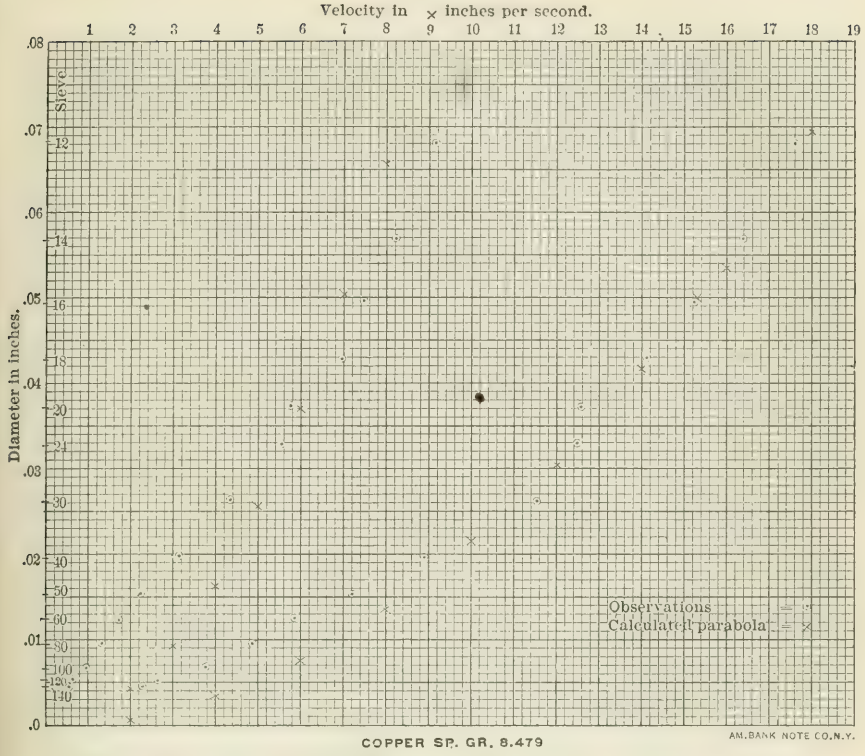
The specific gravities are the result of three or four closely agreeing tests. The diameters are given in fractions of an inch; the velocities, in inches per second. In all cases the grains were thoroughly wetted, and in many cases they were boiled in water, before being dropped.

Some remarks are called for by apparent inconsistencies in Table III. In nearly every instance, cassiterite fell faster than antimony, although its specific gravity is lower. I explain the low specific gravity given by the presence of a little quartz in included grains with the cassiterite, while the free grains probably have a higher specific gravity than antimony, and fall ahead of that metal, as they should. The inconsistency between chalcocite and magnetite is due to the shape of the particles; magnetite has rounded or cubical grains; chalcocite is very flat and scaly. Copper does not lead galena nearly so much as one would expect. This is due to the shape of the particles. The copper was Calumet and Hecla stamp-copper, as free as possible from rock, pieces being all more or less flattened, and the finer particles were to some extent arborescent and leaf-like, while the galena was taken from large pure cubes of Wisconsin mineral. The work upon magnetite broke down at the 60-mesh sieve, sizes below attracting each other so much that the large flakes resulting made a test impossible.

The fall-velocities for the slowest particles, that is, the particles which fell at a period when 90 per cent. (as estimated by the eye) had fallen, are given in Table IV. These are perhaps of less value than those of the fastest particles, since there is a certain personal equation which may vary. But for other reasons, as I think I shall be able to show later, these values are of very great interest and play an important part in the whole ore-dressing discussion. The values marked * and all below them in the columns were measured upon 1-foot fall instead of 8 feet, and are therefore somewhat better determinations than the values for the corresponding sizes in the other columns. Towards the lower ends of these columns frequent inconsistencies will be noted. They are, however, not very serious, and are accounted for, partly as in the case of the fastest grains, Table III., and partly by the difficulty in judging the 90 per cent.

That the meaning of the figures in Tables III. and IV. may be more plainly shown to the eye, curves may be drawn representing the fastest and slowest grains for each mineral. The points of actual observation, and those of the corresponding calculated parabola, are shown in diagrams 1 to 13 inclusive.

DIAGRAM 1.


Velocity in inches per second.
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19
Diameter in inches.
.08 .07 .06 .05 .04 .03 .02 .01 .0
Sieve
12 14 16 18 20 24 30 40 50 60 80 100 120 140
Observations
Calculated parabola
COPPER SP. GR. 8.479
AM. BANK NOTE CO. N.Y.

Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

The parabolas that are plotted in these diagrams with the fall-velocities were calculated as follows: For example, take the fastest grains of galena. A point is selected on the .05 inch diameter line which would apparently bring the parabola well among the observed points. Its velocity, 15.2 inches per second is read upon the upper line. We then fill in the formula for the parabola:

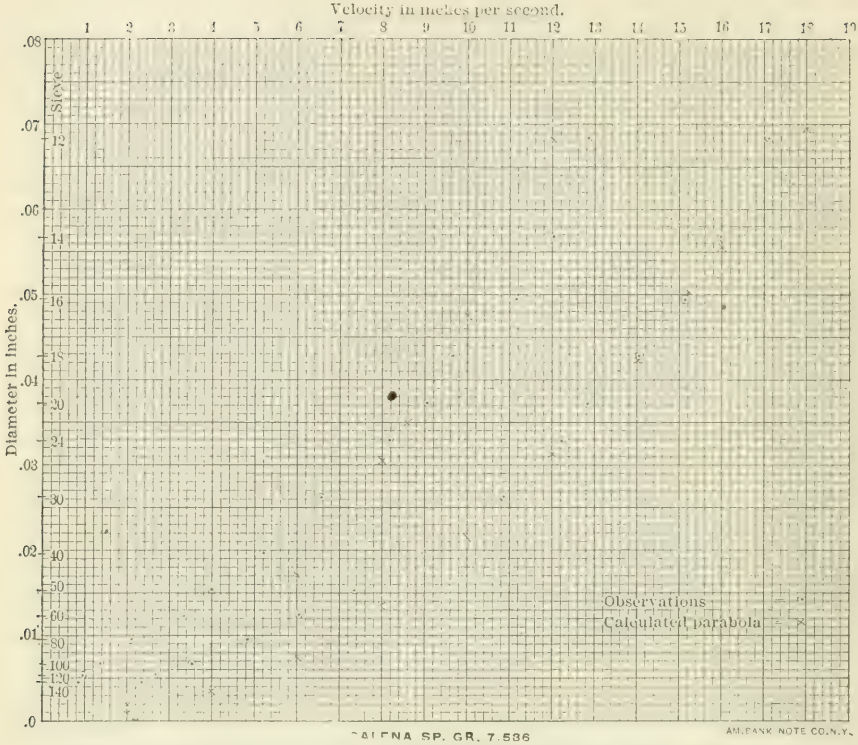
$$\frac{V^2}{D} = C.$$

V = velocity in inches per second.
D = diameter of the grain in inches.
C = a constant.

$$\frac{15.2 \times 15.2}{.05} = C = 4621.$$

We then assume different velocities and solve the equation,

DIAGRAM 2.



Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

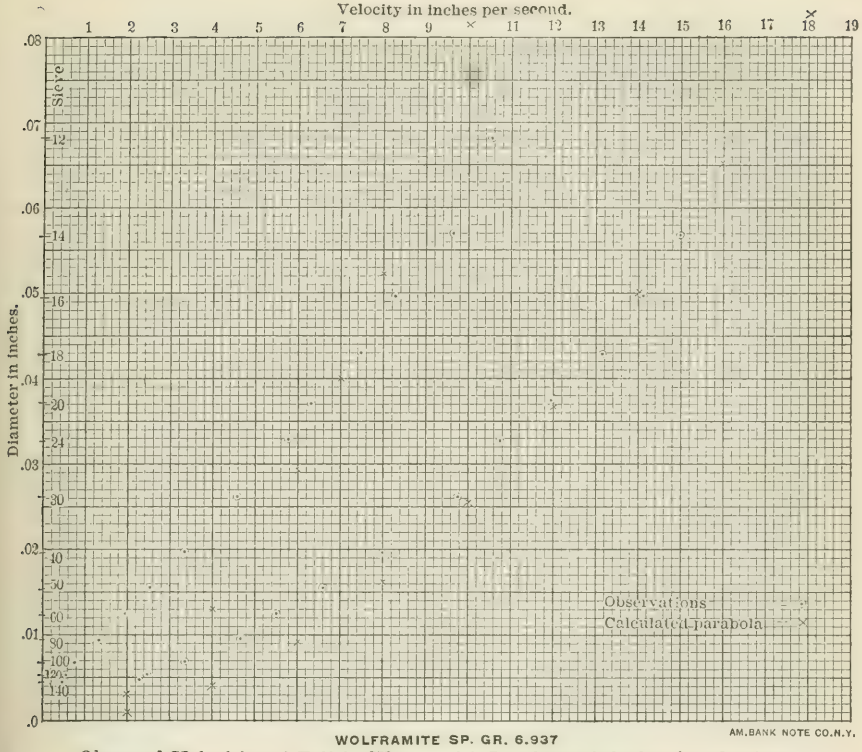
$$\frac{V^2}{D} = 4621,$$

to get the values of D. D and V being known, the plotting of the curve is easily done.

Table V. shows, for these thirteen minerals, both Rittinger's and the author's parabolas for fastest and slowest grains.

The discrepancy between the two sets of parabolas is striking. That Rittinger's values for the slowest grains should be less than mine is natural, since I did not wait to get absolutely the last grain, but marked the moment when 90 per cent. (estimated) had passed. In regard to the fastest grains, however, my method of letting 50 grains fall at a time selects by natural process the fastest grains of the 50, and should yield higher results than the method of picking out compact grains by the eye, and letting them drop one at a time. The test makes a more perfect selection than can the eye. Individual grains were tried, and found to confirm this view.

DIAGRAM 3.

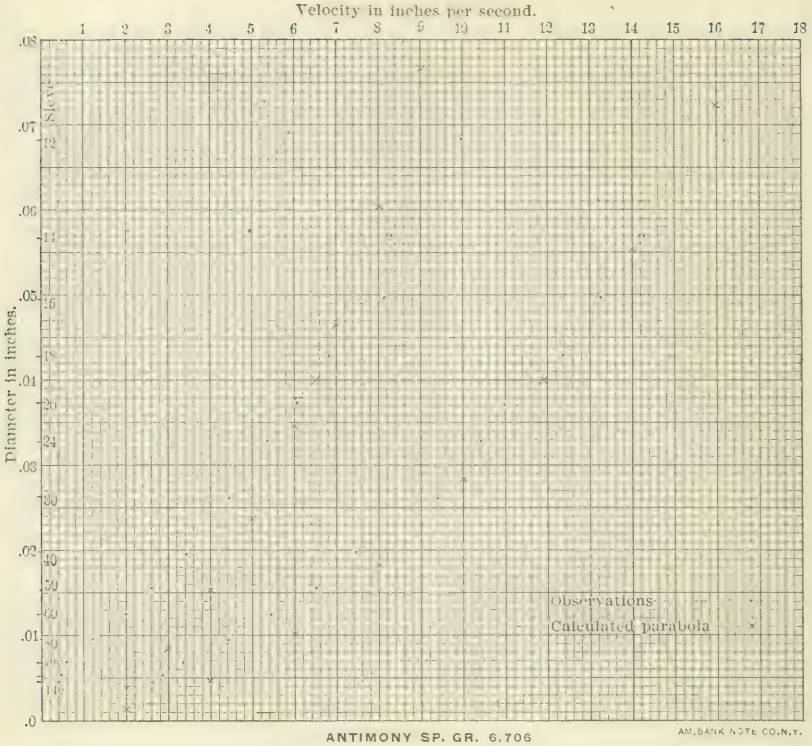

Velocity in inches per second.
1 2 3 4 5 6 7 8 9 11 12 13 14 15 16 17 18 19
Diameter in inches.
.08 .07 .06 .05 .04 .03 .02 .01 .0
Sieve
12 14 16 18 20 24 30 40 50 60 80 100 120 140
Observations
Calculated parabola
WOLFRAMITE SP. GR. 6.937
AM. BANK NOTE CO. N.Y.

Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

TABLE V.—*Computed Parabolas of Fall.*

	RITTINGER'S PARABOLAS.		AUTHOR'S PARABOLAS.	
	For Fastest Grains. $\frac{V^2}{D}$	For Slowest Grains. $\frac{V^2}{D}$	For Fastest Grains. $\frac{V^2}{D}$	For Nearly the Slowest Grains $\frac{V^2}{D}$
Copper	2229	1102	4683	973
Galena	1977	972	4621	2113
Wolframite	1769	875	3920	1225
Antimony	1701	841	3540	1056
Cassiterite	1569	776	3844	1156
Arsenopyrite	1380	682	3425	812
Chalcocite	1293	639	2599	673
Magnetite	1189	588	2599	1037
Pyrrhotite	1046	517	2520	720
Sphalerite	909	450	1730	720
Epidote	709	352	1585	520
Quartz	489	242	925	353
Anthracite	141	70	180	20

DIAGRAM 4.



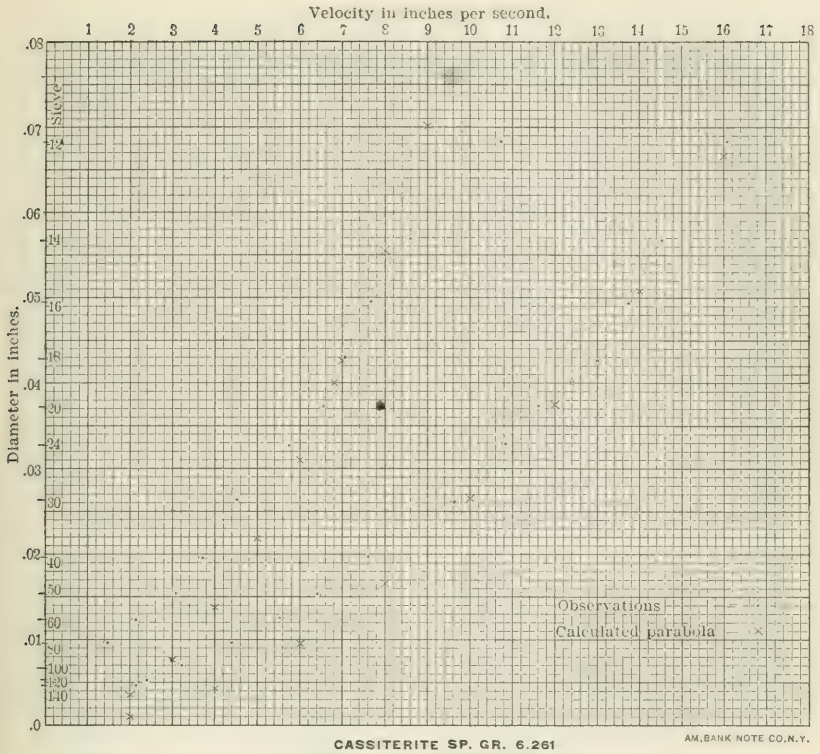
Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

The computations made by A. E. Woodward and myself from Rittinger's formulas, and published in our paper on this subject (*Trans.*, xviii., 644), must therefore give place to the present figures obtained by actual test.

To compare further the results obtained by the fall-tube with those of Rittinger, the ratios of diameters of the particles of the several minerals to those of equal-settling particles of quartz, were determined as follows: A number of velocities were assumed, and corresponding diameters of the fastest grains for each mineral were taken from the curves of free-falling particles. These values are here tabulated. (See Table VI.)

From these figures for fastest grains were computed multipliers to be used in obtaining the diameters of equal-settling particles of quartz. These are given in Table VII., in the final column of which Rittinger's multipliers for average grains are also given.

DIAGRAM 5.

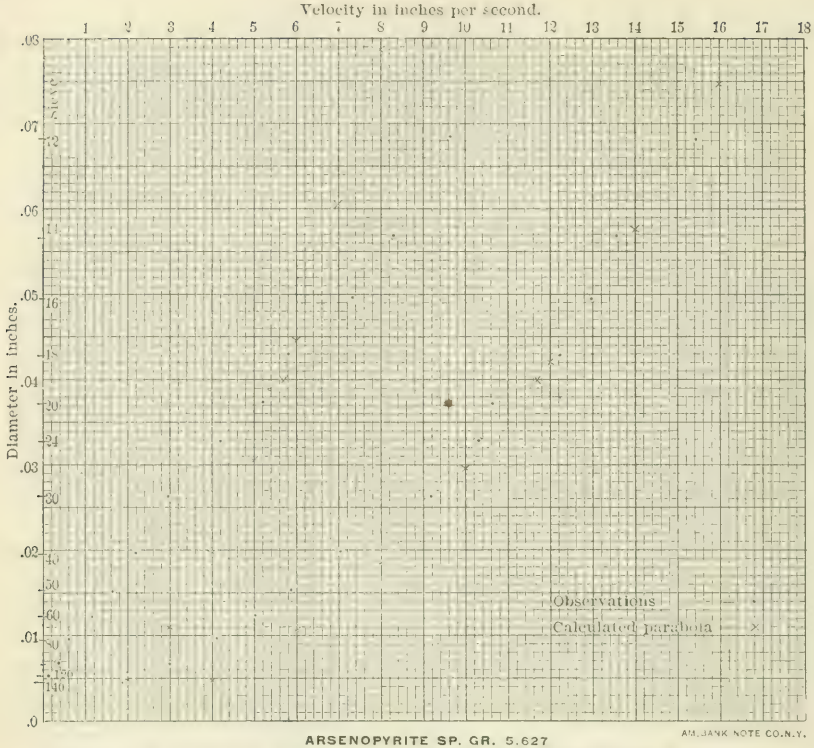


Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

TABLE VI.—*Diameters Corresponding to Given Velocities of Fall.*

	Velocity in Inches Per Second.								
	1.	2.	3.	4.	5.	6.	7.	8.	9.
	Diameter in Inches.								
Anthracite...	.011	.027	.049	.080	.....	.....	.....	.....	.....
Quartz.....	.0055	.0095	.011	.017	.03	.041	.053	.064	.075
Epidote.....	.0035	.007	.0105	.015	.02	.0255	.034	.041	.051
Sphalerite....	.....	.0065	.0105	.0145	.0185	.023	.0315	.0385	.048
Pyrrhotite ...	.....	.0055	.0085	.0115	.015	.0185	.0235	.03	.036
Chalcocite....	.....	.005	.0075	.0105	.0145	.018	.022	.0262	.0345
Arsenopyrite	.....	.005	.007	.009	.0124	.016	.0195	.0225	.0255
Cassiterite....	.....	.0045	.00615	.0085	.011	.014	.0175	.021	.024
Antimony....	.....	.0035	.0055	.0085	.011	.014	.0175	.0215	.025
Wolframite...	.....	.0035	.006	.0082	.0105	.0135	.0165	.0195	.023
Galena	.....	.0035	.006	.0075	.010	.012	.0145	.017	.020
Copper	.....	.0035	.0055	.0072	.010	.0128	.0148	.017	.020

DIAGRAM 6.

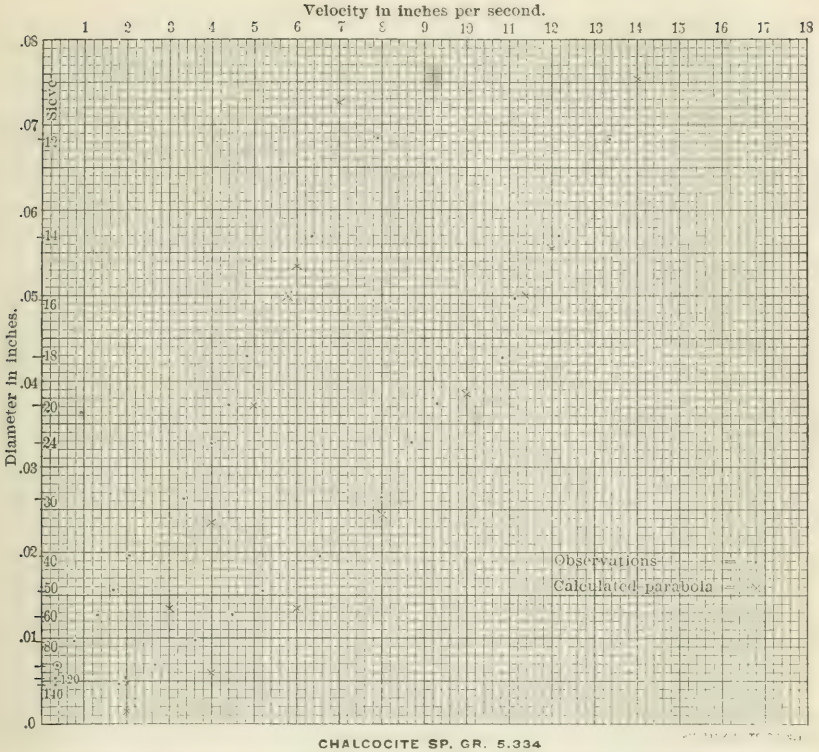


Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

TABLE VII.—*Equal-Settling Factors, or Multipliers for Obtaining the Diameter of Quartz which will be Equal-Settling with the Mineral Specified.*

	Velocity in inches per second.									Rittinger's Multipliers.
	1.	2.	3.	4.	5.	6.	7.	8.	9.	
	AUTHOR'S MULTIPLIERS.									
Anthracite...	.500	.352	.225	.213	.....	.....	.....	.....	.....	.288
Epidote	1.57	1.35	1.05	1.13	1.50	1.61	1.56	1.56	1.47	1.45
Sphalerite ...	.....	1.46	1.05	1.17	1.62	1.64	1.68	1.66	1.56	1.85
Pyrrhotite...	.....	1.73	1.29	1.48	2.00	2.22	2.26	2.13	2.08	2.14
Chalcocite...	.....	1.90	1.47	1.62	2.07	2.28	2.41	2.44	2.17	2.64
Arsenopyrite	.....	1.90	1.57	1.89	2.42	2.56	2.72	2.84	2.94	2.82
Cassiterite...	.....	2.11	1.79	2.00	2.73	2.93	3.03	3.05	3.12	3.32
Antimony ...	.....	2.71	2.00	2.00	2.73	2.93	3.03	2.98	3.00	3.48
Wolframite..	.....	2.71	1.83	2.07	2.86	3.04	3.21	3.28	3.26	3.64
Galena.....	.....	2.71	1.83	2.26	3.00	3.42	3.65	3.76	3.75	4.01
Copper.....	.....	2.71	2.00	2.36	3.00	3.20	3.58	3.76	3.75	4.56

DIAGRAM 7.



Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

Upon inspection of the curves for free-falling grains, in diagrams 1 to 13, three facts are noticed:

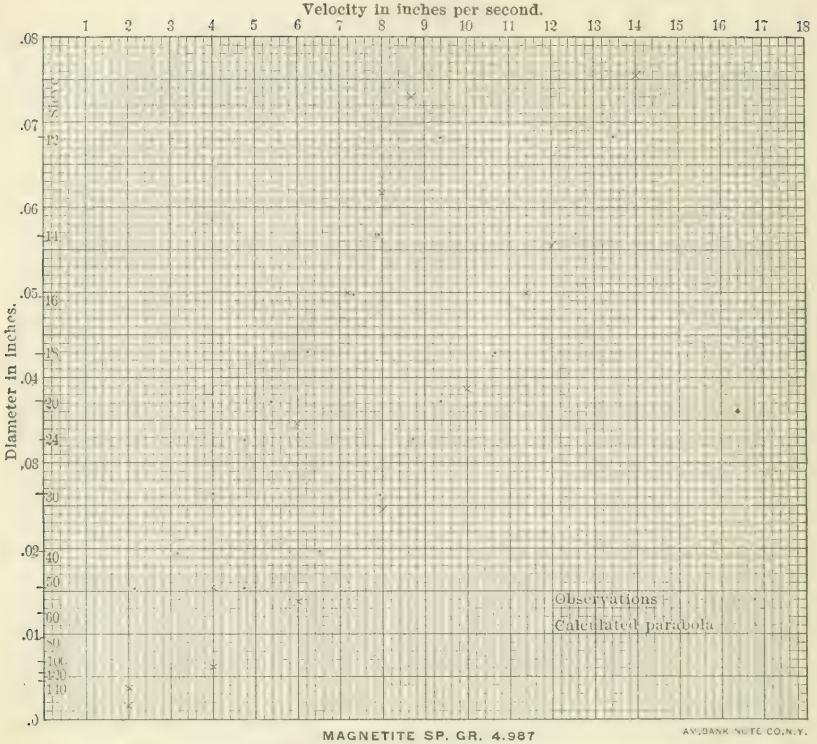
1. The lines of fastest grains, and of slowest grains, depart very considerably from the parabola, particularly on the smaller sizes.

2. The space between the two lines of fastest grains and slowest grains is a wide zone or belt.

3. The width of the zone may vary greatly, depending upon the shape of the particles—for example, the copper zone is much wider than that of the galena.

With regard to the first point, Rittinger makes note of the fact that his formulas fail for finer sizes. These curves, therefore, place on record the degree to which they have failed with the minerals here tested. We see that the velocity of any mineral which tends to break into flat scales, or elongated grains (for instance, copper or chalcocite), is so modified as to change its class entirely. Thus, the

DIAGRAM 8.



Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

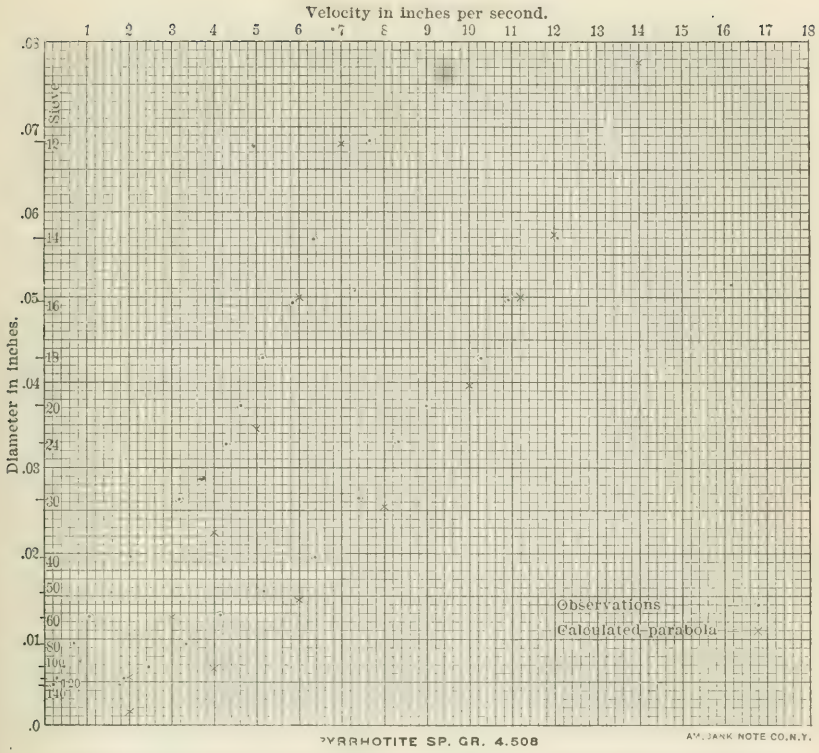
curves for fastest grains of galena and copper are almost identical, although their specific gravities are widely different.

Each mineral has, therefore, its own "personal equation," and minerals cannot be classed by specific gravity alone. Hence, no formula can be founded on specific gravity and sieve-size only. It must be modified by the coefficient of each mineral for practical use.

The second point, also, was recognized by Rittinger, as is shown in his formulas for fastest grains and slowest grains already quoted in this paper. But the deduction which followed from adopting a mean value for these grains has been somewhat of a puzzle to me.

Let us take an example from the curves of sphalerite (blende) and quartz. A mean grain of quartz, of .07 inch diameter, will fall 6.7 inches in one second. Following down the sphalerite curve, we find that a mean grain of sphalerite of .039 inch diameter settles 6.7 inches in one second. If then, we were to use these mean

DIAGRAM 9.

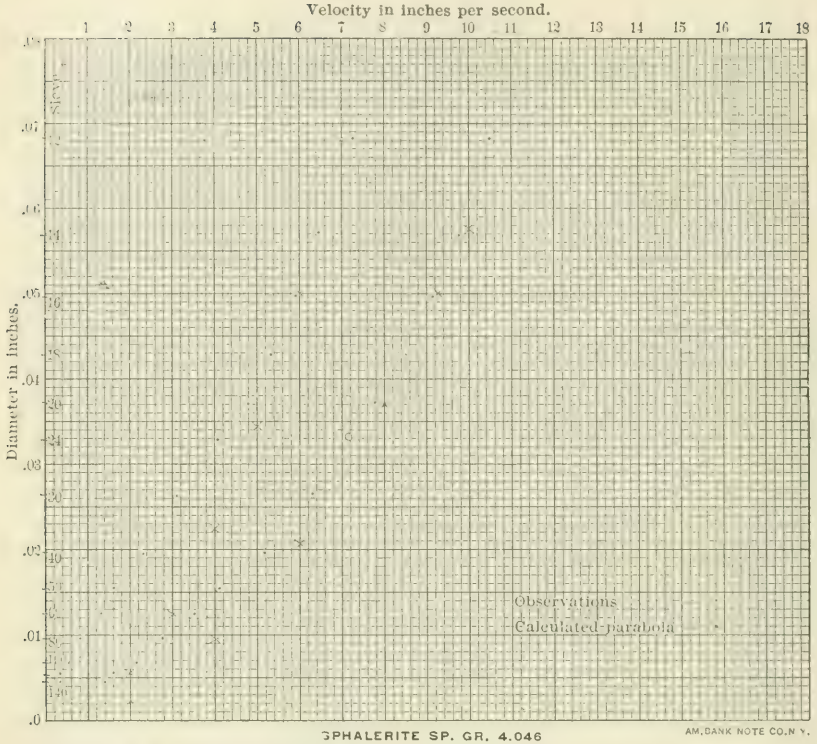

Velocity in inches per second.
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18
Diameter in Inches.
.08 .07 .06 .05 .04 .03 .02 .01 .0
Sieve
12 14 16 18 20 24 30 40 50 60 80 100 120 140
Observations
Calculated parabola
PYRRHOTITE SP. GR. 4.508
AM. BANK NOTE CO. N.Y.

Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

values, as Rittinger recommends, for obtaining our sieve-scale, we should conclude that, with two sieves, one having holes of .07 inch diameter and the other of .039 inch diameter, the fastest grain of quartz between these two sizes has less speed in settling than the slowest grain of the sphalerite, and therefore this product would be jiggable, according to the law of equal-settling particles. But, in fact, the fastest grain of quartz falls about 8.5 inches in one second, while the slowest grain of sphalerite settles only 5 inches in one second; hence, the product is unjiggable according to the law of equal-settling particles. Indeed, these zones of quartz and sphalerite lap over on each other, even on a single sieve; and if the law of equal-settling particles were the only law of jigging, sphalerite and quartz could not be sized closely enough to prepare them for jigging.

The converse seems, therefore, evident, namely, that under no

DIAGRAM 10.



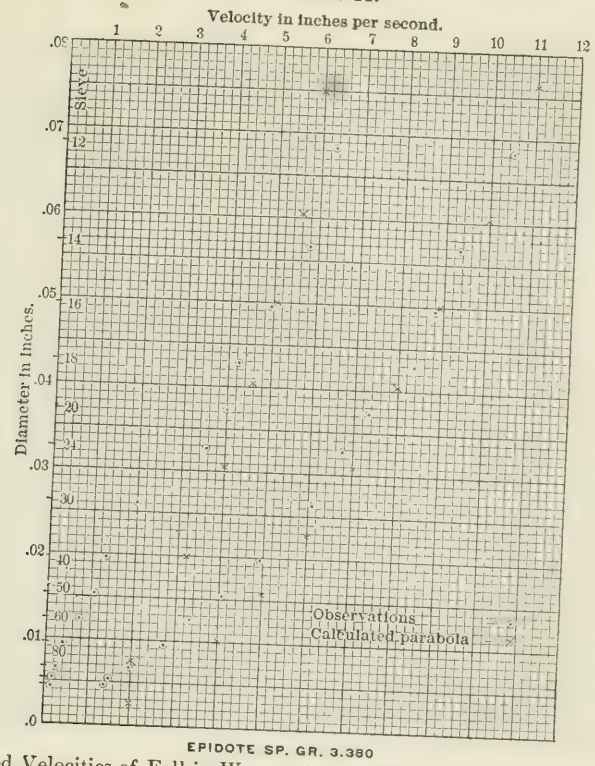
Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

circumstances is the law of equal-settling particles the whole law of jigging.

II.—Interstitial Currents.

Professor H. S. Munroe tested, in 1889, the effect of confined space upon falling particles. He timed different sizes of lead shot, or spheres, falling in a narrow glass tube filled with water, Fig. 3a. If d equals the diameter of the shot, and D that of the tube, he found that the larger the fraction $\frac{d}{D}$ the greater was the retardation or loss of velocity by the shot. When this fraction equals unity, the shot stops. He applies this principle to the question of equal-settling particles as follows: If particles of quartz, for example (Fig. 3b), are represented by the larger circles, and those of equal-settling galena by the smaller circles, then, when these mixed particles are settling *en masse*, or are held in suspension by a rising current of

DIAGRAM 11.



Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

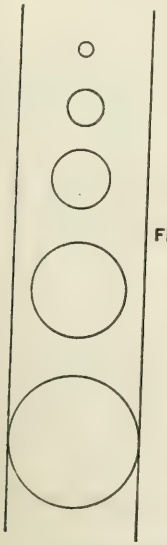
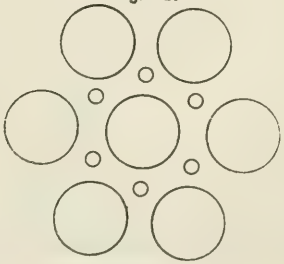


Fig. 3a.

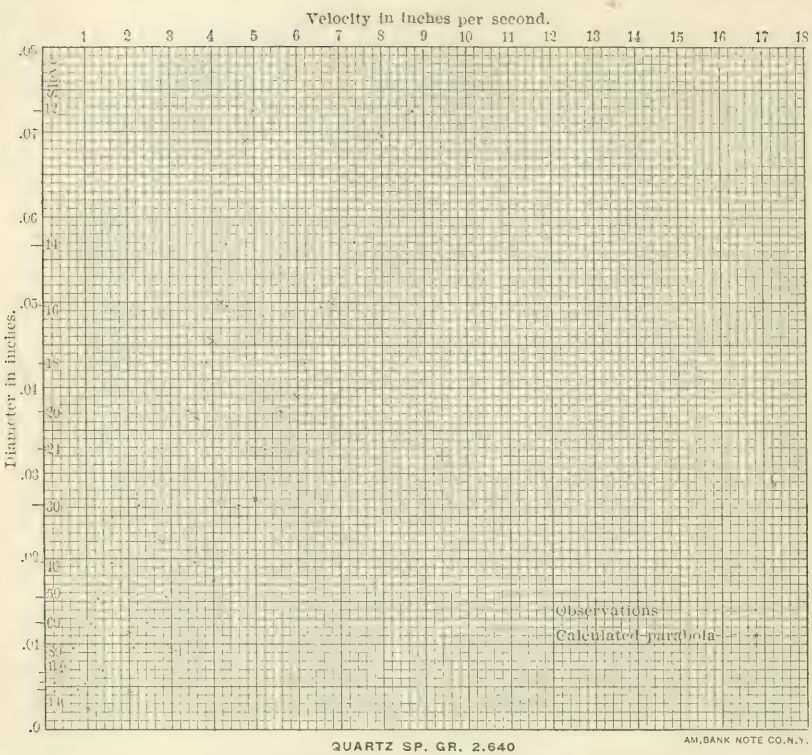
Falling spheres.

Fig. 3b.



Interstitial fall.

DIAGRAM 12.



Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

water, each particle may be considered to be falling in a tube, the walls of which consist of the surrounding particles. Substituting a circle in each case for the imaginary tube, we have Fig. 4, representing the conditions for galena and quartz, the outer circle representing, in each case, the imaginary tube. A glance is sufficient to show us that the fraction $\frac{d}{D}$ is much smaller for the galena particle than it is for the quartz. The galena particle will, therefore, be less impeded in its fall than the quartz, and, in consequence, the particles of galena that are found adjacent to the particles of quartz will be smaller than the ratio which the law of equal-settling particles would indicate. Professor Munroe infers that these interstitial currents account for the fact,

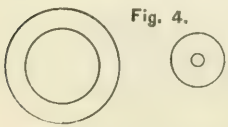
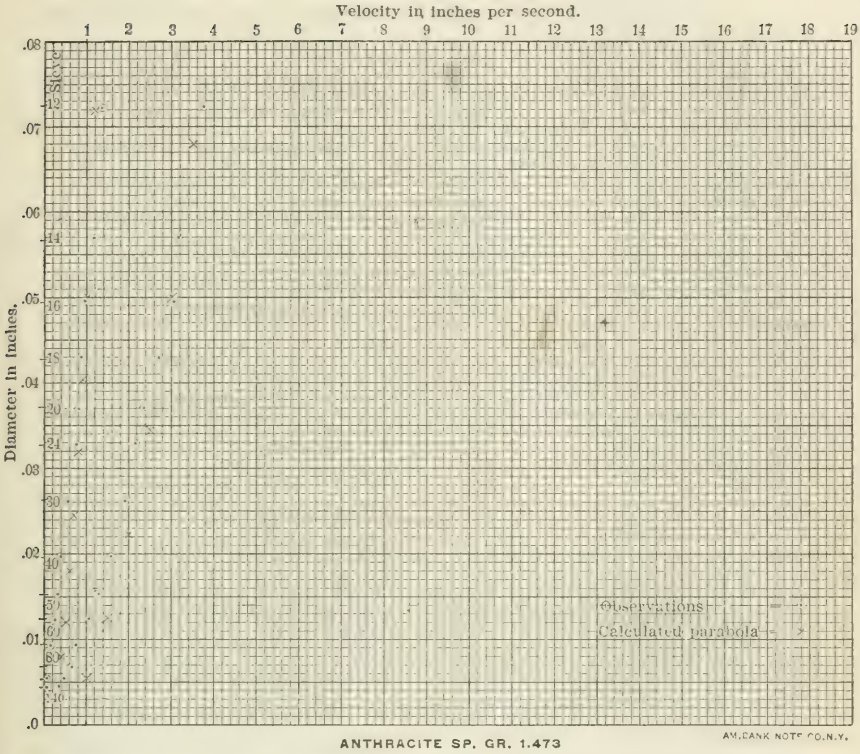


Fig. 4.

Imaginary tubes of interstitial fall.

DIAGRAM 13.



Observed Velocities of Fall in Water, with Parabolas Calculated to Conform Most Nearly Thereto.

made use of in the mills, that a jig will save galena which is much finer than would be the case if the law of equal-settling particles was the only law of jigging. And he finally proves, by equating his formulas, that the ratio of diameters for the quartz and galena, after the interstitial currents have brought the grains to equilibrium, will be as 30 : 1.

Prof. Munroe says further (*Trans.*, xvii., 650):

"We see, therefore, that if the material to be treated is sized between the limits of 1 mm. and 30 mm., it will be possible to separate the quartz from the galena. All the spheres of galena will have a greater falling-velocity than the 1 mm. grain; and all the spheres of quartz will rise more readily and fall more slowly than the 30 mm. grain."

I have recently attempted to ascertain by experiment to what ex-

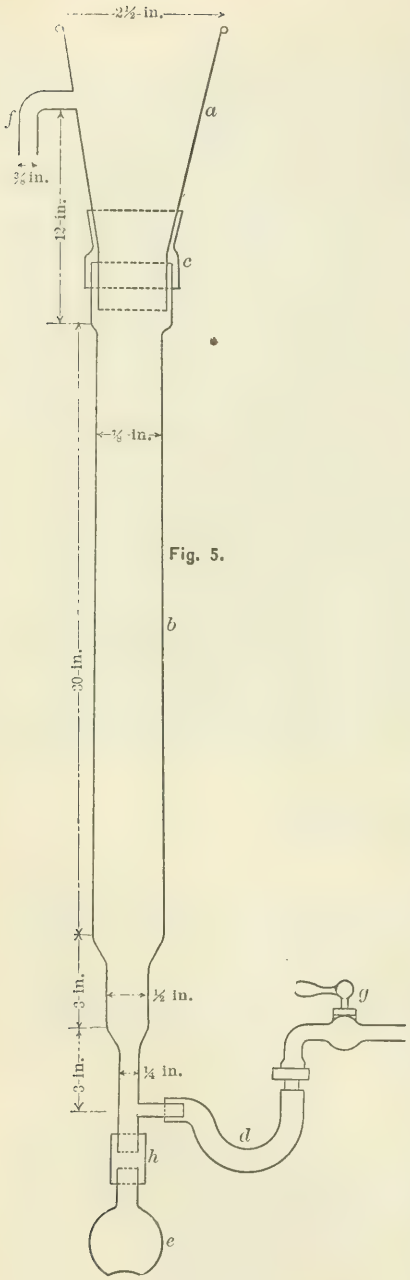


Fig. 5.

Pointed Tube.

tent this law of interstitial currents acts upon irregular particles. For this purpose, I constructed a pointed tube of the form shown in Fig. 5, which consists of a tin cone, *a*, with an overflow, *f*, united to a pointed tube of glass, *b*, by a rubber connector, *c*, and having a water-supply, *d*, regulated by the cock, *g*, and a bulb, *e*, joined by a rubber connector, *h*. If this apparatus be filled with water, and a sample of mixed sands, which pass through a ten-mesh sieve (an ordinary 8-ounce bottleful represents the quantity used), be charged gradually at the top, and a slight upward current of water be admitted through the tube, *d*, the sands will rapidly assume a condition of approximate equilibrium. Here we have sands, say, of two specific gravities, and of sizes ranging from ten-mesh to dust, which are held in gently-moving suspension by the slow upward current. This device presents, therefore, the conditions necessary to test the limits of interstitial currents.

By means of this pointed tube, the behavior of the minerals named in Table VIII., taken by pairs, has been tested.

TABLE VIII.—*List of Mineral Pairs Treated in the Pointed Tube.*

Quartz,	specific gravity	2.640	and Copper,	specific gravity	8.479
"	"	"	Galena,	"	7.586
"	"	"	Wolframite,	"	6.937
"	"	"	Antimony,	"	6.706
"	"	"	Cassiterite,	"	6.261
"	"	"	Arsenopyrite,	"	5.627
"	"	"	Chalcocite,	"	5.334
"	"	"	Magnetite,	"	4.987
"	"	"	Pyrrhotite,	"	4.508
"	"	"	Sphalerite,	"	4.046
"	"	"	Epidote,	"	3.380
"	"	"	Anthracite,	"	1.473

Each of these pairs was treated in the pointed tube (Fig. 5) by allowing from one half hour to two hours for the grains to come to equilibrium; and since the larger part of the sorting is done in the first minute, we may consider that the work is practically completed in half an hour.

While the sands are still kept in gently-moving suspension, the current is slightly slackened by means of the cock, *g*, and the heavier grains are allowed to find their way down into the bulb, *e*. When the bulb is full, the rubber connector, *h*, is pinched with the thumb and finger, and the bulb is replaced with a new one, which has been completely filled with water, care having been taken to remove the bubble of air from the neck. In like manner the second bulb is filled and removed, and a third, a fourth, and so on until all the sand, to the finest slimes, has been drawn off. The overflow will be found to contain very light particles, and also a few particles carried over by greasy flotation. This should be caught, and may be called the last bulb or drawing.

Each of these drawings, which were ten in number, was carefully dried and then sized in the nest of sieves (Fig. 2). The sizes, for example, in the galena series (see Plate II *a*), in the fifth flask, were found to be perfectly pure quartz down to the 30-mesh sieve. The 40-mesh contained a little galena; the 50-mesh was nearly all galena, with a little quartz; and all the sizes below 50 were pure galena.

The twelve pairs of minerals were all treated in this way, and the photographs (Plates I. to XII. inclusive) show the results obtained in the form of a graphical plot of the actual grains. The vertical columns Nos. 1, 2, 3, 4, etc., represent the successive bulbs. The horizontal lines indicate groups of particles resting upon like sieves.

This series of photographs shows very prettily what can be done with a pointed tube in the way of water-sorting, preparatory to sizing. A series of tables (Tables IX. to XXI. inclusive) has been prepared to accompany the photographs. These tables, giving the estimated percentage in every hill of the mineral which was to be concentrated, will be found convenient in interpreting the photographs.

In the galena-quartz photograph (Plates II. *a*, II. *b*) there are scarcely any mixed grains until slimes are reached. The ninth and tenth drawings are much mixed, and always will be so, coming from the apparatus here described. Their separation will be the subject of another paper.

The same is true of the copper-quartz photograph (Plate I.), except that the sample contained none of the very finest slimes, and therefore the ninth and tenth drawings are practically pure quartz.

Following through the various minerals and gravities, we see a general set of features possessed in common, but changing a little with each successive photograph. First, we have in copper a range of clean, pure quartz hills from No. 4 on 12 to No. 9 on 100. We also have a clean range of copper hills from No. 1 on 12 to No. 8 slimes, and, between the two, a valley almost destitute of grains, which is widest from No. 4 on 20 to No. 5 on 30, and narrows very much at No. 8 on 120.

In arsenopyrite (Plate VI.), the valley is gone on the 100-mesh line, and we have a plateau instead. In chalcocite (Plate VII.), the plateau has reached up to the 50-mesh line. In pyrrhotite (Plate IX.), it has reached the 40-mesh line. In epidote (Plate XI.), the plateau has disappeared below 24-mesh, and a single wide range of hills has taken its place.

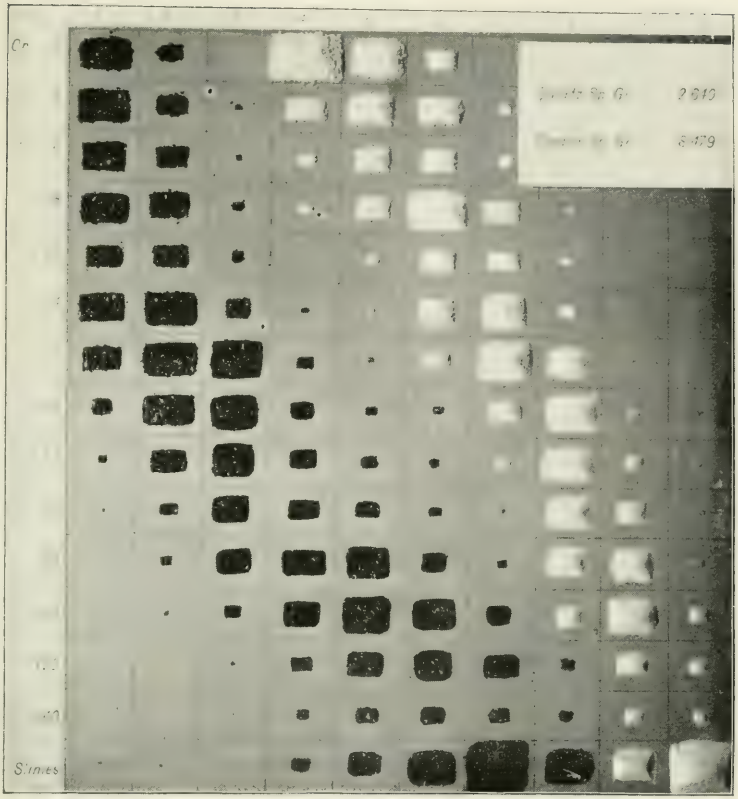
The above features are due to the convergence, towards the hill marked No. 10 slimes, of the two approximately parabolic curves.

A single plate (Plate XIII.) is given to illustrate a three-mineral separation, in which slate (specific gravity, 2.735), barite (specific gravity, 4.127), and galena (specific gravity, 7.586) are the three minerals. Table XXII. accompanies it, giving estimated percentages in the hills.

The weights of the hills for three columns of each of Plates I. to XII. inclusive (except VIII.) will be found in the following Tables, XXV. to XXXVI.

They are absolute weights in every case except where a hill contained a mixture of two minerals. In such a case the total weight

PLATE I.



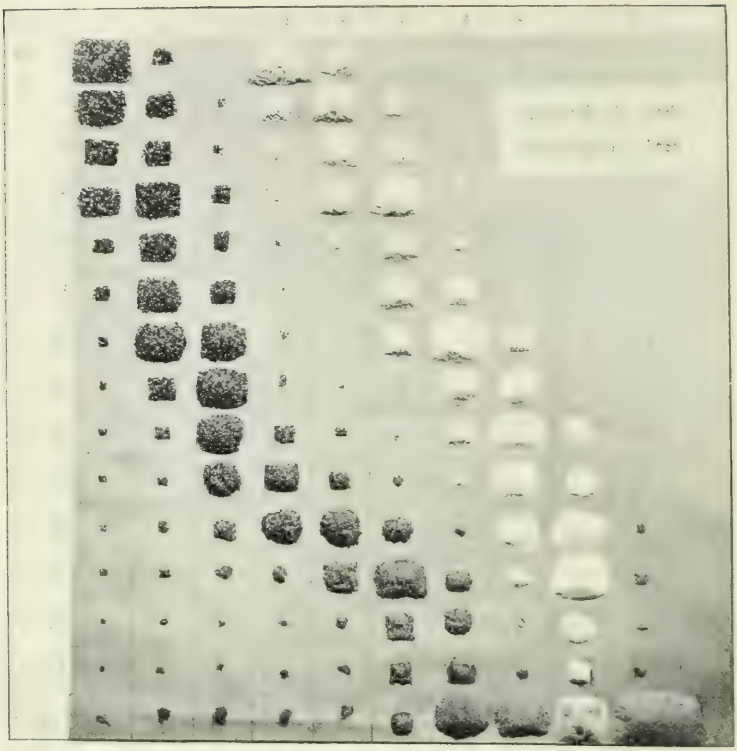
Separation of Quartz and Copper by Pointed Tube and Sieves.

TABLE IX.—*Estimated Percentage of Copper in the Hills of Plate I.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	100	.....	.....	.....	.....	.....	.....	.....	.....
14	100	100	100	.....	.....	.....	.....	.....	.....	.....
16	100	100	100	.....	.....	.....	.....	.....	.....	.....
18	100	100	100	.....	.....	.....	.....	.....	.....	.....
20	100	100	100	.....	.....	.....	.....	.....	.....	.....
24	100	100	100	100	2	.....	.....	.....	.....	.....
30	100	100	100	100	97	10	.....	.....	2	.....
40	100	100	100	100	100	95	1	.....	2	20
50	100	100	100	100	100	99	7	.....	2	20
60	100	100	100	100	100	100	45	.....	2	20
80	100	100	100	100	100	100	95	.....	2	20
100	100	100	100	100	100	100	99	5	2	10
120	100	100	100	100	100	100	100	60	2	10
140	100	100	100	100	100	100	100	90	5	10
Slimes	100	100	100	100	100	100	100	99	35	5

For the weights of the hills in columns 5, 6 and 7, see Table XXV.

PLATE II. *a*.

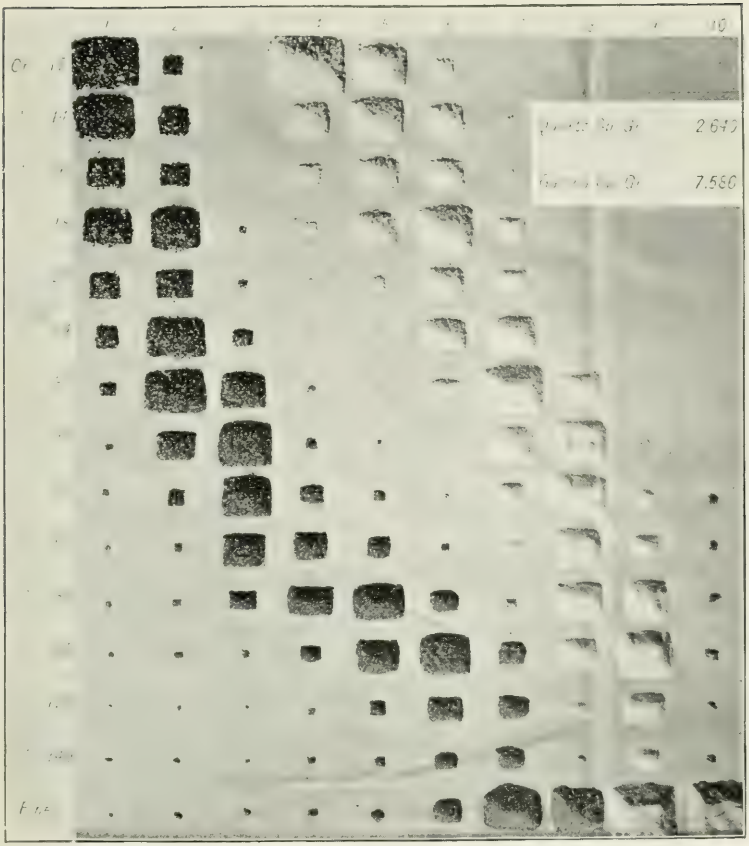


Separation (First Time) of Quartz and Galena by Pointed Tube and Sieves.

TABLE X.—*Estimated Percentage of Galena in the Hills of Plate II. a.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	100	.....	.....	.....	.....	.....	.....	.....	.....
14	100	100	100	.....	.....	.....	.....	.....	.....	.....
16	100	100	100	.....	.....	.....	.....	.....	.....	.....
18	100	100	100	.....	.....	.....	.....	.....	.....	.....
20	100	100	100	40	.....	.....	.....	.....	.....	.....
24	100	100	100	50	.....	.....	.....	.....	.....	.....
30	100	100	100	75	10	.....	.....	.....	.....	.....
40	100	100	100	90	50	10	.....	.....	.....	.....
50	100	100	100	100	90	60	.....	.....	.....	.....
60	100	100	100	100	100	90	5	.....	.....	.....
80	100	100	100	100	100	100	90	.....	.....	60
100	100	100	100	100	100	100	100	.....	.....	60
120	100	100	100	100	100	100	100	50	.....	50
140	100	100	100	100	100	100	100	90	25	50
Slimes	100	100	100	100	100	100	100	100	60	75

PLATE II. *b*.



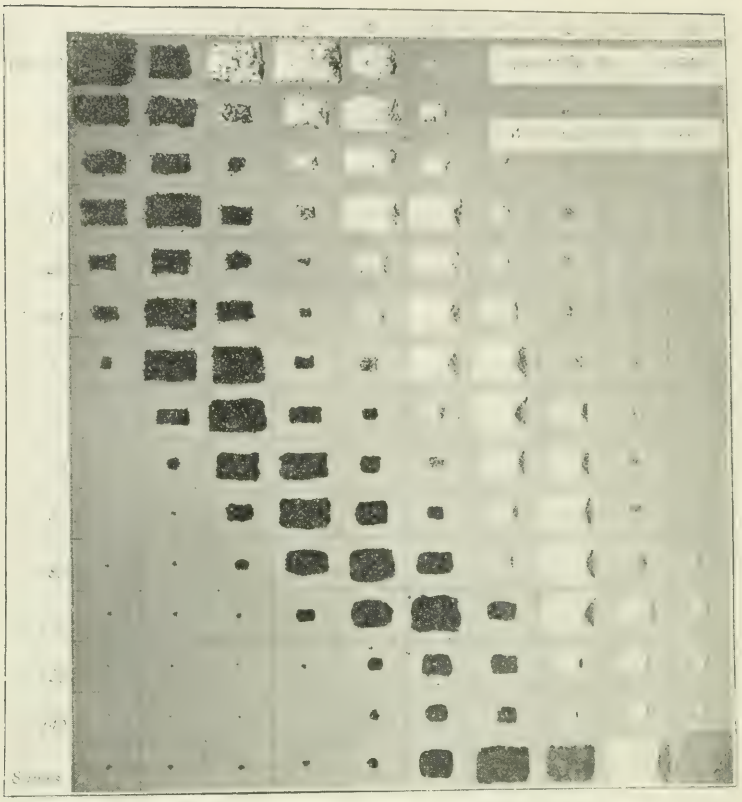
Separation (Second Time) of Quartz and Galena by Pointed Tube and Sieves.

TABLE XI.—*Estimated Percentage of Galena in the Hills of Plate II. b.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	100	.....	.....	.....	.....	.....	.....	.....	.....
14	100	100	.....	.....	.....	.....	.....	.....	.....	.....
16	100	100	.....	.....	.....	.....	.....	.....	.....	.....
18	100	100	100	.....	.....	.....	.....	.....	.....	.....
20	100	100	100	.....	.....	.....	.....	.....	.....	.....
24	100	100	100	25	.....	.....	.....	.....	.....	.....
30	100	100	100	75	.....	.....	.....	.....	.....	.....
40	100	100	100	100	50	.....	.....	.....	.....	.....
50	100	100	100	100	100	50	.....	.....	.....	75
60	100	100	100	100	100	100	.....	.....	.....	75
80	100	100	100	100	100	100	40	.....	.....	75
100	100	100	100	100	100	100	90	.....	.....	50
120	100	100	100	100	100	100	100	30	.....	50
140	100	100	100	100	100	100	100	40	.....	60
Slimes	100	100	100	100	100	100	100	95	60	75

For the weights of the hills in columns 5, 6 and 7, see Table XXVI.

PLATE III.



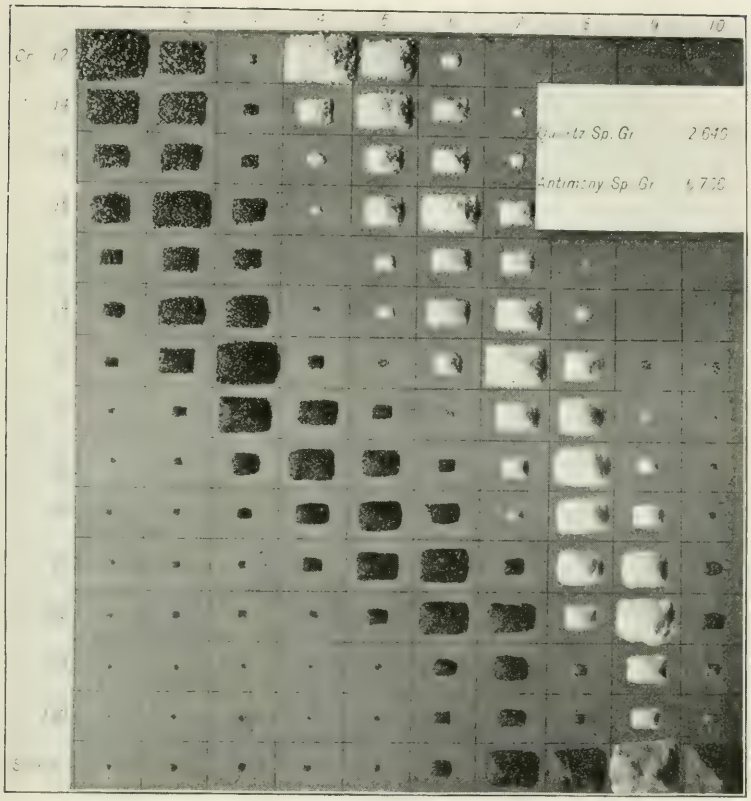
Separation of Quartz and Wolframite by Pointed Tube and Sieves.

TABLE XII.—*Estimated Percentage of Wolframite in the Hills of Plate III.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	100	1	.....	.....	.....	.....	.....	.....	.....
14	100	100	5	.....	.....	.....	.....	.....	.....	.....
16	100	100	30	.....	.....	.....	.....	.....	.....	.....
18	100	100	75	.....	.....	.....	.....	.....	.....	.....
20	100	100	95	5	.....	.....	.....	.....	.....	.....
24	100	100	100	15	.....	.....	.....	.....	.....	.....
30	100	100	100	80	10	.....	.....	.....	.....	.....
40	100	100	100	95	85	5	.....	.....	.....	.....
50	100	100	100	100	95	20	.....	.....	.....	.....
60	100	100	100	100	100	90	1	.....	.....	.....
80	100	100	100	100	100	95	10	.....	.....	2
100	100	100	100	100	100	100	95	1	.....	2
120	100	100	100	100	100	100	100	5	.....	2
140	100	100	100	100	100	100	100	10	.....	2
Slimes	100	100	100	100	100	100	100	90	30	45

For the weights of the hills in columns 5, 6 and 7, see Table XXVIII.

PLATE IV.



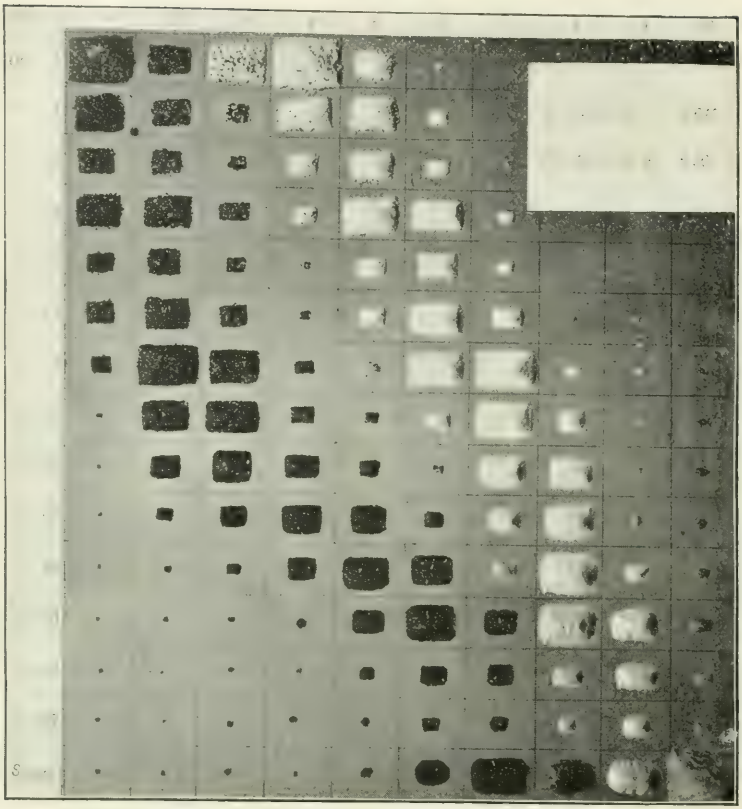
Separation of Quartz and Antimony by Pointed Tube and Sieves.

TABLE XIII.—*Estimated Percentage of Antimony in the Hills of Plate IV.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	100	50	.....	.....	.....	.....	.....	.....	.....
14	100	100	99	.....	.....	.....	.....	.....	.....	.....
16	100	100	100	.....	.....	.....	.....	.....	.....	.....
18	100	100	100	.....	.....	.....	.....	.....	.....	.....
20	100	100	100	.....	.....	.....	.....	.....	.....	.....
24	100	100	100	95	.....	.....	.....	.....	.....	.....
30	100	100	100	100	50	.....	.....	.....	.....	50
40	100	100	100	100	100	50	.....	.....	.....	50
50	100	100	100	100	100	95	.....	.....	.....	75
60	100	100	100	100	100	100	40	.....	.....	75
80	100	100	100	100	100	100	90	.....	.....	75
100	100	100	100	100	100	100	100	30	.....	75
120	100	100	100	100	100	100	100	55	.....	75
140	100	100	100	100	100	100	100	90	.....	75
Slimes	100	100	100	100	100	100	100	100	60	80

For the weights of the hills in columns 5, 6 and 7, see Table XXIX.

PLATE V.



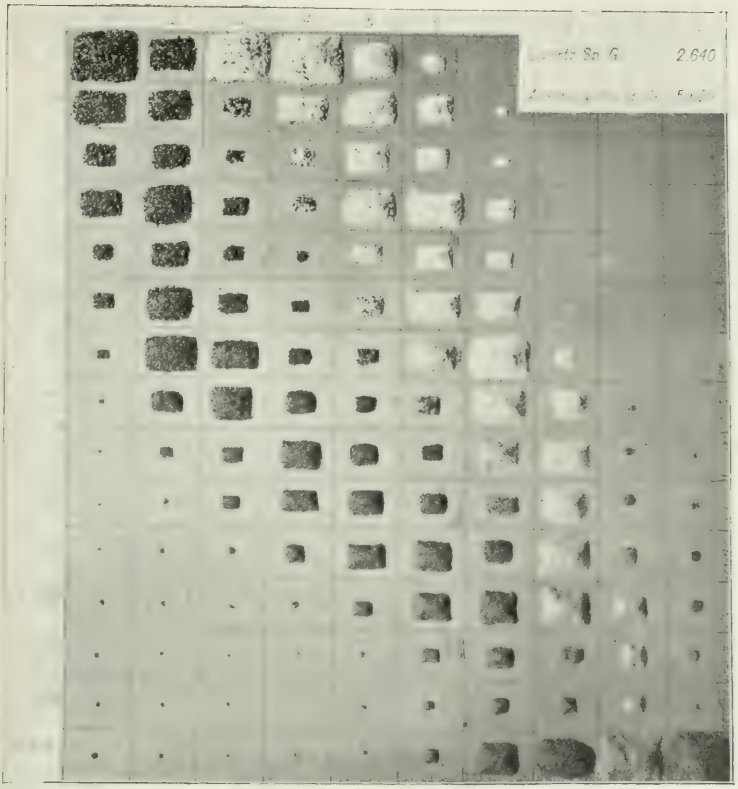
Separation of Quartz and Cassiterite by Pointed Tube and Sieves.

TABLE XIV.—*Estimated Percentage of Cassiterite in the Hills of Plate V.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	100	10	2	.....	.....	.....	.....	.....	.....
14	100	100	75	4	.....	.....	.....	.....	.....	.....
16	100	100	95	6	.....	.....	.....	.....	.....	.....
18	100	100	100	10	.....	.....	.....	.....	.....	.....
20	100	100	100	50	.....	.....	.....	.....	.....	.....
24	100	100	100	90	2	.....	.....	.....	.....	.....
30	100	100	100	100	40	.....	.....	.....	.....	50
40	100	100	100	100	97	5	.....	.....	.....	50
50	100	100	100	100	100	40	.....	.....	2	50
60	100	100	100	100	100	90	4	.....	2	50
80	100	100	100	100	100	100	30	.....	2	50
100	100	100	100	100	100	100	80	30	2	50
120	100	100	100	100	100	100	100	40	2	50
140	100	100	100	100	100	100	100	50	2	50
Slimes	100	100	100	100	100	100	100	75	40	75

For the weights of the hills in columns 5, 6 and 7, see Table XXX.

PLATE VI.



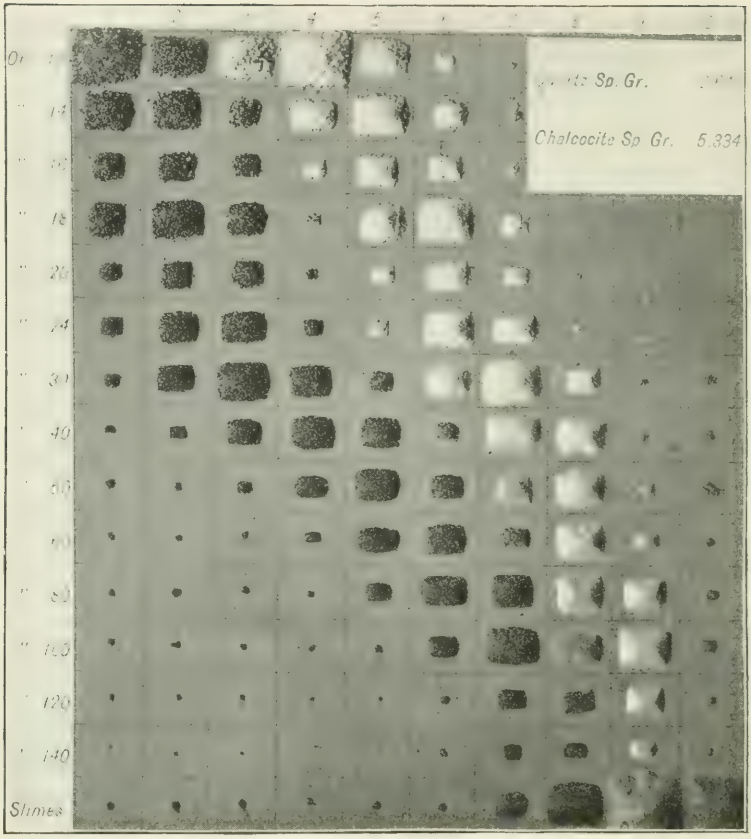
Separation of Quartz and Arsenopyrite by Pointed Tube and Sieves.

TABLE XV.—*Estimated Percentage of Arsenopyrite in the Hills of Plate VI.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	100	5	.....	.....	.....	.....	.....	.....	.....
14	100	100	40	.....	.....	.....	.....	.....	.....	.....
16	100	100	95	5	2	.....	.....	.....	.....	.....
18	100	100	99	15	4	1	.....	.....	.....	.....
20	100	100	100	90	6	1	.....	.....	.....	.....
24	100	100	100	99	15	2	1	.....	.....	.....
30	100	100	100	100	80	10	2	1	.....	.....
40	100	100	100	100	98	70	10	1	25	.....
50	100	100	100	100	100	95	25	2	50	50
60	100	100	100	100	100	100	45	5	25	50
80	100	100	100	100	100	100	80	10	15	75
100	100	100	100	100	100	100	100	15	15	75
120	100	100	100	100	100	100	100	30	15	60
140	100	100	100	100	100	100	100	50	20	50
Slimes	100	100	100	100	100	100	100	90	50	50

For the weights of the hills in columns 5, 6 and 7, see Table XXXI.

PLATE VII.



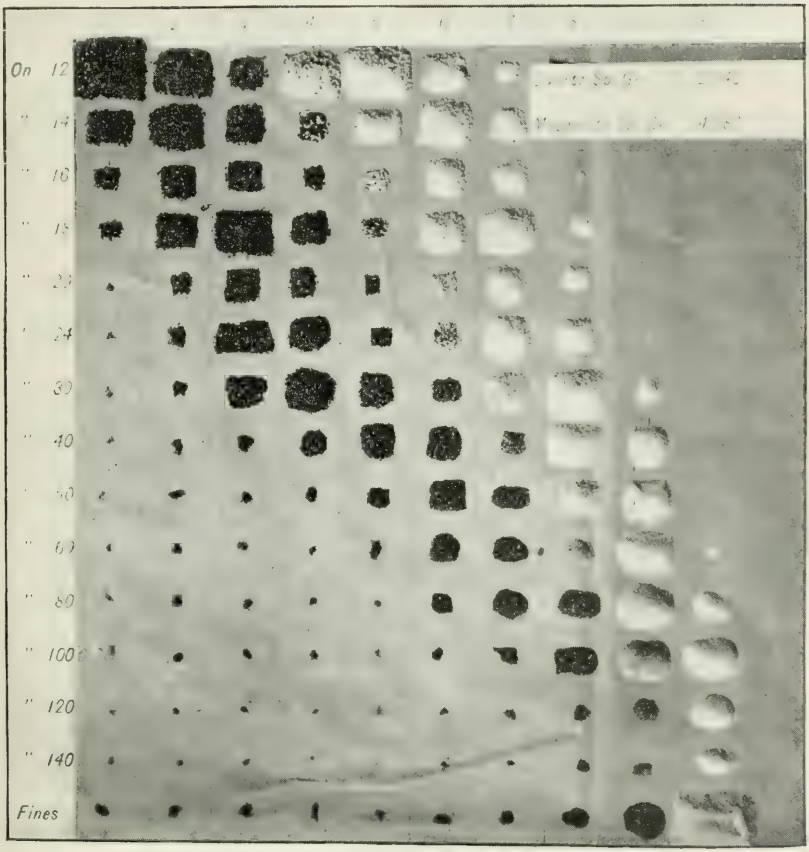
Separation of Quartz and Chalcocite by Pointed Tube and Sieves.

TABLE XVI.—*Estimated Percentage of Chalcocite in the Hills of Plate VII.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	100	10	.....	.....	.....	33	.....	.....	.....
14	100	100	90	.....	.....	.....	20	.....	.....	.....
16	100	100	100	2	.....	1	5	.....	.....	.....
18	100	100	100	50	.....	1	5	.....	.....	.....
20	100	100	100	100	1	1	5	50	.....	.....
24	100	100	100	100	35	1	5	30	.....	.....
30	100	100	100	100	95	20	15	2	90	100
40	100	100	100	100	100	60	50	2	30	99
50	100	100	100	100	100	99	75	2	5	99
60	100	100	100	100	100	100	95	2	5	99
80	100	100	100	100	100	100	100	30	5	90
100	100	100	100	100	100	100	100	50	5	85
120	100	100	100	100	100	100	100	95	10	80
140	100	100	100	100	100	100	100	100	15	80
Slimes	100	100	100	100	100	100	100	100	40	75

For the weights of the hills in columns 5, 6 and 7, see Table XXXII.

PLATE VIII.

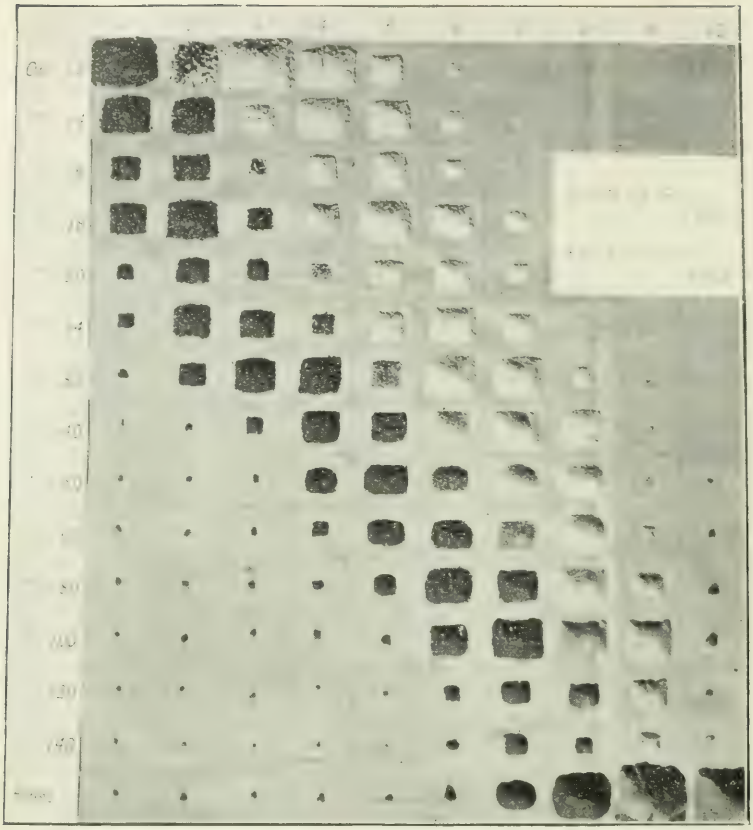


Separation of Quartz and Magnetite by Pointed Tube and Sieves.

TABLE XVII.—*Estimated Percentage of Magnetite in the Hills of Plate VIII.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	100	100	15	.....	.....	.....	.....	.....	.....
14	100	100	100	80	2	.....	.....	.....	.....	.....
16	100	100	100	100	40	.....	.....	.....	.....	.....
18	100	100	100	100	75	5	.....	.....	.....	.....
20	100	100	100	100	100	40	.....	.....	.....	.....
24	100	100	100	100	100	70	2	.....	.....	.....
30	100	100	100	100	100	100	50	.....	.....	.....
40	100	100	100	100	100	100	90	5	.....	60
50	100	100	100	100	100	100	100	35	.....	50
60	100	100	100	100	100	100	100	50	.....	15
80	100	100	100	100	100	100	100	100	10	.....
100	100	100	100	100	100	100	100	100	50	.....
120	100	100	100	100	100	100	100	100	90	.....
140	100	100	100	100	100	100	100	100	100	5
Slimes	100	100	100	100	100	100	100	100	100	35

PLATE IX.



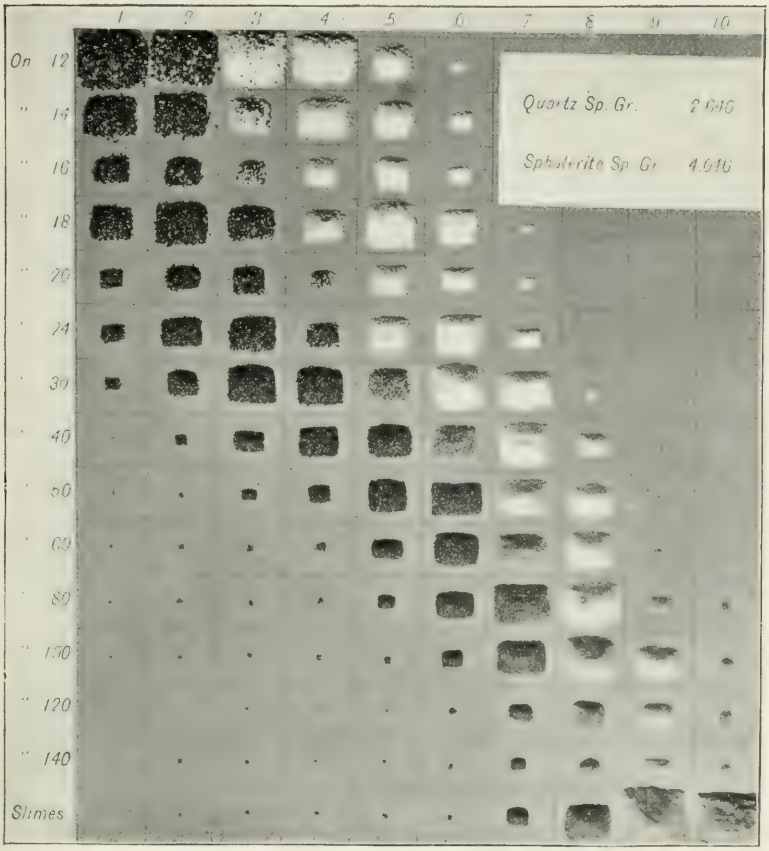
Separation of Quartz and Pyrrhotite by Pointed Tube and Sieves.

TABLE XVIII.—*Estimated Percentage of Pyrrhotite in the Hills of Plate IX.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	50	1	.....	.....	.....	.....	.....	.....	.....
14	100	100	10	1	.....	.....	.....	.....	.....	.....
16	100	100	50	2	1	.....	.....	.....	.....	.....
18	100	100	95	25	1	.....	.....	.....	.....	.....
20	100	100	100	50	1	1	.....	.....	.....	.....
24	100	100	100	90	10	1	1	.....	.....	.....
30	100	100	100	98	50	10	2	2	40	.....
40	100	100	100	100	98	50	5	4	40	.....
50	100	100	100	100	100	90	10	4	35	40
60	100	100	100	100	100	98	50	5	25	40
80	100	100	100	100	100	100	90	10	20	50
100	100	100	100	100	100	100	100	50	25	50
120	100	100	100	100	100	100	100	98	25	50
140	100	100	100	100	100	100	100	100	25	50
Slimes	100	100	100	100	100	100	100	100	65	60

For the weights of the hills in columns 5, 6 and 7, see Table XXXIII.

PLATE X.



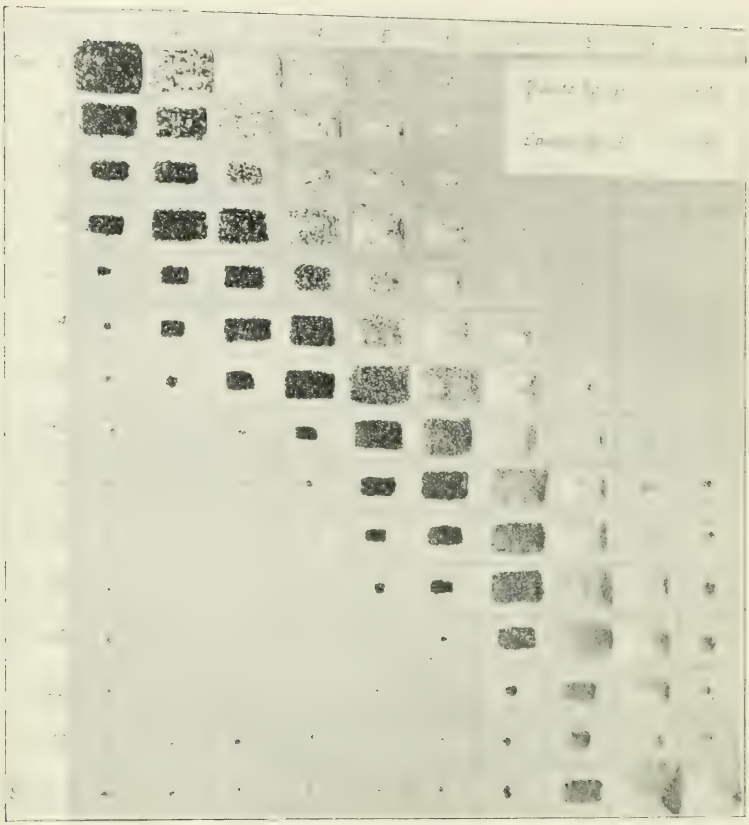
Separation of Quartz and Sphalerite by Pointed Tube and Sieves.

TABLE XIX.—*Estimated Percentage of Sphalerite in the Hills of Plate X.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	99.5	80	10	.....	.....	.....	.....	.....	.....	.....
14	100	99	40	.....	.....	.....	.....	.....	.....	.....
16	100	100	60	2	.....	.....	.....	.....	.....	.....
18	100	100	80	10	.....	.....	.....	.....	.....	.....
20	100	100	99	50	2	.....	.....	.....	.....	.....
24	100	100	100	85	10	1	.....	1	.....	.....
30	100	100	100	99	50	10	2	1	.....	.....
40	100	100	100	100	98	50	5	1	25	.....
50	100	100	100	100	100	90	20	2	60	.....
60	100	100	100	100	100	99	50	5	70	.....
80	100	100	100	100	100	100	75	15	50	90
100	100	100	100	100	100	100	95	40	40	90
120	100	100	100	100	100	100	100	50	40	90
140	100	100	100	100	100	100	100	60	40	90
Slimes	100	100	100	100	100	100	100	85	60	85

For the weights of the hills in columns 5, 6 and 7, see Table XXXIV.

PLATE XI.



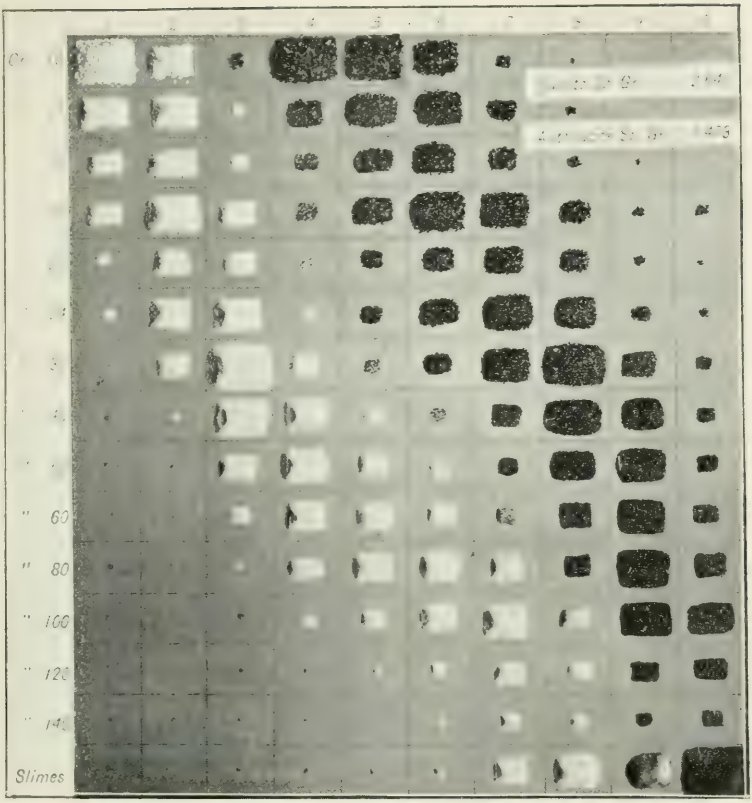
Separation of Quartz and Epidote by Pointed Tube and Sieves.

TABLE XX.—*Estimated Percentage of Epidote in the Hills of Plate XI.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	95	50	3	.....	.....	.....	.....	.....	.....	.....
14	100	90	20	3	.....	.....	.....	.....	.....	.....
16	100	95	50	5	.....	.....	.....	.....	.....	.....
18	100	100	80	40	5	.....	.....	.....	.....	.....
20	100	100	100	70	25	.....	.....	.....	.....	.....
24	100	100	100	97	50	10	.....	.....	.....	.....
30	100	100	100	100	75	50	3	3	50	.....
40	100	100	100	100	95	70	25	5	50	.....
50	100	100	100	100	100	95	50	10	50	.....
60	100	100	100	100	100	100	60	25	25	95
80	100	100	100	100	100	100	75	50	25	95
100	100	100	100	100	100	100	100	75	25	75
120	100	100	100	100	100	100	100	90	50	50
140	100	100	100	100	100	100	100	97	50	40
Slimes	100	100	100	100	100	100	100	100	50	25

For the weights of the hills in columns 5, 6 and 7, see Table XXXV.

PLATE XII.



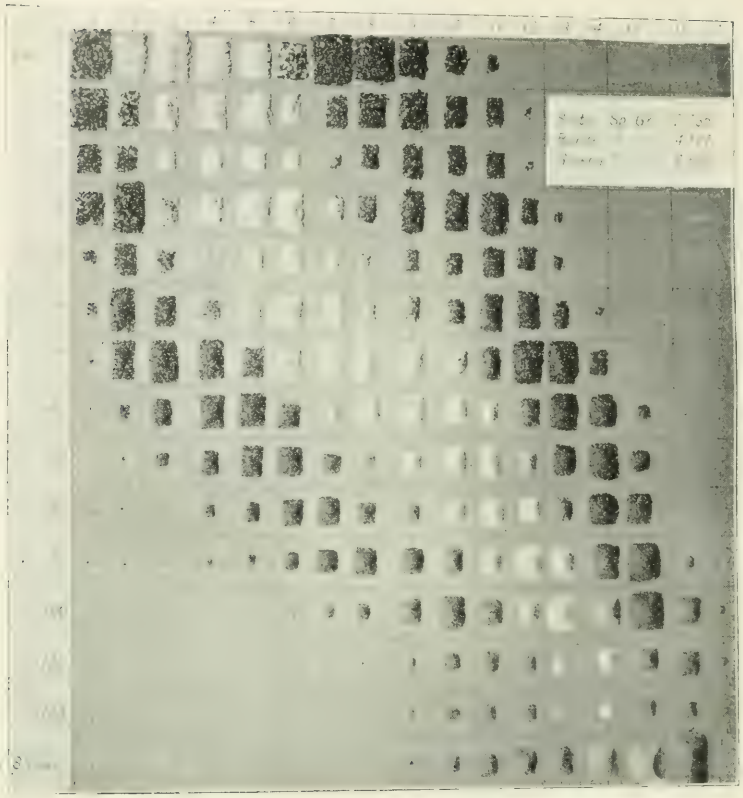
Separation of Quartz and Anthracite by Pointed Tube and Sieves.

TABLE XXI.—*Estimated Percentage of Anthracite in the Hills of Plate XII.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12			80	99	100	100	100	100		
14			5	98	100	100	100	100		
16			1	90	100	100	100	100	100	
18				65	100	100	100	100	100	100
20				45	100	100	100	100	100	100
24				5	100	100	100	100	100	100
30				2	50	100	100	100	100	100
40					4	50	100	100	100	100
50						5	95	100	100	100
60							40	100	100	100
80							5	95	100	100
100								4	100	100
120								1	95	98
140									90	98
Slimes									35	60

For the weights of the hills in columns 5, 6 and 7, see Table XXXVI.

PLATE XIII.

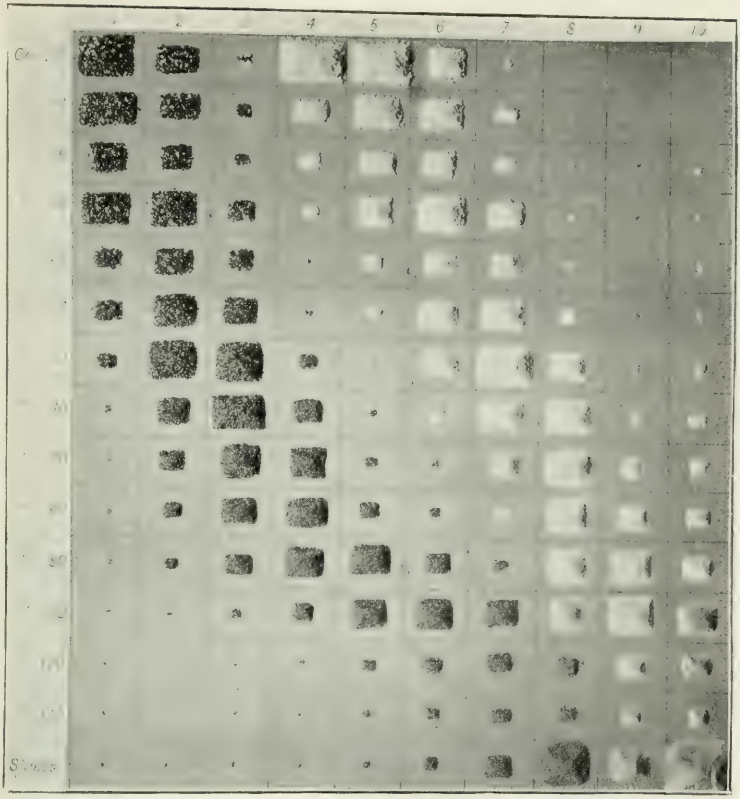


Separation of Slate, Barite and Galena by Pointed Tube and Sieves.

TABLE XXII.—*Estimated Percentage of Barite in the Hills of Plate XIII.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
12	.....	80	100	100	100	50	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
14	.....	10	99	100	100	98	5	1	1	.....	.....	.....	.....	.....	.....	.....	.....
16	.....	.....	95	100	100	100	50	2	1	.....	.....	.....	.....	.....	.....	.....	.....
18	.....	.....	50	95	100	100	95	20	1	1	.....	.....	.....	.....	.....	.....	.....
20	.....	.....	10	75	99	100	100	80	10	1	.....	.....	.....	.....	.....	.....	.....
24	.....	.....	.....	50	90	100	100	95	45	5	1	1	.....	.....	.....	.....	.....
30	.....	.....	.....	2	25	80	100	100	90	90	5	1	1	.....	.....	.....	.....
40	.....	.....	.....	.....	.....	10	75	100	100	100	95	5	1	1	1	.....	.....
50	.....	.....	.....	.....	.....	.....	15	60	99	100	100	75	5	1	2	.....	.....
60	.....	.....	.....	.....	.....	.....	.....	2	40	90	100	98	20	2	2	.....	.....
80	.....	.....	.....	.....	.....	.....	.....	.....	2	25	90	100	90	5	5	10	.....
100	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	5	60	98	55	5	10	40
120	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	5	80	98	10	10	20
140	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	50	98	20	5	20
Slimes	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	30	50	75	20	10

PLATE XIV.



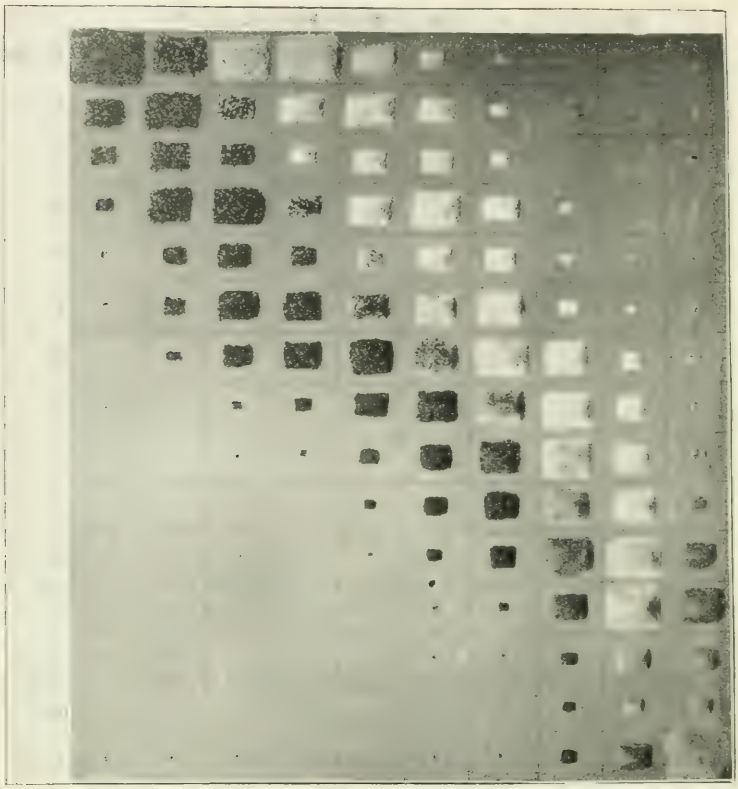
Separation of Quartz (Sp. Gr. 2.64) and Galena (Sp. Gr. 7.586) by Pulsion-Jig and Sieves.

TABLE XXIII.—*Estimated Percentage of Galena in the Hills of Plate XIV.*

Sieve-mesh.	1	2	3	4	5	6	7	8	9	10
12	100	100	50	.....	.....	.....	.....	.....	.....	.....
14	100	100	99	.....	.....	.....	.....	.....	.....	.....
16	100	100	100	.....	.....	.....	.....	.....	.....	.....
18	100	100	100	2	.....	.....	.....	.....	.....	.....
20	100	100	100	50	.....	.....	.....	.....	.....	10
24	100	100	100	90	.....	.....	.....	.....	.....	15
30	100	100	100	100	5	.....	.....	.....	.....	15
40	100	100	100	100	95	5	.....	.....	.....	15
50	100	100	100	100	100	50	3	.....	.....	10
60	100	100	100	100	100	100	5	1	.....	10
80	100	100	100	100	100	100	90	3	.....	5
100	100	100	100	100	100	100	100	40	5	50
120	100	100	100	100	100	100	100	60	10	40
140	100	100	100	100	100	100	100	80	10	40
Slimes	100	100	100	100	100	100	100	100	60	50

For the weights of the hills in columns 5, 6 and 7, see Table XL.

PLATE XV.



Separation of Quartz (Sp. Gr. 2.64) and Sphalerite (Sp. Gr. 4.046) by Pulsion-Jig and Sieves.

TABLE XXIV.—*Estimated Percentage of Sphalerite in the Hills of Plate XV.*

Sieve-mesh	1	2	3	4	5	6	7	8	9	10
12	100	80	5	.....	.....	.....	.....	.....	.....	.....
14	100	100	50	.....	.....	.....	.....	.....	.....	.....
16	100	100	90	3	.....	.....	.....	.....	.....	40
18	100	100	100	50	5	1	.....	.....	.....	15
20	100	100	100	95	40	1	.....	.....	.....	50
24	100	100	100	100	75	20	1	.....	.....	10
30	100	100	100	100	98	50	5	1	.....	40
40	100	100	100	100	100	95	50	3	1	40
50	100	100	100	100	100	100	90	30	2	40
60	100	100	100	100	100	100	100	40	5	50
80	100	100	100	100	100	100	100	60	30	75
100	100	100	100	100	100	100	100	75	40	70
120	100	100	100	100	100	100	100	100	50	60
140	100	100	100	100	100	100	100	100	50	50
Slimes	100	100	100	100	100	100	100	100	60	50

For the weights of the hills in columns 5, 6 and 7, see Table XLI.

TABLE XXV.—*Quartz and Copper* (*Plate* I.).

Sieve. Mesh.	Bulb, No. 5.		Bulb, No. 6.		Bulb, No. 7.	
	Copper, Grammes.	Quartz, Grammes.	Copper, Grammes.	Quartz, Grammes.	Copper, Grammes.	Quartz, Grammes.
12.....	.....	10.7193	.....	2.0011	.....	.....
14.....	.....	6.3910	.0452	4.4756	.....	.1486
16.....	.....	3.1724	.0270	2.6766	.....	.2097
18.....	.....	2.5210	.1236	12.2374	.....	2.3365
20.....	.....	0.1277	.0277	2.7450	.....	1.7427
24.....	.0010	.0476	.0348	3.4430	.....	6.3275
30.....	.0305	.0009	.1278	1.1504	.....	11.2542
40.....	.2008	.....	.2048	.0108	.0207	2.0472
50.....	.6324	.....	.2115	.0002	.0147	.1947
60.....	1.8367	.....	.2851	.....	.0179	.0219
80.....	12.8308	.....	2.4554	.....	.1117	.0062
100.....	19.6367	.....	12.2217	.....	2.2148	.0224
120.....	6.8088	.....	9.5750	.....	5.4133	.....
140.....	1.6049	.....	2.0052	.....	1.1046	.....
Slimes.	5.2685	.....	16.8152	.....	39.3308	.....
Total...	48.8511	22.9799	44.1600	28.7401	48.2285	24.3116

was obtained, the relative proportions of the two minerals were estimated by the eye, and the total weight was divided between them in accordance with this estimate.

In order to throw light upon the question of interstitial currents, it was necessary to obtain the ratio of diameters for the particles of quartz and galena, for example, when the two minerals had arrived at equilibrium through the action of the upward current. The values given in Tables XXV. to XXXVI. for the whole series of minerals were used for this purpose.

The particles in any given column may be assumed to be in equilibrium because the light and heavy materials there drawn out together have had ample time to choose their partners.

The mode of computation for any given column or bulb, may be shown by taking as an example Plate II. *b*, and bulb No. 5 of

TABLE XXVI.—*Quartz and Galena* (*Plate* II. *b*.).

SIEVE.	BULB, No. 5.		BULB, No. 6.		BULB, No. 7.	
Mesh.	Galena, Grammes.	Quartz, Grammes.	Galena, Grammes.	Quartz, Grammes.	Galena, Grammes.	Quartz, Grammes.
12.....	.....	8.281	.....	0.0880	.....	.....
14.....	.....	11.713	.....	1.447	.....	.....
16.....	.....	5.525	.....	1.946	.....	0.0943
18.....	.....	6.412	.....	10.582	.....	0.3535
20.....	.....	0.4273	.....	4.436	.....	0.5507
24.....	.....	0.1306	.....	6.790	.....	3.465
30.....	.....	0.0129	.....	3.268	.....	12.982
40. ...	0.0339	.....	.....	0.0570	.....	5.897
50.....	0.2403	.....	0.0485	.....	.....	1.633
60.....	1.696	.....	0.1170	.....	.....	0.1737
80.....	16.335	.....	4.918	.....	0.1950	.....
100.....	7.705	.....	21.141	.....	2.955	.....
120.....	0.2278	.....	4.523	.....	3.738	.....
140.....	0.0580	.....	2.378	.....	3.382	.....
Slimes.	0.1310	.....	1.902	.....	25.040	.....
Total...	26.4270	32.5018	35.0275	28.6140	35.3100	25.1492

Table XXVI. Here the average diameter of the quartz particles was obtained by multiplying all the quartz-weights in column 5 by their diameters, and dividing the sum of the products by the sum of the weights. The galena figures in column 5, treated similarly, give an average diameter for the galena particle. This diameter of quartz is then divided by the diameter of the galena. In like manner computations were made upon all of the eleven minerals, and these interstitial factors are given for all the minerals except magnetite in Table XXXVII.

Lest the proportions of the two minerals (equal volumes) used in the pointed-tube tests (Plates I. to XII.) might have influenced the results, a trial-test for comparison with Plate II. *b*, was made, using a quantity of galena equal to about one-sixteenth of the volume of

TABLE XXVII.—*Quartz and Galena (Ratio 16 : 1 by Volume).*

Sieve, Mesh.	Bulb, No. 4.	
	Quartz, Grammes.	Galena, Grammes.
12	3.598	.....
14	3.744	.....
16	2.295	.....
18	5.791	.....
20	1.320	.....
24	1.916	.....
30	0.880	0.098
40	0.0336	0.0112
50	0.0017	0.0323
60	.....	0.1152
80	.....	0.723
100	.....	1.409
120	.....	0.233
140	.....	0.0576
Slimes	.....	0.0737
Total	19.5793	2.7530

quartz, instead of equal volumes of the two minerals. The 4th, 5th and 6th bulbs were sized, and gave hills apparently at the same points as shown in Plates II. *a*, and II. *b*. To demonstrate the point still further, weights and computations were made upon the 4th bulb (Table XXVII.), and yielded the ratio of the diameter of the galena particle to that of the quartz,

1 : 5.966.

This ratio is practically the same as those given in Table XXXVII. for galena and quartz, and therefore demonstrates that the relative quantities of the two minerals have nothing to do with the law of interstitial currents. The ratio of diameter is fixed.

The foregoing experiments show that, under the conditions existing in the pointed tube, the diameter-ratio of galena and quartz in equi-

librium together, when interstitial currents have done their whole work, is about 1 : 6 for particles ranging between 10-mesh and 100-mesh. They further show that interstitial currents have a real existence, and have decidedly advanced the ratio from the equal-settling particles of the free-falling test which is given by Rittinger as 1 : 4, and which has been found in this investigation for 10- to 12-mesh quartz as 1 : 3.75. My tests do not, however, substantiate the claim made by Prof. Munroe, that galena and quartz will not come to equilibrium in interstitial currents until a ratio of 1 : 30 for their diameters has been reached.

Fig. 6.

Reversible Free-fall Tube.

One or two interesting facts may be noted here, although they are one side from the main thread of this paper.

In the first galena trial (Plate II. *a*) it was found that fine galena appeared in the first drawing below 30-mesh. This may be attributed wholly to particles abraded during the subsequent sifting operation. To test the question, the galena-quartz lot was mixed up thoroughly and run over again (Plate II. *b*); and this time the fine galena, below the main range of galena hills is much reduced, proving the conjecture to be substantially correct.

Again, the fall-velocities of these different heaps were taken in a tube, Fig. 6, designed by C. Le Neve Foster, in which by inverting the tube the measure may be taken over and over. The results are here given. They show that, for example, on the 18-mesh line, grains of galena in No. 1 are faster than No. 2, and No. 2 than No. 3; also that the quartz Nos. 4, 5, 6 and 7 fall in that order, 4 being the fastest, and 7 the slowest, 4 containing the 18-mesh grains that are nearest to a cube, and 7 being the flat oyster shells that fall much slower. Results of these trials are given in Table XXXVIII.

In this test a group of 20 or 30 grains was timed. When the average grain passed the upper and lower marks the time was taken. The results are therefore averages. In the light of these facts the remarkable resemblance between these two tests of galena and quartz (Plates II. *a* and II. *b*) becomes more interesting; for it

TABLE XXVIII.—*Quartz and Wolframite* (*Plate* III.).

SIEVE, Mesh.	BULB, No. 5.		BULB, No. 6.		BULB, No. 7.	
	Wolframite, Grammes.	Quartz, Grammes.	Wolframite, Grammes.	Quartz, Grammes.	Wolframite, Grammes.	Quartz, Grammes.
12.....	.....	5.0224	.....	0.0723	.....	.....
14.....	.....	8.4741	.....	0.7525	.....	.....
16.....	.....	5.2412	.....	1.0360	.....	0.0342
18.....	.....	9.1827	.....	7.5598	.....	0.2932
20.....	.....	1.3058	.....	3.5209	.....	0.3887
24.....	.....	1.1077	.....	8.2641	.....	2.4168
30.....	0.0418	0.3766	.....	6.8889	.....	10.4951
40.....	0.2220	0.0392	0.0453	0.8611	.....	10.0156
50.....	0.8650	0.0455	0.0499	0.1977	.....	4.7618
60.....	3.7694	.....	0.3763	0.0418	0.0173	1.5664
80.....	12.1549	.....	3.9501	0.2079	0.0690	0.5311
100.....	6.8694	.....	15.6509	.....	1.5409	0.1865
120.....	0.3685	.....	3.2758	.....	1.7262	.....
140.....	0.0911	.....	1.4869	.....	0.7340	.....
Slimes..	0.0886	.....	6.1610	.....	18.2780	.....
Total...	24.4707	30.7952	30.9962	29.4030	22.3654	30.6894

is probable that, with certain latitude, the particles of the first test found their way back to the same identical heap in the second test. This principle of almost absolute predestination of particles for their own appointed places in ore-dressing is very important. It was mentioned by me in Chicago last summer, in discussing the paper of Oberbergrath Bilharz, under the heading "Once Middlings, Always Middlings" (*Trans.*, xxii., 700).

It will be noticed that the light color of the slimes of No. 9, in Plates I. to XII. is almost always marked when compared with its neighbors on the right and left of it. This being a truly sorted product, the finer dark mineral hides itself beneath the coarser light mineral. This shows in all the sets except copper, which had no slimes, magnetite, which had almost none, and epidote, the fine powder of which is extremely light colored, and of which the 10th imes are therefore as light as the 9th.

TABLE XXIX.—*Quartz and Antimony* (*Plate* IV.).

Sieve, Mesh.	Bulb, No. 5.		Bulb, No. 6.		Bulb, No. 7.	
	Antimony, Grammes.	Quartz, Grammes.	Antimony, Grammes.	Quartz, Grammes.	Antimony, Grammes.	Quartz, Grammes.
12.....	.....	10.8059	.....	0.4286	.....	.....
14.....	.....	10.5023	.....	2.3922	.....	0.0973
16.....	.....	3.8306	.....	2.7134	.....	0.2042
18.....	.....	4.5723	.....	9.6878	.....	2.1545
20.....	.....	0.4613	.....	2.9766	.....	1.7760
24.....	.....	0.2330	.....	3.9996	.....	5.8914
30.....	0.0377	0.0377	.....	1.5920	.....	12.5619
40.....	0.8874	.....	0.0390	0.0390	.....	4.3622
50.....	6.8722	.....	0.0316	0.5894	.....	1.1711
60.....	10.1563	.....	3.1825	.....	0.0992	0.1488
80.....	8.5035	.....	14.3174	.....	1.0615	0.1180
100.....	1.2058	.....	13.5745	.....	9.6491	.....
120.....	.....	.....	1.3978	.....	4.6588	.....
140.....	.....	.....	0.3979	.....	1.5357	.....
Slimes..	.....	.....	0.9932	.....	14.3481	.....
Total...	27.6629	30.4431	33.9339	24.4186	31.3524	28.4854

The ultimate effect of an upward current of water upon sands of two specific gravities is shown in Plates I. to XIII. inclusive. This, then, is the ultimate equilibrium, which is approached but never quite reached by the pulsion-movement of a jig. The ideas conveyed by these photographs may be represented geometrically by Fig. 7, in which each cone stands for a column of particles of a single mineral species, as they would be arranged in an upward current of water. The heaviest grain with the largest diameter is represented by the diameter of the base of the cone, *m*; the lightest and smallest is represented by the apex of the cone, *n*. Two cones placed together, as *a* and *b*, represent two minerals of different specific gravity treated together in a pointed tube. Their vertices will be together, because the finest dust of the two will settle at almost the same rate; that is, with almost no velocity.

The largest particles, represented by the bases of the two cones,

TABLE XXX.—*Quartz and Cassiterite* (*Plate* V.).

SIEVE, Mesh.	BULB, No. 5.		BULB, No. 6.		BULB, No. 7.	
	Cassiterite, Grammes.	Quartz, Grammes.	Cassiterite, Grammes.	Quartz, Grammes.	Cassiterite, Grammes.	Quartz, Grammes.
12	.....	4.0757	.....	0.0274	.....	.....
14	.....	7.3256	.....	0.4636	.....	.....
16	.....	5.0848	.....	0.6899	.....	0.0094
18	.....	10.2372	.....	6.1156	.....	0.2319
20	.....	1.5721	.....	3.4824	.....	0.3015
24	0.0241	1.1806	.....	7.7527	.....	1.6953
30	0.2061	0.3092	.....	7.8932	.....	10.8524
40	0.3305	0.0102	0.0551	1.0469	.....	8.7152
50	1.2270	.....	0.0864	0.1296	.....	4.8594
60	6.6046	.....	0.9265	0.1030	0.0768	1.8441
80	12.2446	.....	9.3352	.....	0.3125	0.7292
100	4.3263	.....	16.2333	.....	3.5888	0.8972
120	0.3272	.....	2.3566	.....	2.4621	.....
140	0.0748	.....	0.6666	.....	0.7038	.....
Slimes	0.1580	.....	4.6162	.....	15.6383	.....
Total	25.5232	29.7954	34.2759	27.7043	22.7823	30.1356

Fig. 7.

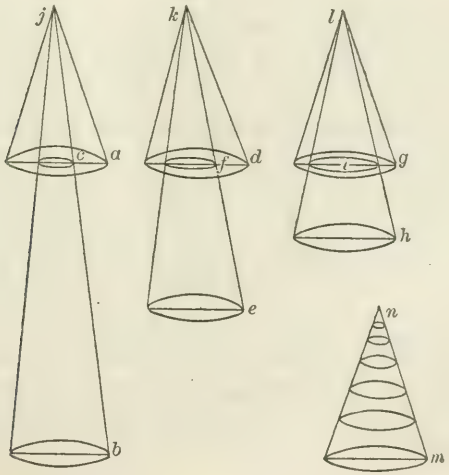


TABLE XXXI.—*Quartz and Arsenopyrite* (*Plate* VI.).

Sieve, Mesh.	Bulb, No. 5.		Bulb, No. 6.		Bulb, No. 7.	
	Arsenopyrite, Grammes.	Quartz, Grammes.	Arsenopyrite, Grammes.	Quartz, Grammes.	Arsenopyrite, Grammes.	Quartz, Grammes.
12.....	.....	6.3982	....	1.0015	. .....	.....
14.....	.....	9.0563	.....	3.2585	.....	0.1371
16.....	0.0993	4.8676	.....	2.1703	.....	0.2182
18.....	0.3430	8.2314	0.0945	9.3590	.. ..	1.4163
20.....	0.0805	1.2618	0.0316	3.1256	.....	0.9750
24.....	0.2363	1.3387	0.1278	6.2628	0.0362	3.5838
30.....	0.7460	0.1867	0.5144	4.6292	0.2451	12.0106
40.....	0.9764	0.0199	0.8400	0.3600	0.7444	6.6991
50.....	2.9162	.....	0.9052	0.0476	0.9213	2.7638
60.....	5.3491	.....	2.4436	.....	0.8064	0.9856
80.....	6.0430	.....	7.6572	.....	2.1537	0.5384
100.....	0.8948	.....	6.9215	.....	7.7638	.....
120.....	.....	.....	0.7400	.....	2.5433	.....
140.....	....	.....	0.1858	.....	0.9098	.....
Slimes..	.....	.....	0.4976	.....	8.7290	.....
Total...	17.6846	31.3606	20.9592	30.2145	24.8530	29.3279

will settle at very different rates, and the relative lengths of the cones may be chosen to represent the relative positions in the pointed tube of the coarsest particles of each of the two minerals. For example, the cone, *b*, may represent galena, and *a*, quartz, as they would appear when in equilibrium in a pointed tube. The cones, *d* and *e*, may represent quartz and arsenopyrite, and *g* and *h* may represent quartz and epidote. From these main facts the conclusion is natural. The particle, *c*, of galena, which is adjacent to *a* of quartz, is very small. It is much smaller than the interstices between the quartz particles, *a*. The same relation holds between all the adjacent particles of cones, *aj* and *cj*. The particle, *f*, of arsenopyrite is larger than *e;* it is about equal to the interstices between the particles of quartz, *d;* and consequently all the particles of arsenopy-

TABLE XXXII.—*Quartz and Chalcocite* (*Plate* VII.).

SIEVE, Mesh.	BULB, No. 5.		BULB, No. 6.		BULB, No. 7.	
	Chalcocite, Grammes.	Quartz, Grammes.	Chalcocite, Grammes.	Quartz, Grammes.	Chalcocite, Grammes.	Quartz, Grammes.
12	.....	6.4328	.....	0.3836	0.0103	0.0212
14	.....	9.4660	.....	2.2082	0.0235	0.0938
16	.....	3.8929	0.0217	2.1473	0.0049	0.0925
18	.....	5.6878	0.0992	9.8193	0.0532	1.0115
20	0.0069	0.6846	0.0341	3.3762	0.0413	0.7848
24	0.1514	0.2812	0.0692	6.8519	0.1946	3.6983
30	1.1977	0.0630	0.8250	3.2998	1.6990	10.6278
40	6.1287	.....	0.7190	0.4794	3.0624	3.0624
50	8.8292	.....	3.2620	0.0329	1.8900	0.6300
60	4.9825	.....	6.0144	.....	1.7927	0.0944
80	1.5220	.....	8.0520	.....	7.2729	.....
100	0.0491	.....	2.3114	.....	11.3342	.....
120	.....	.....	0.0897	.....	1.7587	.....
140	.....	.....	0.0195	.....	0.7359	.....
Slimes	.....	.....	0.0505	.....	2.7702	.....
Total	22.8675	26.5083	21.5677	28.5986	32.6438	20.1167

rite in the little cone, *fk*, will bear the same relation to the adjacent quartz particles, they all being equal to the interstices in the cone, *dk*. The particles, *i*, of epidote will be larger than the interstices between the quartz particles, *g*, and this relation will hold all the way up for adjacent particles in the cones, *gl* and *il*.

The ultimate result of the pointed tube, the *Spitzlutte*, the Lake Superior separator, and of the pulsion action of the jig, is to associate together particles of the two minerals that are to be separated, in which the ratio of the grains lies in one of these three classes:

1. The lesser, higher specific gravity grain is smaller than the quartz interstices.

2. The lesser, higher specific gravity grain is equal to the quartz interstices.

3. The lesser, higher specific gravity grain is greater than the quartz interstices.

TABLE XXXIII.—*Quartz and Pyrrhotite* (*Plate* IX.).

SIEVE,	BULB, No. 4.		BULB, No. 5.		BULB, No. 6.	
Mesh.	Pyrrhotite, Grammes.	Quartz, Grammes.	Pyrrhotite, Grammes.	Quartz, Grammes.	Pyrrhotite, Grammes.	Quartz, Grammes.
12.....	.....	12.9000	.....	2.8578	.....	0.1936
14.....	0.0854	8.4606	.....	5.6642	.....	0.6740
16.....	0.0489	2.3944	0.0362	3.5878	.....	0.6461
18.....	0.7106	2.1317	0.0836	8.2714	.....	4.2311
20.....	0.3483	0.3483	0.0111	1.0969	0.0233	2.3082
24.....	1.3307	0.1478	0.2680	2.4115	0.0558	5.5242
30.....	9.5668	0.1950	1.8550	1.8550	0.8274	7.4467
40.....	6.9965	.....	4.4928	0.0917	1.6844	1.6844
50.....	3.3025	.....	5.5675	.....	2.6784	0.2976
60.....	0.5011	.....	3.8000	.....	5.0036	0.1021
80.....	.....	.....	1.2862	.....	8.5450	....
100.....	.....	.....	.....	.....	4.9831	.....
120.....	.....	.....	.....	.....	0.4192	.....
140.....	.....	.....	.....	.....	0.1300	.....
Slimes..	.....	.....	.....	.....	0.1617	.....
Total...	22.8908	26.5778	17.4004	25.8363	24.5119	23.1080

These laws are important, and should be recognized and duly considered when mills are planned.

III.—ACCELERATION.

Rittinger, having found that jigs save galena of smaller sizes than his formula of 1866 allows, worked out in his appendix of 1870 the theory of acceleration to account for that fact, showing that a particle of galena which is equal-settling with a particle of quartz reaches its maximum velocity in perhaps one-tenth the time required by the quartz. The oft-repeated pulsations of a jig give the galena particles a decided advantage over the quartz, placing beside the quartz, when equilibrium is reached, a much smaller particle of galena than we should expect according to the law of equal-settling particles. He concludes that the excess of jigging-power over that

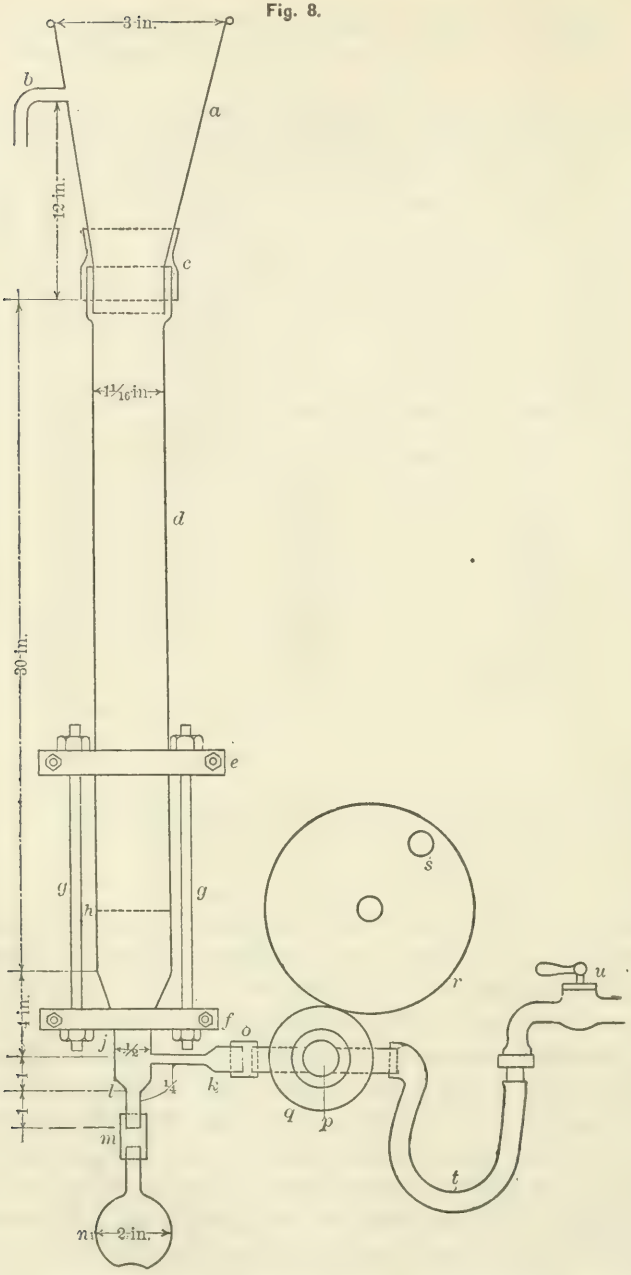
TABLE XXXIV.—*Quartz and Sphalerite* (*Plate* X.).

Sieve, Mesh.	Bulb, No. 5.		Bulb, No. 6.		Bulb, No. 7.	
	Sphalerite, Grammes.	Quartz, Grammes.	Sphalerite, Grammes.	Quartz, Grammes.	Sphalerite, Grammes.	Quartz, Grammes.
12.....	.....	3.4518	.....	0.1900	.....	.....
14.....	.....	4.4504	.....	0.5707	.....	.....
16.....	.....	3.1366	.....	0.4898	.....	.....
18.....	.....	8.7064	.....	3.9642	.....	0.0922
20.....	0.0539	2.6426	.....	1.8967	.....	0.1643
24.....	0.3923	3.5302	0.0665	6.5786	.....	0.9541
30.....	2.6226	2.6226	0.1017	10.0720	0.1247	6.1078
40.....	6.3358	0.1293	2.7952	2.7952	0.4212	8.0019
50.....	4.6660	.....	5.9173	0.6575	1.3300	5.3187
60.....	1.5942	.....	5.6285	0.0569	3.1622	3.1622
80.....	0.3894	.....	3.7332	.....	6.4017	2.1339
100.....	.....	.....	0.6867	.....	6.4078	0.3373
120.....	.....	.....	.....	.....	0.6962	.....
140.....	.....	.....	.....	.....	0.2921	.....
Slimes..	....	.....	.....	.....	0.8976	.....
Total...	16.0542	28.6699	18.9291	27.2716	19.7335	26.2724

indicated by the law of equal-settling particles is due to acceleration. Unfortunately, he has not given a ratio of diameters of quartz and galena, which represents equilibrium with regard to acceleration.

To test this question of acceleration, I have designed a pulsion-jig or modified *Setzpumpe*, which is shown in Fig. 8. It consists of a tin funnel, *a*, with overflow, *b*, connected by rubber connector, *c*, to a glass tube, *d*, cut apart at *h* for the insertion of a disk of sieve-cloth. The two parts are held together by two clamps, *e* and *f*, and two bolts, *g*, *g*, and the leaky joint, at *h*, is made tight by a belt of rubber plaster. The tube has a branch, *k*, joined by rubber connector, *o*, to a common plug-cock, *p*, provided with a gear-wheel, *q*, which intermeshes with a larger gear, *r*, having a crank, *s*, turned by hand. Water is supplied through the rubber hose, *t*, and the hydrant, *u*. The lower end of the tube is drawn down to one-quarter inch in

Fig. 8.



Pulsion-Jig.

diameter at *l*, and by rubber connector, *m*, is joined to a bulb, *n*, for receiving what passes through the sieve.

TABLE XXXV.—*Quartz and Epidote* (*Plate* XI.).

SIEVE, Mesh.	BULB, No. 5.		BULB, No. 6.		BULB, No. 7.	
	Epidote, Grammes.	Quartz, Grammes.	Epidote, Grammes.	Quartz, Grammes.	Epidote, Grammes.	Quartz, Grammes.
12	.....	1.1376	.....	0.0858	.....	.....
14	.....	2.6366	.....	0.4537	.....	.....
16	.....	2.1569	.....	0.4406	.....	.....
18	0.4433	8.4234	.....	3.3701	.....	0.1179
20	0.7673	2.3020	.....	2.1856	.....	0.1693
24	2.7109	2.7109	0.5945	5.3501	.....	0.8444
30	8.7481	2.9161	6.1561	6.1561	0.2146	6.9373
40	5.0263	0.2645	5.8478	2.5062	2.1476	6.4429
50	1.6755	.....	5.0005	0.2632	4.6980	4.6980
60	0.4066	.....	1.9449	.....	4.1074	2.7382
80	0.0816	.....	0.4786	.....	5.0657	1.6886
100	.....	.....	.....	.....	2.2078	.....
120	.....	.....	.....	.....	0.1084	.....
140	.....	.....	.....	.....	.....	.....
Slimes	.....	.....	.....	.....	.....	.....
Total	19.8596	22.5480	20.0224	20.8114	18.5495	23.6366

The method of operating this pulsion-jig is simply to turn on the water gently at *u*, and revolve the crank, *s*, at the speed desired. The revolution of the plug-cock, *p*, makes and breaks the water-connection, and the rubber tube, *t*, is elastic enough to act as an accumulator for the instant that the water is shut off. The sand fed in at the funnel, *a*, quickly falls to the sieve, *h*, and then receives a series of intermittent upward pulsations from the movement of the water. The sand is therefore subjected to an upward current of water at one instant, which remains stagnant the next instant. These pulsations may be given at almost any rate up to 800 per minute.

This instrument seems well calculated to answer the question. Does Rittinger's acceleration, due to intermittent upward pulsations, add anything to the effect of Munroe's interstitial currents?

TABLE XXXVI.—*Quartz and Anthracite* (*Plate* XII.).

SIEVE,	BULB, No. 6.		BULB, No. 7.		BULB, No. 8.	
Mesh.	Quartz, Grammes.	Anthracite, Grammes.	Quartz, Grammes.	Anthracite, Grammes.	Quartz, Grammes.	Anthracite, Grammes.
12.....	.....	2.5000	.....	0.0732	.....	0.0022
14.....	.....	2.7538	.....	0.4072	.....	0.0133
16.....	.....	2.0190	.....	0.4554	.....	0.0296
18.....	.....	4.6107	.....	1.7692	.....	0.4063
20.....	.....	0.8121	.....	1.2981	.....	0.3932
24.....	.....	1.2677	.....	3.0468	.....	1.6826
30.....	.....	0.4710	.....	2.8781	.....	5.3864
40.....	0.0504	0.0504	.....	0.6647	.....	3.6871
50.....	0.3122	0.0164	0.0112	0.2118	.....	1.9739
60.....	0.9343	.....	0.1603	0.1068	.....	0.8904
80.....	3.0169	.....	1.4114	0.0743	0.0273	0.5186
100.....	2.2728	.....	1.1505	.....	1.1045	0.0460
120.....	0.1862	.....	0.5641	.....	0.5585	0.0056
140.....	0.0534	.....	0.2224	.....	0.2224	.....
Slimes..	0.0942	.....	1.8073	.....	3.9229	.....
Total...	6.9204	14.5011	5.3872	10.9856	5.8356	15.0352

Two tests were made with the pulsion-jig, one upon galena and one upon sphalerite, each paired with quartz.

For convenience, when in use, the sieve was removed. This permitted the products to be drawn off by the bulb in series, exactly as they were in the pointed-tube test, and the bulbs so drawn off were sized, and the different little hills were laid out and photographed as before (Plates XIV. and XV.).

The ratio for three columns of hills, computed by the method adopted for the pointed tube, yielded for the two minerals treated, namely, galena and sphalerite, the figures shown in Table XXXIX.

If now we compare these ratios of Table XXXIX. with those obtained by the pointed tube (Table XXXVII.), we see that nothing whatever has been gained by adding acceleration to interstitial currents. But since acceleration must logically produce some result,

TABLE XXXVII.—*Interstitial Factors, or Multipliers for Obtaining the Diameter of the Particle of Quartz which in the Pointed Tube will be in Equilibrium with the Mineral Specified.*

Quartz and	Ratio of the Diameter of the Heavier to that of the Lighter Mineral.				
	Column 4.	Column 5.	Column 6.	Column 7.	Column 8.
Copper	.....	8.373	7.791	9.629	.....
Galena	.....	6.325	5.656	5.544	.....
Wolframite	.....	4.924	5.381	5.161	.....
Antimony	.....	4.816	4.932	4.942	.....
Cassiterite	.....	4.944	4.713	4.436	.....
Arsenopyrite	.....	3.847	3.747	3.617	.....
Chalcocite	.....	3.464	3.246	2.636	.....
Pyrrhotite	2.640	2.795	2.988	.....	.....
Sphalerite	.....	2.308	2.030	2.042	.....
Epidote	.....	1.702	1.610	1.571	.....
Anthracite	.....	.....	5.540	5.569	5.724

there should be some explanation of its apparent failure here. It is quite possible that this is a case of "parallelism," just as if two cells of a battery be placed side by side in multiple arc nothing is gained in volts, or, as two horses harnessed side by side are no faster than one horse.

Tables XL. and XLI. give the weights of the hills in three columns each of Plates XIV. and XV., from which the computations in Table XXXIX. were made.

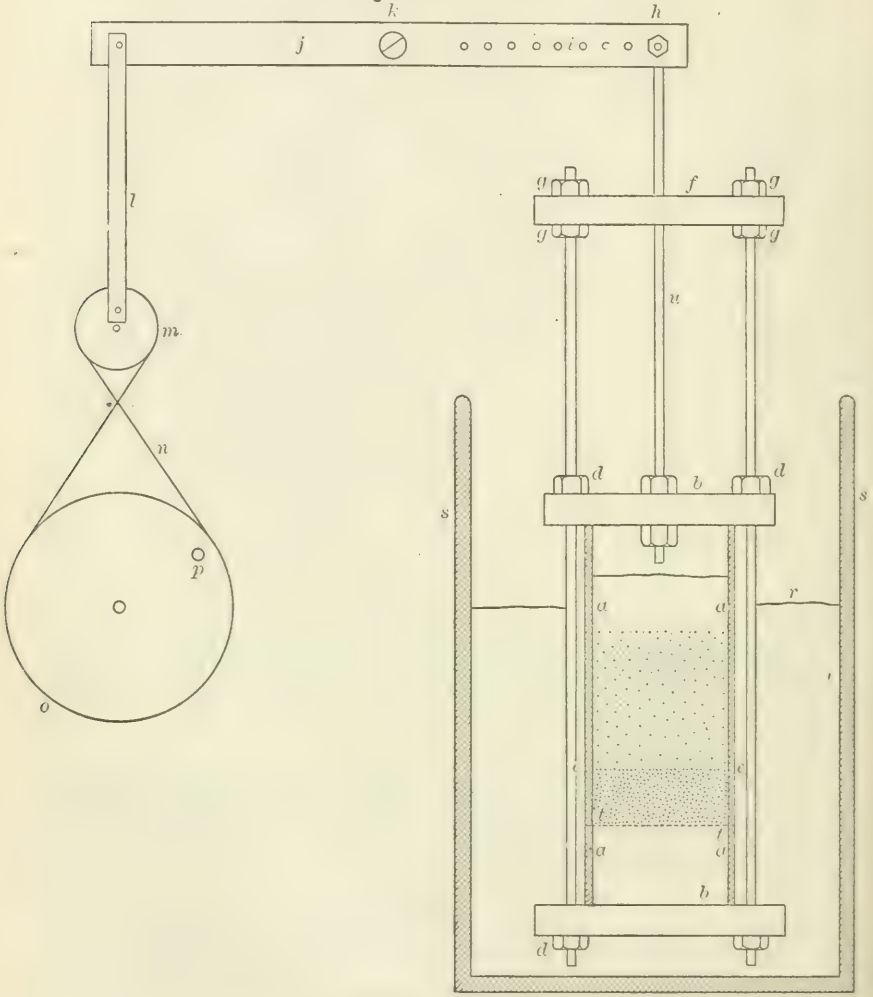
IV.—SUCTION.

This law of jigging has not received the attention which it deserves. Munroe says of it, that suction appears to be necessary for jigging through a bed. Hoppe says that suction is very necessary to jigging, and that he has in progress an investigation of it.

In order to test the limits of the law of suction, I have designed a little movable sieve-jig, shown in Fig. 9, which gives a very perfect jigging-action. It consists of a glass tube, a, a, a, a, 5 inches long and $1\frac{1}{4}$ inches in bore, which is cut at t, t, into two parts, 4 inches

and 1 inch long respectively—the 4 inches being above the sieve; a disk of sieve-cloth, *t*, *t*, is inserted between them; the parts are held together by the wooden bars, *b*,*b*, and the bolts, *e*,*e*, with nuts, *d*,*d*.

Fig. 9.



Movable-sieve jig.

Power is transmitted through the rod *hu*, the beam, *j*, oscillating upon a pivot, *k*, a connecting-rod, *l*, a small pulley, *m*, with crank-pin, a belt, *n*, and a large pulley, *o*, driven by a crank, *p*. The cross-bar, *f*, and the lock-nuts, *g*, *g*, are used simply to stiffen the rod, *u*. The jig is suspended in a glass jar, *s*, with water-level at *r*.

TABLE XXXVIII.—*Galena and Quartz Hills of Plate II. b, Tested with the Free-falling Tube (Fig. 6) for Fall-velocities.*

SIEVE.	Series of Bulbs Drawn from Pointed Tube.									
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
Mesh.	Velocity in Inches per Second (Mean of 4 Determinations).									
On 12...	15.36	14.22	.....	6.62	6.192	6.047	.....	.....	.....	.....
14...	13.24	13.01	.....	6.295	6.192	5.687	4.291	.....	.....	.....
16...	13.24	12.39	.....	5.446	5.774	5.224	4.266	.....	.....	.....
18...	11.46	11.81	9.6	5.333	5.019	4.80	4.412	.....	.....	.....
20...	11.13	10.97	8.257	.....	4.544	4.266	4.062	.....	.....	.....
24...	10.52	10.24	9.72	.....	4.196	4.00	3.8975	3.413	.....	.....
30...	9.48	9.142	7.603	.....	.....	3.821	3.412	.....	.....	.....
40...	6.678	7.110	6.562	5.731	.....	.....	2.898	2.666	.....	.....
50...	.....	6.399	5.774	.....	4.314	.....	2.377	2.151	1.969	.....
60...	.....	.....	4.86	4.86	3.692	3.443	1.882	1.714	1.506	.....
80...	.....	.....	4.314	.....	3.491	3.011	.....	1.326	1.196	.....
100...	.....	.....	.....	3.00	3.00	2.493	2.121	1.046	0.8438	.....
120...	.....	.....	.....	.....	2.603	2.40	2.453	.....	0.5253	.....
140...	.....	.....	.....	.....	.....	1.846	1.740	.....	0.4757	.....
Slimes...	.....	.....	.....	.....	.....	1.691	.....	.....	.....	.....

By turning the crank, p, an oscillating motion up and down is given to l, received by u, and transmitted to the jig-sieve, t, t. The amount of oscillation may be controlled by connecting u with j, by means of any of the holes, i. The smallest oscillation was $\frac{1}{8}$-inch, the largest $\frac{5}{8}$-inch. The latter was preferred for the tests.

The effects of pulsion and suction were studied in three different combinations, namely, full pulsion with much, with little, and with no suction.

1. *Full Pulsion with much Suction.*—When the jig (Fig. 9) is run with the glass tube elevated $1\frac{1}{2}$ inches above the surface of the water at the lowest point of its stroke, the jig operates during the first few pulsations as a lift-pump, elevating the surface of the water within its tube until the inside water-level is, perhaps, one inch above the outside level, the sand-particles acting like so many little valves. Thus, it reaches equilibrium; and, from this time on, the suction due to the downward rush of water must be equal to the pulsion due

TABLE XXXIX.—*Acceleration Factors, or Multipliers for Obtaining the Diameter of the Particle of Quartz, which, in the Pulsion-Jig, will be in Equilibrium with the Mineral Specified.*

Quartz with	Ratio of Average Quartz Diameter to Average Mineral Diameter,		
	Column 5.	Column 6.	Column 7.
Galena	7.161	6.103	5.074
Sphalerite	2.091	2.148	1.972

to the upward rush of water. The bed of this jig is tight and only slightly mobile. The strong suction compacts it more or less. Mobility may be restored by using a long stroke.

TABLE XL.—*Quartz and Galena by Pulsion-Jig* (*Plate* XIV.).

SIEVE.	BULB, No. 5.		BULB, No. 6.		BULB, No. 7.	
Mesh.	Galena, Grammes.	Quartz, Grammes.	Galena, Grammes.	Quartz, Grammes.	Galena, Grammes.	Quartz, Grammes.
12	.....	14.1593	.....	4.7280	.....	0.0512
14	.....	6.9501	.....	5.5411	.....	0.5227
16	.....	2.7127	.....	2.6621	.....	0.4019
18	.....	3.6445	.....	8.7326	.....	3.1527
20	.....	0.5471	.....	2.2216	.....	2.0265
24	.....	0.2946	.....	4.1468	.....	5.3038
30	0.0023	0.0430	.....	3.1682	.....	11.0832
40	0.0378	0.0020	0.0161	0.3059	.....	4.6484
50	0.1887	.....	0.0189	0.0189	0.0520	1.6818
60	1.3844	.....	0.1124	.....	0.0136	0.2582
80	11.4166	.....	2.5483	.....	0.3189	0.0354
100	8.7252	.....	9.9107	.....	5.9146	.....
120	0.3900	.....	1.1889	.....	2.2753	.....
140	0.0487	.....	0.3558	.....	0.9300	.....
Slimes	0.0382	.....	0.5548	.....	3.2828	.....
Total	22.2319	28.3533	14.7059	31.5252	12.7872	29.1658

2. *Full Pulsion with Little Suction.*—When the jig is run with the glass tube inundated to a depth of $\frac{3}{8}$-inch below the surface of the

TABLE XLI.—*Quartz and Sphalerite by Pulsion-Jig* (*Plate* XV.).

SIEVE.	BULB, No. 5.		BULB, No. 6.		BULB, No. 7.	
Mesh.	Sphalerite, Grammes.	Quartz, Grammes.	Sphalerite, Grammes.	Quartz, Grammes.	Sphalerite, Grammes.	Quartz. Grammes.
12.....	.....	5.1771	.....	0.9170	.....	0.0533
14.....	.....	7.3021	.....	2.7287	.....	0.2564
16.....	.....	2.5224	.....	2.0799	.....	0.3592
18.....	0.2887	5.4855	0.0809	8.0078	.....	2.8745
20.....	0.5432	0.8149	0.0246	2.4345	.....	1.9888
24.....	2.7250	0.9083	0.8308	3.3234	0.0594	5.8787
30.....	9.9139	0.2023	4.2891	4.2891	0.4980	9.4620
40.....	3.5496	.....	6.8335	0.3597	3.5382	3.5382
50.....	0.7999	.....	3.5911	.....	5.9406	0.6601
60.....	0.1484	.....	1.3368	.....	4.0482	.....
80.....	.....	.....	0.3341	.....	1.8364	.....
100.....	.....	.....	.....	.....	0.0969	.....
120.....	.....	.....	.....	.....	.....	.....
140.....	.....	.....	.....	.....	.....	.....
Slimes..	.....	.....	.....	.....	.....	.....
Total..	17.9687	22.4126	17.3209	24.1401	16.0177	25.0712

water at the lowest point in its stroke, then, during the downward movement of the sieve, a full pulsion movement is given to the water as it passes up through the sieve, and the sand settles through it. But, on the upward movement, the sand settles in the sieve, and comparatively little suction results, from the inertia of the water. The reason is, that there is a free discharge of the water at the top of the glass tube. Here, we have full pulsion with little suction. The bed of this jig is loose, and very mobile. There is not enough suction to compact it. A shorter stroke here suffices for mobility.

3.—*Full Pulsion with no Suction.*—When the pulsion-jig (Fig. 8) is used upon mixed sands, it matters not whether we revolve the cock rapidly, giving rapid, small pulsations with short intervals of repose, or, more slowly, giving fewer and stronger pulsations with longer periods of repose—the result is the same. The sands are treated by pulsion without suction. The bed of this jig is extremely loose and mobile, there being no suction to compact it.

In all the tests upon jigging now to be described, unless otherwise stated, the stroke of the jig was $\frac{5}{8}$-inch; the layer of quartz was 2 inches thick; the layer of the added mineral to be separated from the quartz, was $\frac{3}{16}$-inch deep, and placed on top of the quartz. When the jig is said to be "elevated," the top of the glass is $1\frac{1}{2}$ inches above the water at the lowest point of the stroke; and when the jig is said to be "inundated," the top of the tube is $\frac{7}{8}$-inch below the surface of the water at the lowest point in the stroke. A 16-mesh sieve was used in the jig throughout the tests.

The first series of tests was made with quartz and galena to note the behavior of different sizes of galena with a single standard size of quartz. For this purpose the two minerals were jigged together as follows:

Quartz, in all cases, through 10- and on 12-mesh; average diameter, 0.0683 inch.

Galena.	Tests 1, 2 and 3.	4, 5 and 6.	7, 8 and 9.	10, 11 and 12.	13, 14 and 15.	16, 17 and 18.
Through (meshes per inch).	10	16	24	30	60	140
On (meshes per inch).	12	18	30	40	80	.....
Average diameter—Inches.....	0.0683	0.0429	0.0262	0.0195	0.0095	0.0042

Each pair was treated with much suction, with little suction, and with no suction.

The results are given in Table XLII.; and the following conclusions as to the behavior of quartz and galena are indicated:

Light suction is more rapid than heavy suction (tests 1 and 2); no suction is more rapid than light suction (tests 2 and 3). Where the galena is fine, much suction is rapid, but no suction is also rapid (tests 10, 11, and 12) until 60- to 80-mesh is reached (test 15), where galena is in equilibrium with the quartz. With heavy suction there is an interesting maximum reached at .0262-diameter galena (test 7). Here the galena is too fine for equal-settling particles to help it much, and it is too coarse to be sucked down in the interstices of the quartz, hence its slow action. Compare it with .0195-diameter galena (test 10), which is small enough to be sucked down in the interstices. Here heavy suction brings quite a rapid separation.

The second series of tests was made upon quartz and sphalerite

TABLE XLII.—*Jig-Tests of Quartz and Galena.*

TEST 1.

Quartz of diameter, .0683.
Galena " "
Jig elevated, much suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
31	199	95	layer on sieve.
40	257	100	" "
60	385	.....	.....

TEST 2.

Quartz of diameter, .0683.
Galena " "
Jig inundated, little suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
25	153	100	layer on sieve.
60	368	.....	.....
Repeated—			
15	95	100	on sieve.
60	381	.....	.....

TEST 3.

Quartz of diameter, .0683.
Galena " "
Pulsion-jig, no suction.

Time, Sec.	Pulsations.	Water.	Per cent.	
45	237	.....	100 down.	
60	317	996 c.c.	too little water, quite sluggish.	
Repeated with more water.				
4	18	.....	100 per cent. down on sieve.	
60	275	1319 c.c.	(very active).	

TEST 4.

Quartz of diameter, .0683.
Galena " .0429.
Jig elevated, much suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
35	235	0	on sieve.
45	302	100	"
60	403	.....	.....

TEST 5.

Quartz of diameter, .0683.
Galena " .0429.
Jig inundated, little suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
50	320	0	on sieve.
55	352	50	"
60	384	100	"

TEST 6.

Quartz of diameter, .0683.
Galena " .0429.
Pulsion-jig, no suction.

Time, Sec.	Pulsations.	Water	Per cent.	
5	25	.....	95	down on sieve.
10	50	.....	100	" "
60	301	1231c.c.	.....	.....

TEST 7.

Quartz of diameter, .0683.
Galena " .0262.
Jig elevated, much suction.

Time, Sec.	Pulsations.	Per cent.	Notes.	
40	222	0	through sieve.	
45	249	80	"	
75	416	92	"	worked very slowly.
135	748	98	"	"

TEST 8.

Quartz of diameter, .0683.
Galena " .0262.
Jig inundated, little suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
15	76	90	through sieve.
30	153	98	" "
60	306	100	" "

TEST 9.

Quartz of diameter, .0683.
Galena " .0262.
Pulsion-jig, no suction.

Time, Sec.	Pulsations.	Water.	Per cent.	
10	58	.....	98	down through sieve.
60	346	999 c.c.		
Repeated—				
10	62	.....	98	down through sieve.
60	371	1235 c.c.		

TABLE XLII.—*Continued.*

TEST 10.

Quartz of diameter, .0683.
Galena " .0195.
Jig elevated, much suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
30	158	0	through sieve.
35	196	80	" "
60	337	99	" "

TEST 11.

Quartz of diameter, .0683.
Galena " .0195.
Jig inundated, little suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
20	105	0	through sieve.
30	157	80	" "
40	210	99	" "
45	236	100	" "
60	315	.....	

TEST 12.

Quartz of diameter, .0683.
Galena " .0195.
Pulsion-jig, no suction.

Time, Sec.	Pulsations.	Water.	Per cent.	
15	92	.....	75	down.
60	368	999 c.c.	95	down.

TEST 13.

Quartz of diameter, .0683.
Galena " .0095.
Jig elevated, much suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
5	31	0	through sieve.
15	95	80	" "
30	190	100	" "
60	380	.....	

TEST 14.

Quartz of diameter, .0683.
Galena " .0095.
Jig inundated, little suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
5	25	0	through sieve.
15	76	80	" "
30	153	100	" "
60	306	...	

TEST 15.

Quartz of diameter, .0683.
Galena " .0095.
Pulsion-jig, no suction.

Time, Sec.	Pulsations.	Water.	Per cent.	
60	368	1231 c.c.	60	through.

A portion of this went through in a body. The rest stayed up and appeared perfectly balanced among the quartz in equilibrium.

TEST 16.

Quartz of diameter, .0683.
Galena " .0042.
Jig elevated.

Time, Sec.	Pulsations.	Notes.
15	86	All through except galena-stained water.
60	346	

TEST 17.

Quartz of diameter, .0683.
Galena " .0042.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
5	30	All the coarser part through the sieve. The finer sizes were floating just above the quartz layer.
60	354	

TEST 18.

Quartz of diameter, .0683.
Galena " .0042.

Pulsion-jig. This was not tried, as the galena would certainly have floated on top of the quartz.

(blende), put together in pairs as in the experiments just described. The diameters were the same as those given above for the similar tests with galena.

The results are given in Table XLIII.; and the following conclusions as to the behavior of quartz and sphalerite are indicated:

When the quartz and sphalerite are of the same size, light suction is far more rapid in its action than heavy suction (tests 19 and 20), and no suction is most rapid of all (test 21). As the discrepancy in size increases, heavy suction gains slightly and light suction loses ground slightly (tests 22 and 23, also 25 and 26). No suction breaks down entirely in test 27; equilibrium is here reached. When, however, .0195-diameter sphalerite is reached, there comes a complete reversal; the heavy suction gives a rapid result (test 28), and the light suction is quite slow (test 29). No suction was not tried because the upward current in the previous experiment was too much for a larger-sized sphalerite. From this down the heavy suction is rapid (tests 31 and 34), while the light suction grows weaker in each number of the series towards the lower end (tests 29, 32, and 35), where the rate of jigging had to be diminished in order to lessen the upward current before the sphalerite would go through the sieve at all (see test 32 for example).

We seem, here, to a more marked degree than with galena, to have the measure of the size of the interstices in the quartz of 10- to 12-mesh or .0683-inch diameter, for the sphalerite through 24 on 30-mesh (= .0262 diameter) is settled only with extreme difficulty according to the laws of equal-settling particles and interstitial currents, while sphalerite of 30- to 40-mesh (= .0195 diameter) is drawn down with great rapidity by suction. We may assume, therefore, that this size is the coarsest that can move freely in the interstices of quartz of 10- to 12-mesh size.

The third series of tests was made upon quartz in large grains, with quartz in small grains, to determine the effects of suction and pulsion. For this purpose, the two kinds were jigged by pairs, as follows (the results are given in Table XLIV.).

Quartz passing through 10-mesh and resting on 12-mesh sieve (average diameter, 0.0683 inch) was jigged with quartz of the following sizes:

Test No.	Through Mesh.	On Mesh.	Diameter in Inches.
37, 38	24	30	0.0262
39, 40	30	40	0.0195
41, 42	60	80	0.0095
43, 44	140	...	0.0042

TABLE XLIII.—*Jig-Tests of Quartz and Sphalerite.*

TEST 19.

Quartz of diameter, .0683.
Sphalerite (blende) " "
Jig elevated, heavy suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	310	0	On sieve, Ave. $\frac{1}{4}$ down.
120	611	0	" Ave. $\frac{4}{16}$ down.
180	908	5	" ave. $\frac{6}{16}$ down.
240	1207	10	" ave. $\frac{7.5}{100}$ down.
300	1513	50	" rest scattering.
360	1819	80	" "
420	2129	96	" "

TEST 20.

Quartz of diameter, .0683.
Sphalerite (blende) " "
Jig inundated, light suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	306	99	On sieve, last few grains hard to separate.

TEST 21.

Quartz of diameter, .0683.
Sphalerite " "
Pulsion-jig, no suction.

Time, Sec.	Pulsations.	Water.	Per cent.	
15	37	.....	95	On sieve.
60	147	770 c.c.	98	On sieve, last end hard to clean.

TEST 22.

Quartz of diameter, .0683.
Sphalerite " .0429.
Jig elevated, heavy suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	270	0	Ave. $\frac{1}{4}$ down.
120	558	2	Through sieve.
180	833	30	" "
240	1117	75	" "
300	1401	90	" "
360	1676	95	" "

TEST 23.

Quartz of diameter, .0683.
Sphalerite " .0429.
Jig inundated, light suction.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	284	50	On or through sieve.
120	563	90	" "
180	838	99	" "

TEST 24.

Quartz of diameter, .0683.
Sphalerite " .0429.
Pulsion-jig, no suction.

Time, Sec.	Pulsations.	Water.	Per cent.	
62	230	.....	90	On or through sieve
Repeated—				
60	202	695 c.c.	95	Much better, but last grains hard to settle.

TABLE XLIII.—*Continued.*

TEST 25.

Quartz of diameter, .0683.
Sphalerite " .0262.
Jig elevated.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	266	25	Sifted.
120	558	50	"
180	850	85	"
240	1138	90	"
300	1422	92	"
360	1759	95	Sifted. Increased speed seemed to help a little here.

TEST 26.

Quartz, of diameter, .0683.
Sphalerite " .0262.
Jig inundated.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	288	70	Sifted.
120	567	95	"
180	846	100	"

TEST 27.

Quartz of diameter, .0683.
Sphalerite " .0262.
Pulsion-jig.

Time, Sec.	Pulsations.	Water.	Per cent.	
60	176	690 c.c.	0	Ave. $\frac{1}{5}$ from top.
120	358	.....	0	Ave. $\frac{1}{4}$ from top and at equilibrium.

Tried slower pulsations on the continuation of the test.

180	406	.....	0	Ave. $\frac{1}{3}$ down, decided gain.

Water cut down to 545 c.c. per min.

240	451	.....	.....	
300	496	.....	50	down, much better but very imperfect operation.

TEST 28.

Quartz of diameter, .0683.
Sphalerite " .0195.
Jig elevated.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	297	95	Sifted.
120	603	100	Sifted, worked very rapidly

TEST 29.

Quartz of diameter, .0683.
Sphalerite " .0195.
Jig inundated.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	319	65	Sifted.
120	611	90	"
180	877	92	"
240	1134	95	"
300	1382	98	"

TEST 30.

The pulsion jig broke down on sphalerite of diameter .0262. It therefore was not tried on these lower sizes.

TABLE XLIII.—*Continued.*

TEST 31.

Quartz of diameter, .0683.
Sphalerite " .0095.
Jig elevated.

Time, Sec.	Pulsations.	Per cent.	Notes.
45	208	99	Sifted very rapidly.

TEST 32.

Quartz of diameter, .0683.
Sphalerite " .0095.
Jig inundated.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	319	75	Sifted.
120	642	80	"
180	930	82	"
240	1307	82	Equilibrium sphalerite collected at top.
300	1529	90	Slow speed caused sifting to start again fairly well.
360	1729	97	

TEST 33.

No test.

TEST 34.

Quartz of diameter, .0683.
Sphalerite " .0042.
Jig elevated.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	288	99	All sifted except a little muddy water. Time probably much too large.

TEST 35.

Quartz of diameter, .0683.
Sphalerite " .0042.
Jig inundated.

Time, Sec.	Pulsations.	Per cent.	Notes.
20	62	0	All floated up on quartz.
Repeated—			
30	84	99	Sifted. Pulsations too slow to pump the quartz, hence sifted.

TEST 36.

No test.

We conclude with regard to quartz of different sizes, that strong suction can draw down small grains of quartz through the interstices between large grains of quartz (tests 39, 41 and 43), and if there is no intercepting heavy mineral layer below, these small particles will go through the sieve. Light suction, with excess of pulsion, cannot draw small quartz down (tests 40 and 42) so long as the jig is really working. When, however, the pulsions are so slow as not to move the coarse quartz (test 42), then suction equals pulsion and sifting takes place. Strong pulsion with no suction cannot draw down fine quartz through the interstices of 10- to 12-mesh quartz under any circumstances. The quartz will always be graded by size; the coarser below, the finer above, as in the pointed tube.

The fourth series of tests comprised the jigging of mixed sizes upon a number of minerals ranging from copper, at the heavy end of the series, to anthracite, at the light end. Each mineral was mixed with quartz in approximately equal quantity by volume, and the sizes in all cases were from 10-mesh to dust. The pairs were composed of quartz, with copper, galena, antimony, arsenopyrite, chalcocite, magnetite, pyrrhotite, sphalerite, epidote, and anthracite, respectively. The results are given in Table XLV. To aid in interpreting these results, the skimmings or tailings from each of the tests were sifted upon the nest of sieves; each size was spread out upon a sheet of paper, and the quantity of the heavy mineral in it was estimated by the eye, as per cent. by volume or number of grains in one hundred grains. The results of this sifting and valuing process are given in Tables XLVI., XLVII. and XLVIII.

As a means of comparing quickly the final results of these jigging-tests, the phrase "5 per cent. in the tails reached up to . . . . mesh" is used. The figures inserted in the blank are taken from Tables XLVI., XLVII., and XLVIII., and will be seen to give a quick and fairly good summing-up of the tests.

The conclusions as to the jigging of mixed sizes are as follows:

In the elevated (heavy suction) tests, the tailings retrograde rapidly from copper towards arsenopyrite. From arsenopyrite on, they retrograde but little. (See Table XLVI.)

Again, in the inundated (light-suction) tests, beginning with copper, the quality of the tailings of each mineral is much poorer than that of the mineral next preceding it, until arsenopyrite is reached. Here again, there is a change—the tailings of arsenopyrite are much worse than those of antimony, and but little better than those of its lighter neighbors to the right.

TABLE XLIV.—*Jig-Tests of Coarser and Finer Quartz.*

TEST 37.

Quartz of diameter, .0683.
Quartz " .0262.
Jig elevated.

Time, Sec.	Pulsations.	Per cent.	Notes.
60	265	0	Fine quartz all on top.
120	628	0	" "
180	1177	0	A little down ¼ in.
240	1952	0	" " 1 in.

The great variation in speed effected little.

TEST 38.

Quartz of diameter, .0683.
Quartz " .0262.
Jig inundated.
Not tried.

TEST 39.

Quartz of diameter, .0683.
Quartz " .0195.
Jig elevated, ⅝-in. stroke.

Time, Sec.	Pulsations.	Notes.
60	292	No sifting, all fine quartz on top.
120	744	Rapid jigging, top layer disappeared, sifting began.
180	988	Slow jigging sent the fine grains back up to the top.

TEST 40.

Quartz of diameter, .0683.
Quartz " .0195.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
60	496	Fast, sent all fine quartz to top.
120	775	Slower, " "
180	997	" " "
240	1179	" " "

TEST 41.

Quartz of diameter, .0683.
Quartz " .0095.
Jig elevated.

Time, Sec.	Pulsations.	Notes.
5	22	Began sifting.
60	266	Sifting freely.
120	536	All sifted.

TEST 42.

Quartz of diameter, .0683.
Quartz " .0095.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
60	399	Fast jigging, all fine quartz on top.
120	696	Slower " "
180	935	" " "
240	1143	Slower, 15 per cent. sifted, no pumping motion.
300	1289	Slower, 30 per cent. sifted, no pumping motion, fine quartz on top.

TEST 43.

Quartz of diameter, .0683.
Quartz " .0042.
Jig elevated.

Time, Sec.	Pulsations.	Notes.
18	88	Fine quartz all sifted except a little muddy water.

TEST 44.

Quartz of diameter, .0683.
Quartz " .0042.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
19	49	Coarsest of fine quartz sifted, finest lifted.

TABLE XLV.—*Jig-Tests of Mixed Sizes.*

TEST 45.

Quartz and Copper through 10-mesh.
Jig elevated, $\frac{5}{8}$-inch stroke.

Time, Sec.	Pulsations.	Notes.
60	323	Some 30-mesh copper still in quartz.
120	633	Practically clean.
180	948	No change.

Five per cent. in the tails reached up to 120-mesh sieve.

TEST 46.

Quartz and Copper through 10-mesh.
Jig inundated, $\frac{5}{8}$-inch stroke.

Time, Sec.	Pulsations.	Notes.
60	319	20-mesh copper in quartz.
120	647	40-mesh " "
195	1055	A little copper still in quartz.
255	1370	No better than last.

Five per cent. in the tails reached up to 80-mesh.

TEST 47.

Quartz and Copper through 10-mesh.
Pointed tube, *i.e.*, pulsion-jig.

Five per cent. in the tails reached up to 30-mesh.

TEST 48.

Quartz and Galena through 10-mesh.
Jig elevated.

Time, Sec.	Pulsations.	Notes.
60	354	Some 40-mesh still showing.
120	669	Clean to the eye.
180	979	No change.

Five per cent. in the tails reached only to fines.

TEST 49.

Quartz and Galena through 10-mesh.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
60	310	Some 40-mesh galena still showing in quartz, a dark layer of fines on top.
120	629	Some 50-mesh galena still showing in quartz, a dark layer of fines on top.
180	935	Some 60-mesh galena still showing in quartz, a dark layer of fines on top.
240	1241	Some 80-mesh galena still showing in quartz, a dark layer of fines on top.
300	1560	Some 80-mesh still showing in quartz, a dark layer of fines on top.
360	1883	Some 80-mesh still showing in quartz, a dark layer of fines on top.

Five per cent. in the tails reached up to 60-mesh.

TEST 50.

Quartz and Galena through 10-mesh.
Pointed tube, *i.e.*, pulsion-jig.

Five per cent. in the tails reached up to 50-mesh.

TABLE XLV.—*Continued.*

TEST 51.

Quartz and Antimony through 10-mesh.
Jig elevated.

Time, Sec.	Pulsations.	Notes.
60	319	80-mesh antimony showing in quartz.
120	656	Quartz practically clean.
180	984	No change.

Five per cent. in the tails reached only to fines.

TEST 52.

Quartz and Antimony through 10-mesh.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
60	315	30 mesh antimony in the quartz, a dark layer of fines on top.
120	652	40-mesh antimony in the quartz, a dark layer of fines on top.
180	962	50-mesh antimony in the quartz, a dark layer of fines on top.
240	1272	50-mesh, no better than last.
300	1573	50-mesh " "

Five per cent. in the tails reached up to 50-mesh.

TEST 53.

Quartz and Antimony through 10-mesh.
Pointed tube, *i.e.*, pulsion-jig.

Five per cent. in the tails reached up to 30 mesh.

TEST 54.

Quartz and Arsenopyrite through 10-mesh.
Jig elevated.

Time, Sec.	Pulsations.	Notes.
60	350	A little 60 mesh arsenopyrite showing in quartz.
120	682	Clean to the eye.
180	905	" "

Five per cent. in the tails reached 100-mesh.

TEST 55.

Quartz and Arsenopyrite through 10-mesh.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
60	315	30-mesh arsenopyrite showing in quartz, a dark layer of fines on top.
120	638	50-mesh arsenopyrite showing in quartz, a dark layer of fines on top.
180	975	60-mesh arsenopyrite showing in quartz, a dark layer of fines on top.
240	1316	60-mesh no better, showing a dark layer of fines on top.
360	1666	60-mesh " "

Five per cent. in the tails reached up to 40-mesh.

TEST 56.

Quartz and Arsenopyrite through 10-mesh.
Pointed tube, *i.e.*, pulsion-jig.

Five per cent. in the tails reached up to 14-mesh.

TABLE XLV.—*Continued.*

TEST 57.

Quartz and Chalcocite through 10-mesh.
Jig elevated.

Time, Sec.	Pulsations.	Notes.
60	275	50-mesh chalcocite showing in quartz.
120	594	60-mesh " " "
180	926	80-mesh " " "
240	1263	Very little improvement.
300	1586	No better.

Five per cent. in the tails reached up to 60-mesh.

TEST 58.

Quartz and Chalcocite through 10-mesh.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
60	323	40-mesh chalcocite in the quartz, a dark layer of fines on top.
120	664	50-mesh chalcocite in the quartz, a dark layer of fines on top.
180	992	60-mesh chalcocite in the quartz, a dark layer of fines on top.
240	1311	Little or no better, a dark layer of fines on top.
300	1626	" " " "
360	1958	No better.

Five per cent. in the tails reached up to 40-mesh.

TEST 59.

Quartz and Chalcocite through 10-mesh.
Pointed tube, *i.e.*, pulsion-jig.

Five per cent. in the tails reached up to 12-mesh.

TEST 60.

Quartz and Magnetite through 10-mesh.
Jig elevated.

Time, Sec.	Pulsations.	Notes.
60	319	20-mesh magnetite in quartz.
120	620	Less 20-mesh " "
180	961	Less 20-mesh " "
240	1302	Less 20-mesh " "
300	1639	No better.

Five per cent. in the tails reached only fines.

TEST 61.

Quartz and Magnetite through 10-mesh.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
60	346	18-mesh magnetite in the quartz, a slight dark layer of fines on top.
120	674	20-mesh magnetite in the quartz, a slight dark layer of fines on top.
180	1011	20-mesh magnetite in the quartz, a slight dark layer of fines on top.
240	1321	24-mesh magnetite in the quartz, a slight dark layer of fines on top.
300	1640	No better.

Five per cent. in the tails reached 24-mesh.

TEST 62.

Quartz and Magnetite through 10-mesh.
Pointed tube, *i.e.*, pulsion-jig.

Five per cent. in the tails reached 14-mesh.

TABLE XLV.—*Continued.*

TEST 63.

Quartz and Pyrrhotite through 10-mesh.
Jig elevated.

Time. Sec.	Pulsations.	Notes.
60	337	30-mesh pyrrhotite in the quartz.
120	683	30-mesh " " " better than last.
180	998	" " " "
240	1317	" " " "
300	1632	No better.

Five per cent. in the tails reached up to 60-mesh.

TEST 64.

Quartz and Pyrrhotite through 10-mesh.
Jig inundated.

Time. Sec.	Pulsations.	Notes.
60	293	30-mesh pyrrhotite in the quartz, a dark layer of fines at top.
120	616	30-mesh better.
180	842	" "
240	1112	" "
300	1458	A little better, dark layer almost gone.
360	1773	No better.

Five per cent. in the tails reached up to 30-mesh.

TEST 65.

Quartz and Pyrrhotite through 10-mesh.
Pointed tube, *i.e.*, pulsion-jig.

Five per cent. in the tails reached up to 12-mesh.

TEST 66.

Quartz and Sphalerite through 10-mesh.
Jig elevated.

Time, Sec.	Pulsations.	Notes.
60	328	20-mesh sphalerite in the quartz.
135	722	30-mesh " "
195	1041	40-mesh " "
270	1440	50-mesh " "
330	1755	A few scales still sifting.
390	2061	" " "
450	2384	Practically clean.

Five per cent. in the tails reached up to 120-mesh.

TEST 67.

Quartz and Sphalerite through 10-mesh.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
60	310	18-mesh sphalerite in quartz, thick dark layer of fines at top.
120	625	20-mesh sphalerite in quartz.
180	904	20-mesh gaining a little.
240	1210	30-mesh " "
300	1533	" " "
360	1870	" " "
420	2202	" " "
540	2849	30-mesh sifting ceased, thin dark layer of fines at top.

Five per cent. in the tails reached up to 30-mesh.

TEST 68.

Quartz and Sphalerite through 10-mesh.
Pointed tube, *i.e.*, pulsion-jig.

Five per cent. in the tails reached up to 12-mesh.

TABLE XLV.—*Continued.*

TEST 69.

Quartz and Epidote through 10-mesh.
Jig elevated, much suction.

Time, Sec.	Pulsations.	Notes.
60	337	Sifting rapidly, but some 14-mesh epidote in quartz.
120	660	Bed packed.
180	997	Still sifting a little.
240	1329	" "
300	1639	No better.
		Much quartz sifted.

Five per cent. in the tails reached up to 16-mesh.

TEST 70.

Quartz and Epidote through 10-mesh.
Jig inundated, little suction.

Time, Sec.	Pulsations.	Notes.
60	283	Much 16-mesh in quartz, a layer of fine epidote on top.
120	595	" " "
180	890	No better.
240	1200	"
300	1488	"
		A little quartz sifted.

Five per cent. in the tails reached up to 16-mesh.

TEST 71.

Quartz and Epidote through 10-mesh.
Pulsion-jig, no suction.

Five per cent. in the tails reached up to 12-mesh.

TEST 72.

Quartz and Anthracite through 10-mesh.
Jig elevated.

Time, Sec.	Pulsations.	Notes.
60	392	Some 16-mesh quartz in the anthracite.
120	602	Some 20-mesh.
195	970	Practically clean.

Five per cent. of quartz in the anthracite reached up to 30-mesh.

TEST 73.

Quartz and Anthracite through 10-mesh.
Jig inundated.

Time, Sec.	Pulsations.	Notes.
60	275	Some 16-mesh quartz in the anthracite.
120	576	A few 16-mesh left.
195	913	Some 40-mesh.
255	1196	A little better.
315	1484	" "
375	1781	Some 60-mesh.
435	2118	Practically clean to the eye.

Five per cent. of quartz in the anthracite reached up to 60-mesh.

TEST 74.

Quartz and Anthracite through 10-mesh.
Pulsion-jig.

Five per cent. of quartz in the anthracite reached up to 24-mesh.

TABLE XLVI.—*Percentage of Heavy Mineral in Tails, in Tests with Jig Elevated. Much Suction.*

Sieve-mesh.	Copper.	Galena.	Antimony.	Arsenopyrite.	Chalcocite.	Magnetite.	Pyrrhotite.	Sphalerite.	Epidote.	Quartz in Anthracite.
On 12	0.	0.	0.	0.	0.	0.	0.	0.	2.	0.1
" 14	0.	0.	0.	0.	0.	2.	0.	0.	3.	0.5
" 16	0.	0.	0.	0.5	0.	1.5	2.	0.	8.	0.7
" 18	0.	0.	0.	0.	0.	3.	2.	2.	20.	1.5
" 20	0.	0.	0.	0.	0.	4.	1.	1.5	25.	1.5
" 21	0.	0.	0.	1.	0.	3.	2.	2.	25.	2.0
" 30	0.	0.	1.	2.	1.	3.	4.	3.	25.	5.0
" 40	0.1	0.1	1.	2.	2.	3.	4.	3.	30.	5.
" 50	0.1	0.1	1.	2.	3.	3.	4.	3.	45.	8.
" 60	0.5	0.1	0.5	3.	5.	2.	5.	2.	45.	10.
" 80	0.5	0.1	0.5	3.	5.	3.	6.	3.	40.	15.
" 100	3.	1.	1.	5.	6.	2.	8.	4.	30.	20.
" 120	5.	1.	2.	5.	5.	3.	8.	5.	25.	35.
" 140	7.	1.	2.	3.	5.	3.	8.	6.	20.	35.
Slimes	5.	7.	10.	8.	10.	5.	20.	10.	20.	50.

For testing the pulsion-jig, the corresponding values were obtained by assuming that the pulsion-jig and the pointed tube have practically the same effect. This has been proved at two ends of the series (compare Plate XIV. with Plate II. *b*, and Plate XV. with Plate X.; also, the tables of figures which correspond).

On this assumption, we take, for example, Plate II.*b*. Clearly, bulbs Nos. 1, 2, and 3, form the heads of jigging, and bulbs Nos. 4, 5, 6, 7, 8, 9, and 10, form the tails. We estimate the percentage of galena in these different sizes of the tailings. There is no galena in 12-, 14-, 16-, 18-, 20-, and 24-mesh grains. From 30-mesh down, my estimate is given in Table XLVIII., together with estimates made in like manner on Plates I. to XII., inclusive.

Once more arsenopyrite appears as the turning-point in the series; the tailings of the successive minerals rapidly retrograding in the

TABLE XLVII.—*Percentage of Heavy Mineral in Tails, in Tests with Jig Inundated. Little Suction.*

Sieve-mesh.	Copper.	Galena.	Antimony.	Arsenopyrite.	Chalcocite.	Magnetite.	Pyrrhotite.	Sphalerite.	Epidote.	Quartz in Anthracite.
On 12..	0.	0.	0.	0.	0.	0.	0.	0.	1.	0.
" 14..	0.	0.	0.	0.	0.	2.	0.	1.	3.	0.
" 16..	0.	0.	0.	0.	0.	0.	0.5	1.	8.	0.
" 18..	0.	0.	0.	1.	1.	3.	1.	2.	8.	0.3
" 20..	0.	0.	0.	1.	2.	4.	2.	2.	20.	0.
" 24..	0.	0.	0.1	1.	3.	5.	4.	4.	25.	0.5
" 30..	2.	0.5	0.5	3.	4.	7.	10.	6.	50.	0.5
" 40..	3.	1.5	4.	6.	10.	10.	15.	10.	55.	1.
" 50..	3.	3.	5.	8.	15.	15.	20.	15.	70.	2.
" 60..	2.	5.	10.	15.	20.	20.	25.	20.	75.	5.
" 80..	5.	7.	20.	25.	25.	20.	35.	30.	80.	50.
" 100..	5.	10.	30.	40.	40.	25.	50.	50.	75.	95.
" 120..	7.	10.	40.	60.	60.	40.	75.	70.	80.	90.
" 140..	7.	10.	40.	75.	75.	75.	95.	80.	80.	80.
Slimes...	60.	80.	85.	90.	95.	90.	99.	90.	90.	90.

series until arsenopyrite is reached, while the tailings of the minerals to the right of arsenopyrite are but little worse than those of that mineral.

Quartz being the heavier mineral, of quartz and anthracite, it is much too good to be in the company it is. The ratio between quartz and anthracite is nearly as great as between quartz and antimony.

The results of this series prove conclusively, that strong suction is more efficient for jigging mixed sizes than weak suction (compare Nos. 45 and 46, 48 and 49, etc.); and again, that weak suction is more efficient than no suction (compare Nos. 46 and 47, 49 and 50, etc.)

These tests show, also, that in jigging mixed sizes of a series of minerals, we have found, in arsenopyrite, the middle cone of the three (see Fig. 7), the turning-point in the set, where the heavier

TABLE XLVIII.—*Percentage of Heavy Mineral in Tails, in Tests from Pointed Tube or Pulsion-Jig. No Suction.*

Sieve-mesh.	Copper.	Galena.	Antimony.	Arsenopyrite.	Chalcocite.	Magnetite.	Pyrrhotite.	Sphalerite.	Epidote.	Quartz in Anthracite.
On 12.....	0.	0.	0.	2.	5.	.5	10.	25.	10.	0.
" 14.....	0.	0.	0.	5.	15.	5.	25.	25.	25.	0.
" 16.....	0.	0.	0.	10.	20.	15.	30.	30.	30.	1.
" 18.....	0.	0.	0.	15.	30.	30.	35.	40.	50.	2.
" 20.....	0.	0.	0.	15.	40.	40.	40.	60.	50.	2.
" 24.....	0.5	0.	0.5	30.	40.	40.	40.	60.	50.	5.
" 30.....	5.	0.5	5.	40.	50.	50.	50.	60.	50.	10.
" 40.....	15.	1.	30.	60.	60.	50.	50.	60.	60.	20.
" 50.....	20.	15.	50.	60.	70.	50.	60.	60.	60.	20.
" 60.....	30.	30.	60.	75.	70.	40.	60.	60.	60.	50.
" 80.....	60.	60.	70.	75.	70.	40.	60.	60.	60.	60.
" 100.....	65.	65.	70.	75.	70.	40.	60.	60.	60.	60.
" 120.....	70.	70.	70.	75.	70.	30.	75.	60.	60.	60.
" 140.....	70.	70.	75.	75.	70.	30.	75.	60.	60.	60.
Slimes.....	70.	70.	75.	75.	75.	30.	75.	70.	70.	40.

mineral grain is just large enough to fill the interstices in the quartz. All the minerals heavier than arsenopyrite jig easily; all those that are lighter jig poorly when mixed sizes are used.

The turning-point referred to in Fig. 7, where the heavy grain is of a size which just fills the interstices among the lighter grains, is represented by the interstitial factor 3.7 of Table XXXVII. This factor has been arrived at in three places in this investigation, namely, by jigging-tests, Nos. 7 and 10; jigging-tests, Nos. 25 and 28, and the facts brought out in Tables XLVI., XLVII., and XLVIII. The factor will probably vary somewhat with the fracture of the minerals; and it also needs confirmation for larger sizes.

While it is a simple matter to make a table of *equal-settling factors* (Table VII.), of *interstitial factors* (Table XXXVII.) and of *acceleration-factors* (Table XXXIX.), no corresponding table of *suction-*

factors can be made. The most that can be said is that suction increases with the length of the plunger-stroke, with the difference in specific gravity of the two minerals, and with the diminishing of the thickness of the bed on the sieve, whether of the heavier minerals only or of both minerals.

Summary.—The two chief reactions of jigging are pulsion and suction.

The effect of pulsion depends upon the laws of equal-settling particles, interstitial currents, and, possibly, also of acceleration. The chief function of pulsion is to save the larger grains of the heavier mineral, or the grains which settle faster and farther than the waste.

The effect of suction depends upon the interstitial factor of the minerals to be separated (see Table XXXVII. and Fig. 7). If this factor is greater then 3.70, suction will be efficient and rapid. If the factor is less than 3.70, suction will be much hampered and hindered. The use of a long stroke will help to overcome this difficulty. The chief function of suction is to save the particles that are too small to be saved by the laws of equal-settling particles, and of interstitial currents, acting through the pulsion of the jig.

For jigging mixed sizes, pulsion with full suction should be used.

For jigging closely-sized products, pulsion with a minimum of suction should be used.

The degree of sizing needed as preparation for jigging, if we are looking for the most perfect work, depends solely upon the interstitial factor of the minerals to be separated. If the factor is above 3.70 (assuming this value to be sufficiently proved), then sizing is simply a matter of convenience. The fine slimes should, of course, be removed; and, if it is more convenient to send egg-size, nut-size, pea-size, and sand-size, each to its own jig, the suitable screens should be provided for this purpose, and a hydraulic separator for grading the finest sizes. But if, on the other hand, the factor is below 3.70, then, the jigging of mixed sizes cannot give perfectly clean work, and the separation will be approximate only. To effect the most perfect separation, close sizing must be adopted, and the closer the sizes are to each other the more rapid and perfect will the jigging be. There may be conditions where the jigging of mixed sizes of this class will be considered sufficiently satisfactory, as an expedient, under the circumstances. Indeed, it is probable, that as much as 90 per cent. of the mineral was saved in every test recorded in Table XLVI., except that of epidote.

The small scale on which this work has been done may have ex-

aggerated, to some extent, some of the jigging-results. It is hoped, however, that if the reader does not find here the large-scale work exactly pictured, he will find analogies from which he may be able to predict results.

The author is indebted to his assistant, Mr. W. A. Tucker, for the careful and accurate way in which the whole investigation has been conducted; and particularly to Mr. J. B. Seager, graduate of the Michigan Mining School at Houghton, for the help he has given, not only in conducting the experiments, but also in the way of suggestion and intelligent criticism.

ORE-DRESSING AND CONCENTRATION IN SWEDEN.

BY P. G. LIDNER, NEW YORK CITY.

(Bridgeport Meeting, October, 1894.)

Introductory.

The mechanical concentration of ores has not attained any considerable extent in Sweden, by reason of a scarcity of ores calling for this kind of treatment. Of rich iron-ores there is still an abundant supply; and magnetic concentration, which in the future undoubtedly will become of great importance, not being as yet demanded by necessity, has not advanced beyond a few experiments. At the copper-mines mechanical concentration has been superseded by chemical processes; the lead mines are worked out; and it is (with one exception) only the mines producing blende, or blende and galena intimately mixed, that have adopted mechanical methods for enriching their low-grade ores. While the concentrating-plants in Sweden are few and their output is comparatively insignificant, the methods employed, the general features of the work and the results obtained under extremely unfavorable conditions, may offer some points of interest to persons who are engaged in this branch of industry and have similar problems to deal with.

Before going into a detailed description of the several plants, I will offer a few remarks that hold good for all of them and constitute a general review of Swedish concentration and its conditions. At all the works the ore to be treated is a mixture of blende and

galena* with either a fine-grained gray gneiss (granulite) or limestone as gangue; and the object is to separate the two former minerals in the form of concentrates as clean and rich as existing working methods and commercial economy will permit, since they must be subsequently shipped long distances, and subjected to heavy transportation-charges. The ores are exceedingly difficult to concentrate for the reason that the minerals are so finely disseminated in the gangue that separate particles of galena and blende cannot be distinguished by the naked eye, and no sufficiently high concentration can be accomplished until the material has been ground down to at most 2.5 mm. diameters of grain. In most cases, 1 mm. is the largest permissible size. As a consequence, the Swedish mills are principally made up of slime-concentrating apparatus, and machines for coarse concentration are either not at all needed or occupy a very subordinate position. The mills are of the German pattern, and Continental methods and principles are prevalent. Before the ore is sent to the mill it is thoroughly overhauled and hand-culled, pieces of pure mineral or dead rock and foreign substances being thrown out and only the stuff too badly mixed or too fine for hand-sorting subjected to further treatment. This preparatory work deserves attention because it effects a greater saving than is generally supposed. The cheapest labor is employed for the sorting; on the picked, clean ore there is no loss in concentration; there is no wear and tear of machinery by anything thus removed, be it clean mineral or gangue; and both the machinery and the mill-work are highly benefited by keeping out broken tools, pieces of iron or wood, rags, etc. While I would not recommend in American practice the carrying of hand-work as far as in Europe, because wages are higher on this side of the Atlantic, I venture to say that this preliminary operation is generally too much neglected in the United States and that many concentrating works, especially in the zinc-mining districts, would profit by paying more attention to it.

At what time mechanical concentration was first introduced in Sweden it is hard to tell with certainty, but there seems to be no doubt that the earliest efforts to separate metalliferous minerals from accompanying gangue by means of machinery were made at Sala, and from the limited capacity of these ancient works and their very large accumulations of tailings, I would infer that the old plants were in operation about two centuries. There must have been two

* To this statement, Sala, an old silver-lead mine, at the present time pretty well exhausted, constitutes an exception, which will be explained later on.

of them, and both have gone out of existence, being replaced by modern methods and appliances. These old mills were *Pochwerke*, copied from German practice and operated in a very simple manner. The cobbed ore was crushed fine by old-fashioned stamps, with wooden stems, etc., and the pulp run into a pit for settling. From these the pulverized material was dug up by hand and fed on *Stossherde*, inclined tables with longitudinal motion (end-bumps). However imperfect this method may appear at first sight, the results obtained by it have never been equalled by the new, modern mill, so far as close saving of the minerals is concerned. With regard to capacity, the latter is, of course, superior.

At Falun, the principal copper-mine in Sweden, a large concentrating-mill was erected about twenty-five years ago, but the losses being too high (25 per cent., and often more), the machinery was taken out and the building turned into a lixiviation-plant. By leaching, fully 95 per cent. of the copper is extracted, which proves, at least in this case, the superiority of chemical processes over mechanical concentration. Hand-sorting is the only means employed at Falun for enriching the stuff brought from the mine. It works well. Of 75,000 tons of material annually hoisted, 15,000 tons of ore suitable for extraction (3 per cent. of copper) are picked out, and the remaining 60,000 tons, being either dead rock or containing only a slight trace of copper, are dumped back into the mine, and used as filling. In the same way the ore is dressed at the other copper-mines in the country and also at some small lead- and zinc-mines which cannot afford concentrating-machinery.

The Works at Åmmeberg and Johannesborg.

The most prominent concentrating-works in Sweden belong to the French *Vieille Montagne* Co., and are run in connection with this company's extensive zinc-mines at Åmmeberg, in the province of Nerike. There are two plants—one at Åmmeberg and another smaller one at Johannesborg, about three miles distant. The main plant, at Åmmeberg, was built in 1859–61. Although it has from time to time been enlarged and improved in conformity with more modern principles, it is still rather antiquated and old-fashioned, but in most respects it is doing excellent work. Blende and galena are separated from each other remarkably well, and the loss in tailings has been reduced to a lower figure than at most other mills where zinc-ore is concentrated, but the arrangement of the plant is somewhat disadvantageous. All the machines are on one floor, which

causes much rehandling of the material, and the addition of new parts at different times has given rise to a number of disconnected and independent departments. This is claimed to be an advantage, inasmuch as a temporary shut-down of one department does not affect the whole mill; but it requires an excessive amount of labor, as is proved by the fact that not less than seventy men are engaged in and about the mill. The general course of the work is hard to follow, and even one experienced in concentration will at first be puzzled by the excessive rehandling and the great number of laborers running around with wheelbarrows in every direction.

In order to get a clear view of the concentration we will first make a visit to the mines, situated about 8 miles from Åmmeberg, and connected with this place by a railroad, built and operated by the company. The extensive bedded deposit of blende is opened up by two main shafts; and the material, brought up from the mines, after some preliminary overhauling (by which mostly large chunks of dead rock are thrown out), is run through crushers of the ordinary pattern, and divided by sizing-drums into two classes, one coarser and one finer than 20 mm. The fine stuff goes directly to the mill; the coarse material, after passing out of the drums, is automatically distributed on slowly revolving picking tables, provided with three circular shelves, one above the other. The shelves have on the inside a flange, behind which there are, at the top shelf, two slots extending along the whole circumference, and each of these slots is connected with one of the shelves underneath. The stuff is deposited on the upper shelf, and boys standing at the table pick out pieces of clean blende, which are thrown over the flange into the first slot, and fall down on the second shelf. Pieces of rock are also picked out and put in the second slot leading to the third shelf. By fixed iron plows the stuff is raked off, the rock and clean ore being collected in separate cars underneath, but the remaining mixed ore at the top goes to a second table, which has only two shelves. Here it is picked over again and sorted into two classes: wash-ore, containing, beside blende, a considerable quantity of galena, which is put on the lower shelf, and stuff with little or no galena, which is left on the top shelf. The former is sent to the mill at Johannesborg, and the latter, after calcining, to the main works at Åmmeberg. By this systematic hand-sorting, 7000 tons of clean blende and much more rock are annually picked out at a comparatively trifling expense; and if we take into consideration that the ore at Åmmeberg, being mostly a fine-grained gneiss, with blende and a little galena sprinkled

through it, is not very well suited for culling, this result is evidently a point in favor of my previously expressed opinion that concentration should be preceded by hand-culling and picking to such an extent as circumstances permit.

The coarse wash-ore, without galena, is calcined before being sent to the mill. The burning, which is performed in kilns with alternate layers of ore and coal, serves two purposes. One is to soften the unusually hard gangue, thereby reducing the wear and tear on the grinding-machinery. With this object in view, water was at first put on the hot stuff when drawn from the kilns; but it was found that the sudden cooling gave rise to innumerable small fissures, which afterwards caused the forming of an excessive amount of slime and increased losses in concentration, and the burnt ore is now allowed to cool slowly. The other reason for burning the ore is that it effects a separation of iron pyrites, which is a very objectionable admixture in blende, and cannot be satisfactorily removed in the ordinary course of concentration. If a mixture of iron pyrites and blende is slightly roasted, the blende remains unchanged, but the pyrites crumbles, becomes spongy and porous, and is easily washed off by the subsequent water treatment. This method of separating iron pyrites and blende has been known and practiced by the *Vieille Montagne* Company more than thirty years, and was introduced by the late Ferdinand de Stwolinsky, a former employee of that company, in the Matthiessen and Hegeler works at La Salle, Ill., many years ago, when they operated a concentrating-plant in connection with their zinc-works. I mention this because Prof. W. P. Blake, in a recent paper (*Trans.*, xxii., 569), says that the method has been adopted in Wisconsin, and describes it as a new metallurgical process.

Of the products sent to the mill at Åmmeberg, the clean, picked blende is reduced by crusher and rolls to a suitable size and delivered to the roasting furnaces. The wash-ore, finer than 20 mm. (I.), and the coarse burnt stuff (II.) are kept separate, and concentrated in the following way:

I. *Fine Material.*—This is fed by hand into a sizing drum with two compartments. While feeding, the workman picks out foreign substances, such as pieces of wood, rags, etc., and also, as far as time will permit, coarser pieces of clean blende and rock. The holes in the first part of the drum have a diameter of 2½ mm., and in the second part, 5 mm. Stuff coarser than 5 mm. drops into another drum, with three compartments and holes of 8, 13, and 20-mm. di-

ameter. From these two drums six grades or sizes are obtained and further treated, as follows:

a. Stuff Coarser than 20 *mm.*—Theoretically, there should be no such grade as this, because the ore has previously gone through a drum with 20-mm. holes, but more or less of it is always obtained, for several reasons. The holes in the first drums (at the mines) are often worn to a larger size than the original 20 mm., and moreover, in handling the stuff, some coarse pieces are liable to go astray and get mixed up with the fine ores. This grade is hand-sorted, rock and clean blende are picked out, and the middlings go to a pair of rolls (R_1).

b. Size Between 13 *and* 20 *mm.*, called at the mill 20-mm. stuff. This is treated in one-sieve jigs, run intermittently. A quantity of the material is shoveled in on the sieve, the jigs are run for a while, and then stopped, whereupon the different layers into which the stuff has been arranged, according to specific gravity, are taken out with a piece of sheet-iron. The top layer consists of pieces of rock, which are rejected; then come middlings of rock and blende, which are sent to the rolls, R_1; then, clean blende; and at the bottom, blende with more or less galena, which is also brought over to the rolls, R_1, for finer crushing. Sometimes, no galena is present; then only three products are obtained in these jigs.

c. Size Between 8 *and* 13 *mm.*, called 13-mm. stuff.—The different grades are named after the upper limit. This receives the same treatment as grade *b*.

d. Size Between 5 *and* 8 *mm.*—Wash-ore of this class goes from the sizing-drum directly to a three-sieve jig, which produces in the first compartment clean blende; in the second, also clean blende; and in the third, middlings, which are crushed finer by the rolls, R_1. The tailings from this jig are rejected. If the ore contains galena, middlings of galena and blende are received from the first compartment, and are sent to the rolls.

e. Size Between $2\frac{1}{2}$ *and* 5 *mm.* is treated as grade *d* in another jig.

f. Stuff Finer than $2\frac{1}{2}$ *mm.*—This is conducted to a hydraulic classifier (H_1), where it is divided into two sizes, and each class is treated in a three-sieved jig (J_1 and J_2). The first compartment gives a mixture of galena and blende; and from the second and third sieves clean blende is obtained. The tailings are brought to small grinders (Schwartzmann or Heberle mills), of various construction, for comminution, and are afterwards united with similar material in Part II. The mixed galena and blende from the first

compartment in jigs J_1 and J_2 is separated in another jig, J_3, where the first sieve gives clean galena, and the second and third sieves clean blende. Should the second compartment in J_3 produce middlings of galena and blende, instead of clean blende (which sometimes happens), they are, if rich in galena, fed back on the same jig, or, if poor, sent to the mills, reground, and treated together with corresponding stuff in Part II.

The reason for treating the fine wash-ore separately is, of course, to avoid regrinding of material already fine enough for concentration and to prevent as much as possible the formation of slime. The quantity of fine stuff worked up daily is about 22 tons.

II. *Coarse Material.*—The burnt ore together with the middlings from *a*, *b*, *c*, *d* and *e*, of Part I., is crushed by the rolls, R_1, and raised and sorted by a combined elevating-wheel and sizing-drum into two classes, respectively coarser and finer than 6 mm., of which the former goes back to the same rolls, and the latter is transported by a screw-conveyor to a sizing-drum with $2\frac{1}{2}$-mm. holes, where it is divided into two classes, *a* and *b*.

a. Finer than $2\frac{1}{2}$ *mm.*—This stuff goes first to a classifier (H_2) for separation of slime, and is afterward conveyed to a sizing-drum with 1-, $1\frac{1}{4}$-, $1\frac{1}{2}$- and 2-mm. holes. The different sizes are fed each on one or two four-sieved jigs, where mixed galena and blende is obtained in the first compartment, clean blende in the second and third, and middlings in the fourth. The products from the first and fourth sieves are further comminuted in the grinders and worked over again. Sometimes, when the ore is richer than usual, the tailings are also reground, but ordinarily they are worthless.

b. Coarser than $2\frac{1}{2}$ *mm.*—This is crushed by a pair of rolls (R_2) to a size below $2\frac{1}{2}$ mm., and, after the slime has been removed in a classifer (H_3), it is lifted by a link-belt elevator to two systems of sizing-drums with holes of 1, 1.4, 2 and 2.5 mm. diameter. After sizing, it is treated in three-sieved jigs, where middlings of galena and blende are retained in the first compartment, and clean blende in the second and third. The tailings from these jigs are reground and worked over, as is also the mixed galena and blende on the first sieve.

All middlings and tailings recrushed in the grinders are, after being freed from slime in hydraulic classifiers, sized in drums with 1-, 1.2-, 1.4 and 2-mm. holes, and further treated in jigs in exactly the same way as classes *a* and *b*. The sizing-drums are all conical, and the jigs are of the Hartz pattern, rather small and old-fashioned

and provided with beds of iron shot. Stroke and speed are generally the same as in the best modern practice. Rolls of large size and slow motion are employed; their diameter is 1.3 m.

The overflow from all the above-mentioned classifiers passes through a series of pointed boxes, and the separated coarser stuff is treated in slime-jigs. The overflow from the boxes is run over a number of *Spitzlutten*, which furnish material for the Rittinger tables. Stuff carried off with the overflow from the *Spitzlutten* is classified in a number of larger and smaller *Spitzkasten*, and distributed on round slime-tables.

There are 21 double Rittinger tables 2.55 m. long and wide; that is, the width of each single table is half of its length. They are inclined four to five degrees, and the number of bumps per minute varies from 140 to 180.

For slime-tables, both the concave and convex forms are used, and there are generally two tables, one of each kind on the same shaft, the convex table being placed at the top and the concave underneath. Tables of the convex shape are doing closer work than the others, because on the latter there is a contraction, and consequently an increased force of the current, toward the center, which causes some stuff to be carried off prematurely. The diameter of the concave tables is 4 m. and the inclination from 80 to 125 mm. per meter; the convex tables have a diameter of 4.5 m., and the same inclination. They make one revolution in two and one-half to three minutes. The surface is covered with cement and excellently finished.

The slime from the *Spitzkasten* is fed on the concave tables, where four products are obtained, viz., tailings, which go to the convex tables; middlings, which are sent back to the *Spitzlutten;* blende concentrates, and a mixture of galena and blende, which is separated on Rittinger tables. The products from the convex tables are, tailings, which are worthless, and go to waste; middlings, which are sent back to the same tables, and blende concentrates. Finally, there are a few *Stossherde*, on which some of the headings of the finest size, which are not sufficiently high in zinc, are worked over and enriched to a desired degree.

The works at Åmmeberg are at present run sixteen hours daily, and treat in that time 165 tons of material. Power is furnished by two water-wheels and one auxiliary steam-engine. The wash-ore contains, on an average, 20 per cent. of zinc and somewhat less than one per cent. of lead. The losses in concentration have been brought

down to 21 per cent., which is a remarkably good result in consideration of the slight difference in specific gravity between gangue and blende, and the unfavorable character of the ore. The blende concentrates are roasted at Åmmeberg in reverberatory furnaces before being shipped to the company's reduction-works at Liège, in Belgium. They contain, after roasting, about 38 per cent. of zinc and 6 per cent. of ferric oxide; but through the addition of the clean, picked blende, the average percentage of zinc is increased to 42.

It will hardly escape any one experienced in this kind of work, that a large part of the concentrates is obtained at a high expense, and that the saving of mineral has been carried beyond the ordinary limit of economy. This is explained by the fact that the Åmmeberg blende is more valuable than the ore from other mines belonging to the *Vieille Montagne*, on account of its purity, and is used as raw material for this company's superior brands of oxides. This is also the reason that so much care is taken to separate admixed galena, and that the ore, although not contaminated with iron pyrites in any considerable degree, receives a special treatment for removing that impurity.

To advocates of close sizing, the mill at Åmmeberg ought to be a model plant, with its numerous sizing-drums, ore-classes and subdivisions; but the object of this paper is to describe, not criticize, and I will leave it to the reader himself to form an opinion whether the same results could not be obtained in a simpler way and by a less complicated system.

The plant at Johannesborg, treating wash-ore richer in galena, needs no separate description. It is much smaller than the mill at Åmmeberg, the capacity being about 3000 tons per year, but general arrangements and working methods are the same in both.

The Works at Sala.

At Sala, dressing-works of modern construction were built in 1880 for the concentration of galena and other silver-bearing minerals of a complex nature, such as geocronite and boulangerite. The latter are particularly troublesome, because they occur disseminated in the gangue in such an extremely fine state of division that often their presence is marked only by a gray coloring of the rock. The gangue is limestone, and the galena is associated with other sulphurets, iron pyrites and blende, but not in any considerable quantity; and no regard is paid to them in the course of the work.

The material hoisted from the mine is dumped into several bins, where it is cobbed and hand-sorted into three classes, smelting-ore, wash-ore and rock. The first class is sent to the furnace and the second to the mill. Here it passes first through a jaw-crusher, and from there to a sizing-drum with 24-mm. holes. Stuff coarser than 24 mm. is distributed on a conveyor, along which a number of boys are engaged picking out smelting-ore and rock. What is left on the conveyor goes to a "sectorator," a newly-introduced machine claimed to crush and grind at the same time; but opinions about its merits and value differ widely. Having been reduced in size by the sectorator, the wash-ore is further comminuted by a pair of rolls with a diameter of 900 mm. Material finer than 24 mm. goes directly to the rolls.

From the rolls the crushed ore is lifted by a link-belt elevator to the sizing-drums, which are conical and four in number. The holes in the first one are 4 mm. in diameter; in the second, 3 mm.; in the third, 2 mm.; and in the fourth, 1.4 mm. Stuff coarser than 4 mm. goes back to the rolls, and finer than 1.4 mm. is delivered to three hydraulic classifiers (*Spitzlutten*). Intermediate sizes are treated in jigs with 4 compartments, one jig to each size, and the middlings from the last sieves are ground in Heberle mills before they are taken back for re-treatment. The coarser stuff separated by the three classifiers is worked in fine jigs. According to the size of the material treated, the jigs in this mill have a speed of 150 (for 4-mm. stuff) to 250 strokes per minute. The length of the stroke is 30 to 34 mm. for the coarse jigs and less in the fine jigs.

The overflow from the *Spitzlutten* passes over a system of four *Spitzkasten*, of which the first one furnishes material for two pairs of *Stossherde*, where an enriched product is obtained, but, not being sufficiently clean, it is re-ground in Heberle mills and fed by hand on a third pair of *Stossherde* for final concentration. The stuff from the last three *Spitzkasten* is conveyed to three convex slime-tables, one table for each *Spitzkasten*. They are 5 m. in diameter, inclined 25 mm. per meter, and make one revolution in two minutes. The surface is covered with cement. Lately, some experiments have been made to feed stuff on two opposite sides of the table and get the separation accomplished during a half-revolution, by which arrangement the capacity of each table would be doubled. I do not know whether the experiments have proved a success or not; but where only one kind of mineral is to be saved and the degree of concen-

tration need not be very high, I think the idea is worth trying. Middlings from the tables are sent back to the classifiers.

This is the outline of the process, which is simple enough, but the re-grinding and re-working of material, necessitated by the nature of the ore, has reached such proportions that in the twelve hours the mill is run daily only 16 tons of wash-ore pass through it. The stuff contains, before treatment, 3 per cent. of lead and about 10 ounces of silver per ton; the concentrates assay 20 per cent. of lead and about 46 ounces of silver. The losses in concentration are very high, 50 per cent. of the lead and 60 per cent. of the silver, the latter mainly passing away in the extremely minute particles of geocronite and boulangerite, which could not be saved by any kind of concentrating-machinery. This ore is evidently unfit for mechanical treatment and constitutes a proper material to be dealt with by chemists.

The mill at Sala is conspicuous by the absence of Rittinger tables, which occupy a very prominent position in the other concentrating-works in the country. When the plant was first started up, they were also used here; but it was soon found that the end-bump tables which had been employed in the old mills saved the lighter, complex minerals much more closely than the Rittinger tables, and the latter were discarded and replaced with *Stossherde*. The concentrates at Sala need not be of so high grade as at other places, because they are smelted at the mine, and there is no expense for transportation. Power for the mill is furnished by a turbine of about 40 horse-power.

The Works at Saxberg and Hellefors.

At Saxberg, in the province of Dalarne, a concentrating-plant was built in 1891 for treating an exceptionally difficult material. The wash-ore is a very intimate mixture of galena, blende and gangue; and although the working results are far from encouraging, it may be of interest to know what can be done with such stuff, under the most careful treatment.

Any attempt at hand-sorting of this ore would be useless. It is taken directly to the mill, crushed by means of a "sectorator" to a size of about 10 mm., and delivered by a link-belt elevator to a drum with 5-mm. holes. Each class goes to a pair of rolls and from there to sizing-drums with holes 1 mm. in diameter. Anything coarser than 1 mm. is taken back to the rolls by the elevator; finer than 1 mm. is sent to the concentrating-apparatus. The crushed stuff is

classified in 5 *Spitzlutten* and the coarse material from the first one is worked in a 4-sieved jig, which gives clean galena in the first compartment, and mixed products in the second, third and fourth. The middlings are re-crushed in Heberle mills, and sent back to the classifiers; the tailings are worthless. Stuff from the last four *Spitzlutten* is treated on four double Rittinger tables. From the first three clean galena, middlings and tailings are obtained. The middlings are re-ground and brought back by an elevating-wheel to the *Spitzlutten;* but the tailings are re-worked without any previous comminution, on another double Rittinger table, where middlings and worthless tailings are produced. The former are re-ground and sent back to the classifiers. The table fed from the fifth *Spitzlutten*, gives clean galena, middlings and tailings, which latter are rejected. The middlings are conveyed by an elevating wheel to two other double Rittinger tables. The products here are: impure galena, which is sent back to the classifiers for re-treatment; clean blende concentrates; and tailings, which go back with the galena.

The tables are 7 feet long and 4 feet wide (width of the double table 8 feet), make from 180 to 260 bumps per minute, according to the size of material treated, and are inclined about $5\frac{1}{2}°$. The surface is slightly charred and afterwards planed and polished, by which a high degree of smoothness is obtained, claimed to surpass even enameled duck. The stuff fed on them is properly classified, and the amount of feed and wash-water carefully regulated; conditions which, although they are often neglected, are essential to good work with Rittinger tables.

The overflow from the *Spitzlutten* passes over four *Spitzkasten*, of which the last three are double. They furnish material for three convex slime-tables, where five products are obtained, viz., clean galena; mixed galena and blende; clean blende; mixed blende and gangue; and tailings, which are worthless. The middlings are brought back by a sand pump to the classifiers and re-worked. The slime-tables are 5 m. in diameter, inclined from 8° to 12°, and make one revolution in 2 minutes. The surface is of cast-iron, polished and very smooth.

The wash-ore contains, on an average, 10 per cent. of lead and 20 per cent. of zinc, and the concentrates assay 50 to 60 per cent. in lead and about 42 per cent. in zinc. The separation of galena and blende is highly satisfactory. In spite of all efforts to accomplish a closer saving, the losses in concentration reach the high figure of 50 per cent., both of lead and zinc, and it is to be hoped that other

processes may be invented by which ores of this character can be more successfully treated. The capacity of the plant is only 26 to 28 tons in twenty-four hours, a consequence of the fine grinding and excessive re-working that are necessary. Power is furnished by a steam-engine of 45 horse-power.

Another concentrating-mill of recent date is situated at Hellefors, in the province of Westmanland. The nature of the ore, working methods and practical results are the same as at Saxberg, and a description of this plant would only be a repetition of previous statements and details. The two mills were built by the same parties and the arrangements are identical, the only difference being that the plant at Hellefors has a smaller capacity than the one at Saxberg.

This concludes my brief review of concentration in Sweden as it stands to-day, represented by the above-mentioned five works. Although they do not rank high as producers, quantitatively, they certainly deserve credit for what they accomplish under the many difficulties with which they have to contend.

MAGNESIA AND SULPHUR IN BLAST-FURNACE CINDER.

BY FRANK FIRMSTONE, EASTON, PA.

(Bridgeport Meeting, October, 1894.)

In the *Kaernthner Zeitschrift*, No. 2, 1881, p. 53, *et seq.*, Prof. Ledebur details some experiments on the power of various silicates of lime and alumina, magnesia and alumina, and lime, magnesia and alumina, to remove sulphur from pig-iron, from which it appears (as has long been recognized in practice) that the more basic the silicate the greater the quantity of sulphur it will take up; but also that for the same grade of silicate, this quantity is greatest for the lime-alumina silicate and least for the magnesia-alumina silicate, the lime-magnesia-alumina silicate occupying an intermediate position, and he concluded therefrom (p. 59) that, for the production of pig-iron low in sulphur, pure limestones are to be preferred to those containing magnesia.

Much the same opinion has been published elsewhere on high authority, and lately (not to multiply references) by Sir Lowthian

Bell (*The Iron and Steel Institute in America in* 1890, p. 160) and Mr. Stead (*Jour. Iron and Steel Inst.*, No. II., 1892, p. 242).

Ledebur's experiments seem conclusive as to the exact point he investigated; but long experience in Eastern Pennsylvania and elsewhere, shows that there is no difficulty in making iron low in sulphur, using what is practically a normal dolomite; and the following analyses prove that, under certain circumstances at least, the sulphur in the pig is *reduced* by substituting dolomite for limestone containing about 5 per cent. of magnesia.

The ore used contained from less than 0.01 to 0.09 per cent. of sulphur as a maximum, but was commonly pretty constant at about 0.03 per cent.; the coke (Pocahontas) was very regular at from 0.60 to 0.75 per cent.

For about two years previous to October 5, 1891, the limestone used was about of the following composition:

	Per cent.
Lime,	44.52
Magnesia,	5.45
Silica,	7.18
Alumina and iron,	not det.

On that day we began to use a dolomite containing:

	Per cent.
Lime,	31.42
Magnesia,	20.95
Silica,	2.07
Iron and alumina,	1.27

The cinder made had about the following composition:

	Cinder before Oct. 5, 1891. Per cent.	Cinder after Oct. 5, 1891. Per cent.	
SiO_2,	39.95	37.82	38.37
Al_2O_3,	5.48	4.05	3.98
CaO,	47.39	35.46	35.18
MgO,	5.38	19.88	19.48
FeO,	0.94	0.74	1.22
CaS,	not det.	1.82	1.53
$\frac{O \text{ in } SiO_2}{O \text{ in bases}} =$	1.17	1.01	1.04

(The oxygen in the oxide of iron is not included).

The following tables give the sulphur and silicon in the iron made for periods of thirty-four days before and thirty-four days following the use of dolomite.

TABLE I.—*Silicon and Sulphur in Pig, when Limestone was Used.*

Date.	Grade.	Silicon.	Sulphur.	Date.	Grade.	Silicon.	Sulphur.
		Pr. ct.	Pr. ct.			Pr. ct.	Pr. ct.
Sept. 1.....	Gray Forge	0.590	0.096	Sept. 17.....	No. 2	1.490	0.033
" 4.....	No. 2	1.150	0.059	" 19.....	Gray Forge	0.539	0.087
" 5.....	Gray Forge	0.852	0.069	" 20.....	No. 2	1.010	0.039
" 6.....	No. 2	0.819	0.064	" 21.....	No. 2	1.240	0.031
" 7.....	No. 2	0.676	0.091	" 22.....	No. 2	1.200	0.046
" 8.....	No. 2	1.320	0.026	" 23.....	No. 2	0.954	0.086
" 9.....	No. 2	0.947	0.064	" 25.....	No. 2	0.802	0.076
" 10.....	No. 2	1.520	0.035	" 28.....	No. 2	1.22	0.041
" 11.....	No. 2	1.830	0.044	" 30.....	No. 2	0.813	0.039
" 14.....	No. 2	0.807	0.077	Oct. 3.....	No. 2	1.440	0.034
" 15.....	No. 2	1.020	0.032	" 5.....	No. 2	0.786	0.053
" 16.....	Gray Forge	0.596	0.104				

TABLE II.—*Silicon and Sulphur in Pig, when Dolomite was Used.*

Date.	Grade.	Silicon.	Sulphur.	Date.	Grade.	Silicon	Sulphur.
		Pr. ct.	Pr. ct.			Pr. ct.	Pr. ct.
Oct. 7.....	No. 2	0.681	0.070	Oct. 27.....	No. 2	0.414	0.045
" 8.....	No. 2	0.891	0.057	" 29.....	No. 1	1.10	0.027
" 11.....	No. 1	0.955	0.028	" 31.....	No. 1	1.09	0.028
" 13.....	No. 1	1.30	0.016	Nov. 2.....	No. 1	0.818	0.041
" 15.....	No. 1	1.20	0.008	" 4.....	Gray Forge	0.508	0.070
" 17.....	No. 1	0.549	0.059	" 6.....	No. 1	1.14	0.025
" 19.....	No. 2	0.535	0.052	" 8.....	No. 1	0.910	0.020
" 21.....	No. 1	1.04	0.025	" 10.....	No. 1	0.428	0.048
" 23.....	No. 1	1.13	0.022	" 12.....	No. 1	0.621	0.023
" 25.....	No. 1	1.02	0.025				

Table III., in which the results are arranged according to the silicon contents, shows plainly that for about the same content in silicon the iron made with dolomite is pretty regularly lower in sulphur than that made with limestone.

The dolomite has been regularly used ever since; and a comparison of the iron made over the whole period before its use, with that made since, agrees perfectly with the above tables, and leaves no doubt that the iron is both lower in sulphur on the average, and far more regular in this respect, when the flux is dolomite, in spite of a very much lower percentage of lime in the cinder.

The cinder made was at the rate of about $1\frac{1}{2}$ tons to the ton of iron.

When using limestone, with the silica in the cinder at about 39 to

TABLE III.—*Results Arranged According to Silicon-Contents.*

USING LIMESTONE.			USING DOLOMITE.		
Grade.	Silicon.	Sulphur.	Grade.	Silicon.	Sulphur.
Gray Forge.....	0.539	0.087	No. 2.....	0.414	0.045
"	0.590	0.096	No. 1.....	0.428	0.048
"	0.596	0.104	Gray Forge.....	0.508	0.070
No. 2.....	0.676	0.091	No. 2.....	0.535	0.052
"	0.786	0.053	No. 1.....	0.549	0.059
"	0.802	0.076	"	0.621	0.023
"	0.807	0.077	No. 2.....	0.681	0.070
"	0.813	0.039	No. 1.....	0.818	0.041
"	0.819	0.064	No. 2.....	0.891	0.057
Gray Forge.....	0.852	0.069	No. 1.....	0.910	0.020
No. 2.....	0.947	0.064	"	0.955	0.028
"	0.954	0.086	"	1.020	0.025
"	1.010	0.039	"	1.040	0.025
"	1.020	0.032	"	1.090	0.028
"	1.150	0.059	"	1.100	0.027
"	1.200	0.046	"	1.130	0.022
"	1.220	0.041	"	1.140	0.025
"	1.240	0.031	"	1.200	0.008
"	1.320	0.026	"	1.300	0.016
"	1.440	0.034			
"	1.490	0.033			
"	1.520	0.035			
"	1.830	0.044			

40 per cent. the cinder "slacked"* (fell to fine powder) as it cooled, and there was more or less trouble from the burning-out of the brick-work around and over the tuyeres. This was at once aggravated by any attempt to increase the proportion of limestone used; and the cinder analyzed may be regarded as about at the practical limit in this direction. All trouble on this head at once disappeared with the use of dolomite, and did not recur even with much lower percentages of silica in the cinder (down to 35 per cent.) than those given above, and (partly at least) in consequence, the furnace-work became more regular and the burden carried heavier. The temperature of the blast was kept at about 750° Fahr. during the time covered by the iron-analyses.

It seems that even a moderate amount of magnesia has a beneficial effect on the fusibility and fluidity of such cinder; for there was no serious difficulty in this case in running with a "slacking" cinder

* "Slacking" of cinder (as is well-known) takes place even when it is put into a well closed bottle and is not affected to any appreciable extent by moisture in the air.

of the above composition (Al_2O_3, 5.48; CaO, 47.39; MgO, 5.33), while at Longdale, Va., where the normal cinder was about

	Per cent.
SiO_2,	42.35
Al_2O_3,	7.35
CaO,	41.67
MgO,	1.09

the greatest care was needed to avoid getting the cinder to the slacking-point—a few days' work on slacking cinder resulting in such an accumulation of infusible matter in the hearth as would cause the tuyeres to become dark and the brick-work above and around them to burn out so fast that it was difficult to keep the blast on the furnace.

	1	2	3	4	5	6
	Pr ct.	Pr. ct.	Pr. ct.	Pr. ct.	Pr. ct.	Pr. ct.
SiO_2	24.92	28.74	27.01	26.46	27.53	27.64
Al_2O_3	11.62	13.46	13.00	17.80	18.06	16.24
CaO	35.84	27.72	39.30	35.50	36.30	37.06
MgO	9.16	9.70	9.00	10.80	11.10	10.60
Residue	17.40	17.88	10.82	8.18	6.07	9.04
	98.94*	97.50	99.13	98.74	99.06	100.58
Composition of Residues.						
Al_2O_3	68.11	67.98	65.88	68.92	68.72	74.00
CaO	8.48	9.39	14.96	.....	.....	.....
MgO	23.41	22.63	19.16	31.08	31.28	26.00
	100.00	100.00	100.00	100.00	100.00	100.00
Total Al_2O_3 and MgO in Cinder.						
Al_2O_3	23.64	25.36	20.05	.....	.....	.....
MgO	13.39	13.80	11.05	.....	.....	.....

As is well known, the use of magnesian limestone is condemned by Percy,† as also in other British works on iron-smelting, and this precept is of pretty old date,‡ and is repeated in recent memoirs.§

* $\frac{O \text{ in } SiO_2}{O \text{ in bases}} = 0.688$, excluding spinel.

† *Metallurgy, Iron and Steel*, p. 507.

‡ *Voyages Métallurgiques*, 2d Ed., Paris, 1837, t. i., pp. 324–328; *Manufacture of Iron*, Philadelphia, 1837, p. 26. (Reprinted from the *Library of Useful Knowledge.*)

§ Mr. Stead, in *Jour. I. and St. Inst.*, No. II., 1892, p. 243.

In the face of this, we have the fact of many years' use of dolomite, by preference, in the Lehigh Valley and elsewhere, and contrary opinions by several writers.* An explanation of the contradiction is, I think, indicated by the circumstance that in all the cases in which I have noted the successful use of dolomite, the alumina in the cinder is low compared with that in cinder commonly reported from British iron-works, and the first publication looking to an explanation seems to be Muirhead's communication in *Iron*.† He there gives the foregoing analyses of cinder (page 502).

He notes that when making cinders Nos. 1, 2 and 3, the furnace was working hot, but the cinder was stiff and refractory and the output small; and this he attributes to the formation of the compound of alumina, lime and magnesia (spinel) which remained as an insoluble residue when the cinder was dissolved for analysis in the ordinary way. The formation of this compound seemed to result from a considerable increase in the magnesia over that present when the working was satisfactory. He notes that the working was better with No. 4 cinder. With No. 5 the grade of the iron was bad, with small output; No. 6 gave mixed grade and moderate output.

Mr. Stead‡ also reports a viscous cinder, and mottled and white iron, when magnesian limestone was used with Cleveland iron-ore, the cinder in that district being also relatively high in alumina.

Prof. Åkerman states § that spinel separates in cinders which are more basic than a singulo-silicate, and which contain alumina and magnesia to the extent of 10 per cent. of either substance, with at least 20 per cent. of the other—below which limits spinel does not usually crystallize out.

It seems probable that trouble due to separation of spinel may arise from an increase in alumina in a cinder high in magnesia, as has been indicated above for the opposite case of an increase in magnesia, the cinder being high in alumina.

In the summer of 1890 Mr. P. W. Shimer informed me that he had observed in a cinder from the Musconetcong Iron Works, Stan-

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* Kenneth Robertson: *Trans.*, vol. i., p. 144; De Vathaire: *Etudes sur les Hauts Fourneaux*, 1866, pp. 31, 40; *Construction et Conduite des Hauts Fourneaux*, 1885, pp. 150, 157, 158.

† Vol. xvi. (1880), p. 292.

‡ *Jour. I. and S. Inst.*, No. II., 1892, p. 243.

§ *Stahl und Eisen*, 1886, No. 6, p. 393. (Translation from *Jern-Kont. Annal.*)

hope, N. J., a residue insoluble in acids after fusion of the cinder with sodium carbonate, which he supposed to be spinel, and which amounted to 7.8 per cent. of the weight of the cinder. Unfortunately, he was not then able to make a full analysis of this residue, and we are not now able to get an authentic sample of the cinder in question; but a partial examination, which showed the presence of much alumina, and a comparison with the spinel from the Andover cinder, next to be mentioned, leave no reasonable doubt as to its character.

Through his kindness and that of Mr. I. P. Pardee, of the Musconetcong Iron Works, I am able to give the following particulars:

The normal cinder at Stanhope may be taken as about:

	Per cent.	
SiO_2,	35.99	
Al_2O_3,	8.84	
CaO,	28.80	
MgO,	20.67	
FeO,	0.45	
CaS,	2.52	$\frac{O \text{ in } SiO_2}{O \text{ in bases}} = 0.93.$
TiO_2,	1.40	(FeO not included.)

The cinder analyzed by Mr. Shimer contained

	Per cent.	
SiO_2,	32.64	
Al_2O_3, TiO_2,	13.62	
CaO,	31.10	
MgO,	21.80	$\frac{O \text{ in } SiO_2}{O \text{ in bases}} = 0.82$, approximately.
S,	not. det.	(Not including residue.)

The imperfect examination of the residue showed that the titanic acid present was about the same as in the normal cinder.

Mr. Pardee informs me that with this cinder the hearth choked up badly, and they had very great difficulty in keeping the cinder-notch and the tapping-hole open, although the iron made was gray.

In this case the ill effects of the spinel on the fluidity of the cinder may have been aggravated by the considerable amount of titanic acid normally present in the Stanhope cinder.

Mr. Shimer has also detected spinel in a cinder from the Andover Iron Works, for which we are indebted to Mr. J. C. Kent.

This cinder contained:

	Proximate Per cent.	Ultimate Per cent.	
SiO_2,	37.39	37.40	
Al_2O_3,	9.74	11.40	
CaO,	21.91	21.91	
MgO,	21.68	22.37	
FeO,	0.85	0.85	
CaS,	2.27	2.27	S = 1.01.
TiO_2	1.18	1.20	
Spinel,	2.38	——	
Alkalies,	not det.	not. det.	
	97.40	97.40	

$$\frac{O \text{ in } SiO_2}{O \text{ in bases}} = 0.993 \text{ (spinel and FeO not included.)}$$

The composition of the spinel residue was:

	Per cent.
Al_2O_3,	69.65
MgO,	29.01
TiO_2,	0.59
SiO_2,	0 24
	99.49

In this case no disturbance due to the presence of the spinel was observed, but the cinder is less basic and the quantity of spinel much less than in the Stanhope cinder.

I think it is fair to conclude, from the above, that the old rule against magnesian limestone was well founded, so far as it referred to cinders high in alumina; and although I have only the Stanhope example, yet it seems that when the magnesia in the cinder is high —say 20 per cent. or more—then, in the absence of further data, any increase in the alumina above 10 per cent. should be made cautiously.

THE GEOLOGICAL STRUCTURE OF THE RINGWOOD IRON MINES, NEW JERSEY.

BY FRANK L. NASON, NEW BRUNSWICK, N. J.

(Bridgeport Meeting, October, 1894.)

DURING the months of June and July of the present year, the writer made a special geological survey of the mining property of Messrs. Cooper and Hewitt, at Ringwood, New Jersey. Some of the results of this survey are presented to the society through the courtesy of Hon. Abram S. Hewitt.

The magnetite iron-bearing rocks of the Archæan highlands of New Jersey and of New York and Pennsylvania, have been already described by the writer* as belonging to the Mt. Hope type. This rock consists essentially of orthoclase and plagioclase feldspar, grains of quartz, and occasionally octahedral crystals of magnetite, as well as numerous grains of the same mineral. The accompanying minerals are zircon, rutile and (rarely) molybdenite. Accompanying this rock is another, which preserving the same general physical characteristics, presents some slight mineralogical differences. The feldspar and quartz remain the same in quantity and general appearance; but the proportion of magnetite grains has appreciably diminished, and in their places the basic minerals, hornblende and biotite appear. This rock seems to graduate from the first described type both along and across the strike. It must be rigidly excluded from the rock which in the papers referred to has been described as the Oxford type. The two cannot be confused, as their physical and mineral characteristics are easily distinguishable and are constant. The latter rocks are well developed, and are never found, in the writer's experience, in connection with the iron-ore deposits of the States mentioned. In short, so striking is this fact that they are known as "barren rocks" among the miners.

Interstratified with the Mt. Hope rock at Ringwood, and in the above-mentioned States as well, are found all of the great iron-ore deposits. Associated with the iron-ore deposits are highly basic rocks composed of pyroxene, hornblende, biotite and feldspar. Free quartz, except that which is evidently secondary, is rarely if ever found. Accompanying this bed or stratum are occasional stringers of iron-ore, from a thickness of a few inches down to narrow bands where grains of magnetite are scattered irregularly among the scales of biotite, pyroxene or hornblende, or where all four of these minerals are mingled in varying proportions.

In many of the larger mines like the Dickerson, Mt. Hope, the Port Oram mines, Hurdtown, etc., the lenses of iron-ore lie in immediate contact with the Mt. Hope type of rock or magnetite-gneiss. At other mines, the ore-bodies are separated from this rock by strata of varying thickness, which consist almost wholly of the kind described in this paper where magnetite grains appear to be replaced by scales of biotite. This is especially true at the Peters mine. (See contour-map, Fig. 1).

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* *Ann. Rep. Geol. Sur., N. J.*, 1889, p. 30; and *Am. Geol.*, July, 1893, p. 25.

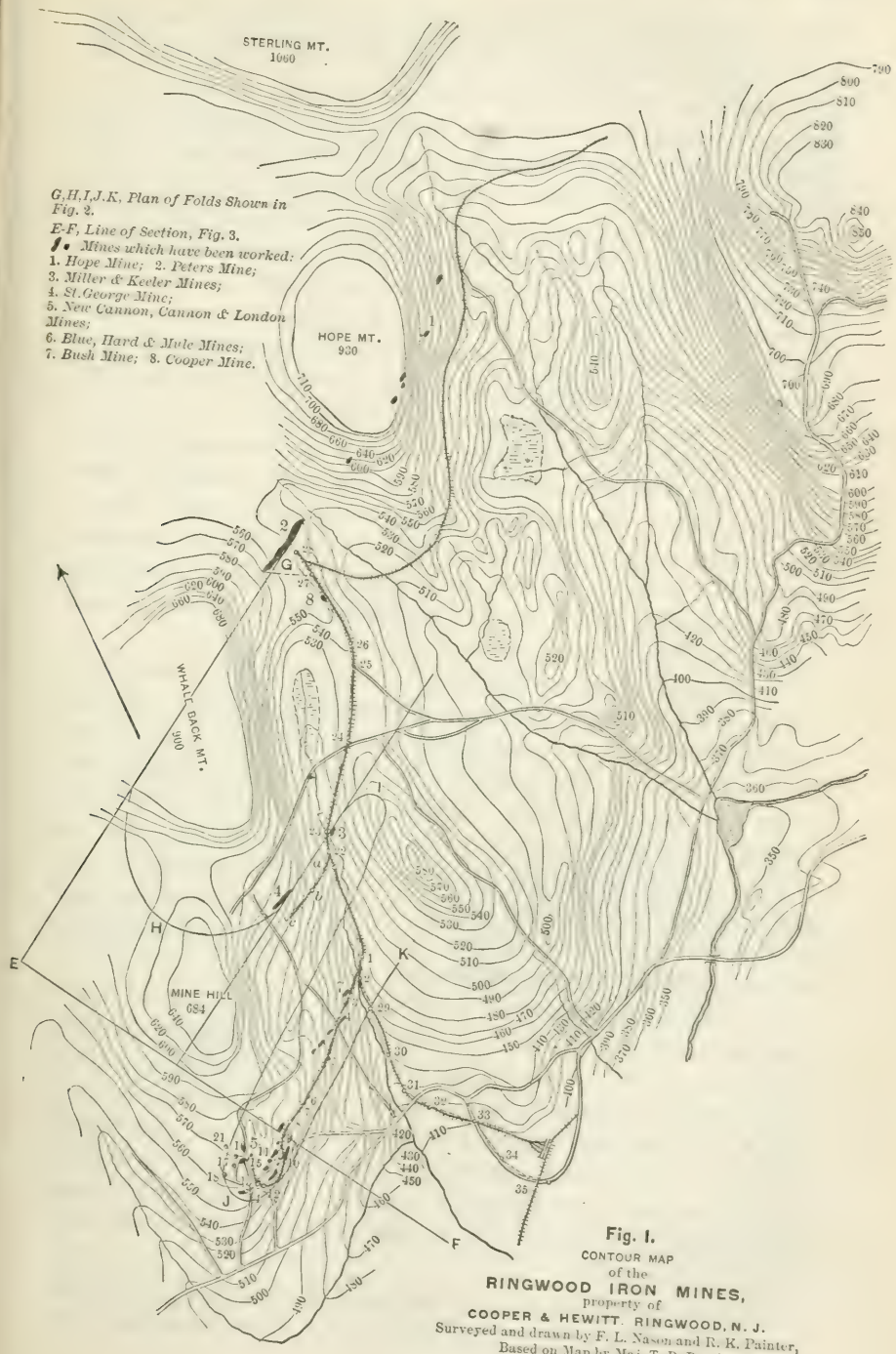


Fig. 1.

CONTOUR MAP
of the
RINGWOOD IRON MINES,
property of
COOPER & HEWITT. RINGWOOD, N. J.
Surveyed and drawn by F. L. Nason and R. K. Painter,
Based on Map by Maj. T. B. Brooks.
Scale 1 Inch = 1650 Feet.
Am. Back Note Co. N. Y.

The position of the ore-bodies in the rock is, without exception, that of exact conformability with the planes of stratification. In other words, no beds of iron-ore have been observed to cut across the plane of bedding as they would do in case of their secondary origin, either from their being of eruptive origin (an idea now obsolete), or from having been deposited in veins or fissures of fractures. It is true that in some instances the workable ore oscillates from side to side, from foot- to hanging-wall, or *vice versa*, and consequently lies at a steeper or a flatter angle than the country-rock; but this oscillation may result from two causes. In the first place, the ore may have become too lean to pay for extraction, and being left, may give the appearance of a flattening angle; or secondly, an unequal thinning of the ore-body would produce the same result.

With regard to their position, referred to a horizontal plane, it may be said that the ore-bodies occupy the same relation as their enclosing rocks. The strike is generally northeast, with a dip to the southeast varying from 40° to 90° and a northeast pitch varying from 0° to 75°. Dip and strike are more readily observed in the country-rock, owing to more extended outcrops, while the pitch is more readily noticed in the bodies of ore.

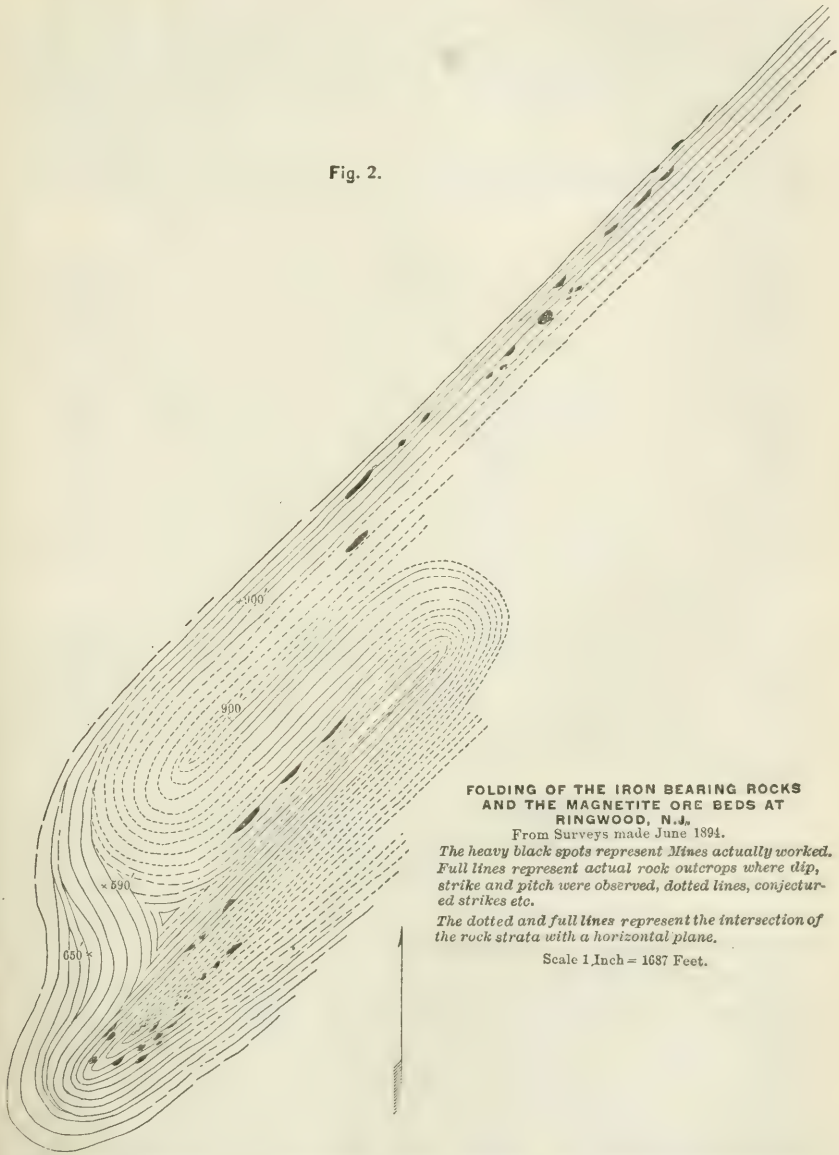
The shape of these ore-bodies or "shoots," as they are commonly called by the miners, is very constant except as to size. They are usually lenticular. A cross-section of an ore-shoot perpendicular to its strike, somewhat generalized, is shown in Fig. 6, where the letters *a–b* represent a plane intersected by the shoot as above described. The lenses are usually compressed in places along the line of pitch as indicated by the letters *c–d*, *e–f*. A longitudinal section along the line *g–h* is shown in Fig. 7. In fact a shoot of ore may be likened to a series of elliptical lenses, fused or welded together at the extremities of their major axes.

For any given outcrop of ore there may be, and often are, more than one shoot of ore. In the Peters mine at Ringwood, there are three such shoots. Two of these shoots lie in the same plane, the third lying in another plane in the hanging-wall, as is shown in Fig. 8. These shoots appeared outcropping in the manner shown by Fig. 9. This general description will apply in every particular to the deposits at Ringwood. Except in a few structural points, composition of ore, size of shoots, etc., the description of one mine would apply to all.

At Ringwood no one of the ore-bodies has yet been completely exhausted. There are twelve mines which have been large pro-

ducers, some of them with histories reaching back of 1760.* All of them are now idle, so that no inspection of their structure is at

Fig. 2.



FOLDING OF THE IRON BEARING ROCKS AND THE MAGNETITE ORE BEDS AT RINGWOOD, N.J.

From Surveys made June 1894.

The heavy black spots represent Mines actually worked. Full lines represent actual rock outcrops where dip, strike and pitch were observed, dotted lines, conjectured strikes etc.

The dotted and full lines represent the intersection of the rock strata with a horizontal plane.

Scale 1 Inch = 1687 Feet.

present possible, save in those mines which are more or less open and above the level of standing water. From the study of the Blue,

* For a description of these mines and references to other literature see Dr. Cook's *Geology of New Jersey*, 1868, p. 546 *et seq.*

Little Blue, Cannon, London, New Cannon, Bush and St. George mines the following facts can be stated:

The central part of the lenses usually carries the more solid ore. At the upper and lower edges of the lenses, especially the lower edges, the ore is gradually replaced by heavily basic minerals, such as black pyroxene, hornblende, and biotite, with thin stringers of iron-ore. At these edges and at the contact of the lenses with the foot-wall, the ores begin to be, in many places, heavily charged with almost pure phosphate of lime, in compact granular masses.

The thickness of the ore-bodies or lenses, measured from foot- to hanging-wall, varies from 6 to 50 feet. From cap- to bottom-rock the distance is not so easily determined; but in the case of the Peters mine the major axis of the section measured about 100 feet as a maximum. In working down the slope of No. 1 shoot of the Peters mine, the mass of ore took approximately the shape shown in Figs. 6 and 7. As the shoot began to pinch, as is shown in the above sections at *c–d*, *e–f*, the ore began to be replaced by the basic minerals above described. Then, after passing the pinch, the ore again widened out to nearly or quite its normal size until another squeeze, when the same phenomenon was repeated.

Peters shoot No. 1 was worked for a distance of 900 feet on the pitch of about 35° to the N.E. At the bottom the ore is reported to have grown very lean in places, and the vein, without diminution in size, to have divided into narrow stringers of ore, interbanded with strings of biotite, pyroxene, etc. At this point work was abandoned.

The Hope mines, about 2400 feet N.E. of the Peters mine, have not been worked for a long time, and little information can be gained of them. From what could be observed at the surface and from the examination of the waste-heaps, the characters of these deposits do not differ essentially from those of the mines described.

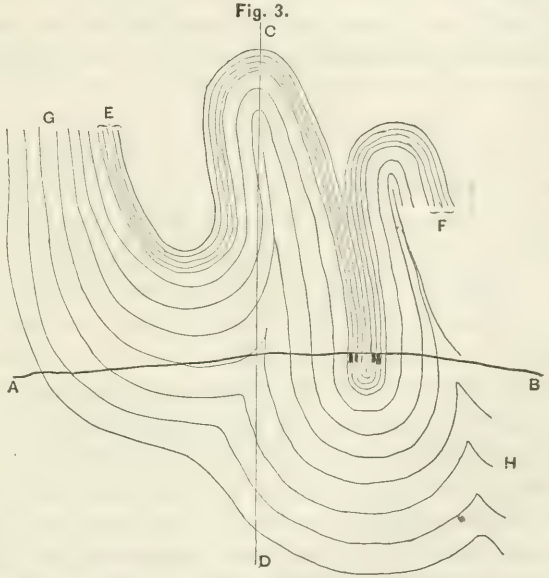
By consulting the accompanying contour-map, Fig. 1, on which the mines have been located accurately with reference to each other, it will be seen that they can be grouped into two quite distinct clusters. Moreover, these two groups exhibit some distinct structural differences. For convenience of description, the S.E. group will be referred to as the Blue Mine group; the N.E. as the Peters Mine group.

This latter group is shown in Fig. 4, on a plane dipping S.E., at an angle varying between 60° and 80°, with an average of about 70°. The pitch of the shoots, with one exception, averages about

35°. The one exception is the Hope, which has a pitch of 55°. This exceptional pitch may be explained on the supposition that the excess over the average of 35° is due to the angle of original deposition, and not to faulting and displacement. It is believed that this assumption can be readily accepted, since the rocks on the slope of Hope Mountain, only a few feet above the mine, have the usual pitch of about 35°.

At the Blue Mine group a very different set of conditions prevails.

Fig. 3.



VERTICAL SECTION ALONG LINE E-F ON CONTOUR MAP FIG. 1.
C-D, Vertical line. A-B, Present contour of surface, Rocks above this line have been denuded.
E-F, Iron bearing strata. G-H, barren sub-strata.
Magnetite iron ore
Scale 1 Inch = 1650 Feet.

In this group there are eight distinct mines. All of them, with one exception, have a dip of 90°. The pitch is 60° N.E. The great difference in the effect of the pitch in these two groups of mines may be more readily appreciated if the statement be made in another form. On a pitch of 35° the ore goes underground at the rate of 1.5 feet in 2 feet, while on a pitch of 60° the ore descends at the rate of 3 feet in 2 feet. The one exception to the rule of the Blue Mine group is an unimportant opening near the Cannon mine, where the pitch is 45°, the dip remaining the same.

Between these two groups there are three mines, which do not fall into the other groups and do not form a group by themselves. These

mines are, beginning at the Blue Mine group, the Bush, the Keeler, Miller and St. George, and the Cooper mine, just south of the Peters. The dip of the Bush mine is 70°, pitch 45°, of the Keeler, Miller and St. George (all on the same line of attraction) the dip is 63° and the pitch 40°, while the dip and pitch of the Cooper are about the same as that of the Peters group. Thus these intermediate mines seem to be, in some way, the connecting link between the two groups of mines described. The reason for this assumption will be pointed out later on.

The mine-rocks accompanying these two groups are as noticeable as is the difference in amount of pitch and dip. In the Peters Mine group there are occasional small stringers and bunches of pegmatite-granite, but the total amount is small, as compared with the regular mine-rock. This rock (the mine-rock) is not badly crushed and broken, but as the great length of the ore-shoot seems to indicate, reaches for long distances little broken. While granite appears on the sides of both Whaleback and Hope mountains, it is not in great abundance. Though in places it is difficult to make out either the dip, pitch or strike, yet when either or all of these are observed, they do not depart more from the normal direction than might be expected in such rocks. No minerals are found in the mines, except those which have already been mentioned as occurring in such ore-deposits.

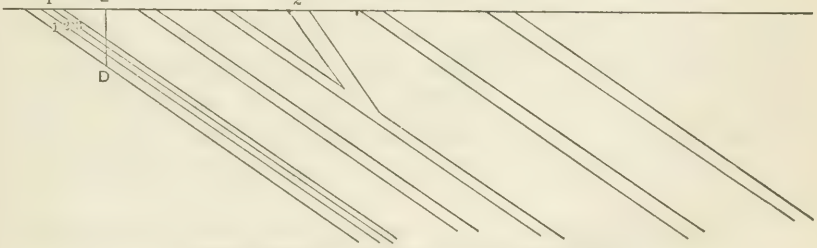
At the Blue Mine group there is a very decided difference. The mine-rock is shattered and broken; the ore-shoots are faulted and slipped, and are bent aside from their normal strike for short distances. The crevices of the shattered mine-rock are filled with coarse pegmatite-granite. A bright salmon-colored feldspar occurs in great masses, and large pyroxene crystals from 1 to 6 inches in length are formed, partly in the quartz and partly in the feldspar. In the clear, glassy quartz are frequently found large tabular crystals of orthoclase. In the slips or "vugs" of the mine-rock and of the country-rock adjoining, the following minerals, in small but very perfect crystals, have been found: flesh-colored stilbite in large radiated clusters; epidote, crystalline, massive, and in small but very fine crystals; heulandite in tabular crystals; calcite or dog-tooth spar, "bergkork," "mountain leather," asbestos and rutile. These minerals are all of secondary origin, and their constituents were derived, probably, from the decomposition of the containing-rocks under the combined influences of heat and pressure.

The surrounding country-rock is also much broken and displaced by granitic rocks. Along the southwestern side of the mines, nearly

parallel with the brook shown on the contour-map, Fig. 1, the granites are also very abundant. From this point, and running nearly due north along the western slope of Mine hill, near the point H on the map, granite seems to be the most abundant. At this point (H) nearly the whole surface is covered with soil, and the nature of the rocks cannot be determined. To the west of the wood-road, southwest from the mines, the same thing is true. The ground here is swampy.

In spite of the abundance of granite and the crushed and broken appearance of the gneiss in the vicinity of the Blue Mine group, careful observation shows a very decided and persistent structure.

Fig. 4.



PLAN OF THE ORE-SHOOTS OF THE PETERS GROUP

1, Peters Mine; 2, Hope Mine;
1, 2, 3, the three Ore-Shoots of the Peters Mine.
E-D, Vertical depth to which Shoot No. 1 has been worked.
(Length of shoots over 200 feet conjectural, except at Peters Mine.
Scale 1 Inch = 1650 Feet.

Beginning at the point K (contour-map, Fig. 1), the rocks show a decided parallelism as to strike with that of the Bush mine (No. 7 on the map). Although the outcrop is not continuous, occasional spots are bare of soil, showing a persistent and tolerably regular strike, varying only from 5° to 10°, to a point about 330 feet southeast of where the line, E F, crosses K J. At this point the strike of the rocks begins to bear to the eastward, and near the point J they have nearly a northwest-southeast strike. There is a very prominent outcrop at this point, the rocks projecting from the surface to a height of about 12 feet. There is a succession of these rocky, precipitous bluffs, extending parallel to the line, E F, for a distance of about 1300 feet, and 200 feet southwest of the point J. Following this bluff around to a point 150 feet north of the point J, the rocks resume their normal northeast strike. A very important fact to be noted is, that in the vicinity of the point, J, the pitch of the rocks has entirely disappeared. In other words, the pitch and the dip have become one and the same thing.

At the points I and H, the rocks are deeply covered with glacial drift, and neither the dip nor strike can be observed. Along the mines 3 and 4 (Keeler and St. George), there are places where both can be observed. The dip is 63° southeast, and the pitch 40° northeast. Along the line E G, on Whaleback mountain, the dips and strikes are normal. On the east side of Mine hill the strike is north 20° west, and the dip 35° towards the east and 20° north. Near the point, H, on the curve, the rocks strike due northwest, the pitch and dip becoming the same; that is, 40° to the northeast. South of this the strike, though not very distinct, appears to be north and south.

From these observations, the curved line, G H I J K, representing the course of the strike of the rocks, was platted on the map, showing the probable relations of the mines to each other and to the surrounding country. Fig. 2 shows the completed curve of foldings, as the outcrops of the dipping and pitching rocks intersect the plane of the horizon. The full lines represent the strike of the rocks as actually observed; the dotted lines are deduced from the known strikes.

From this figure, if it rightly represents the folds in the rocks, it will be seen:

1. That the Peters Mine group, the Keeler and St. George mines, and the Blue Mine group were originally in the same horizontal plane or bed of iron-bearing gneiss.

2. That they were afterwards folded, making a sharp synclinal at the Blue Mine group; an anticlinal, in the western slope of which lie the Keeler and St. George mines; and a synclinal fold, on the western slope of which lies the Peters Mine group.

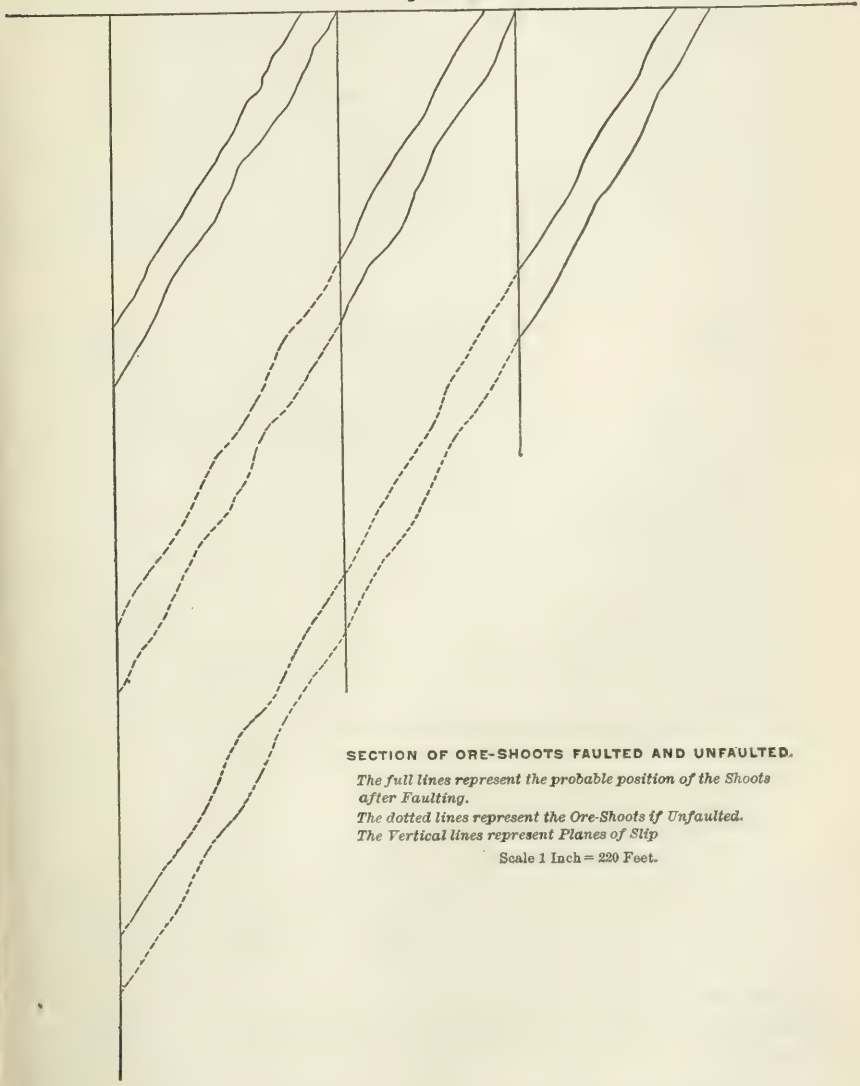
3. That afterwards, or during the process of folding, the rocks were faulted or broken in a northwest-southeast direction, and the folds given a tilt or pitch to the northeast.

4. Finally, partly by weathering, but more largely by glacial action, these folds were reduced to their present contours.

It will doubtless be noted in this connection, that all of the iron-beds cannot possibly be aligned in the same single stratum. In the Blue Mine group, for instance, the Blue mine and Mule mine (see Fig. 10) form a tolerably correct alignment, while nearly parallel, and separated by only 90 feet of rock, lie the Little Blue and Red mines. Again, on the western side of the group, the Cannon and London (the two are practically one and the same shoot of ore) are parallel to the New Cannon. Farther to the northeast, the Bush mine is

separated by about 100 feet of rock from a series of openings to the south of, and nearly parallel to, it (see Fig. 10).

Fig. 5.



SECTION OF ORE-SHOOTS FAULTED AND UNFAULTED.

The full lines represent the probable position of the Shoots after Faulting.
The dotted lines represent the Ore-Shoots if Unfaulted.
The Vertical lines represent Planes of Slip

Scale 1 Inch = 220 Feet.

If, now, we should flatten out the fold of the Blue Mine group, we should find the Blue and the Little Blue lying on one side of a plane or stratum of rock, while the Cannon and London would lie on the other. The Blue and Red mines would be separated verti-

cally by at least 90 feet of rock. This would simply mean that during the deposition of the iron-bearing strata, conditions were successively favorable and unfavorable for the deposition of beds of pure magnetite. If, now, we refer to the Peters Mine group, we shall see that mine 8 (the Cooper mine) is separated from the Peters mine by a distance of about 500 feet. This would mean that the iron-ore-bearing strata are in this place at least 600 feet in thickness, and probably much thicker. The fact that the individual shoots of ore do not lie in the same stratum is also illustrated in Fig. 8 (vertical cross-section of Peters mine), where it will be seen that shoot No. 1 lies in the hanging-wall and wholly outside the stratum in which occur shoots Nos. 2 and 3 of the same mine.

A vertical cross-section through the Blue Mine group along the line E F (see contour-map, Fig. 1), shows the probable shape of the folds before they were swept away by erosion. The four black dots on the lower side of the line A B, Fig. 3, show the position at this point of the Blue Mine group. A reference to the contour-map will show that the folds at the Blue Mine group and between this and the Keeler mines must have been very tight, while the fold between the Keeler mine and the Peters group must have been less compressed. The ratio of thickness among these three groups: Blue, Peters and Keeler, is nearly $\frac{3}{4} : 1 : 2\frac{1}{2}$. Another very conclusive reason for assuming a tight fold at the Blue Mine group is the position and condition of the ore-shoots and the surrounding rocks. As has already been remarked, the ore-shoots and the accompanying rocks are quite vertical, and both are badly crushed and shattered and are filled with granite rocks. These are the usual accompaniments of great disturbances.

It will also be noticed that the Blue Mine group is the only one represented along the line of section and at the present surface. The reason for this is quite evident. Carrying the Keeler and St. George mine out to the line E F (contour-map) on the angle of pitch, they would intersect the vertical plane of E F at about 1000 feet above the present surface. The trough of the Peters synclinal would intersect the same plane at an altitude of about 700 feet. The Peters ore-shoot, if carried to the point of intersection with this same plane at the angle of its present pitch, would intersect this plane at a height of about 2300 feet above the present surface. It is therefore assumed that the rock which actually reaches the surface along this line is the barren substrata, G H (Fig. 3). As has already been

pointed out, this is not a baseless assumption. A careful study of dips, strikes and pitches warrants this conclusion.

Again, the fact that no apparent deposits of ore are found from the Peters mine around the curve at H and to the St. George mine (No. 4, contour-map) does not militate against this theory. It only

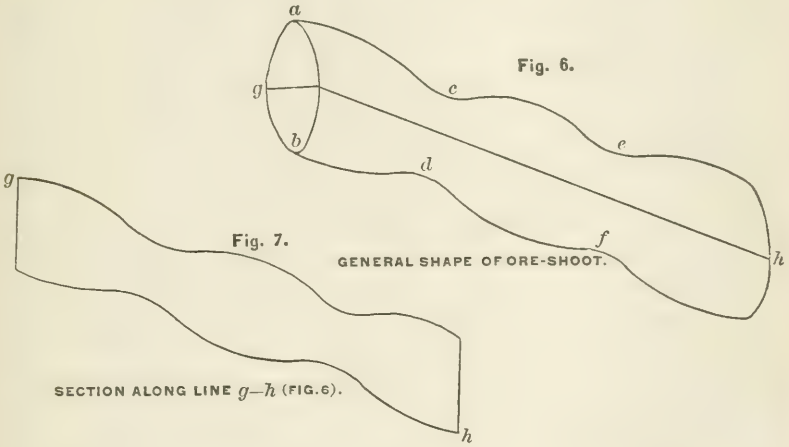


Fig. 6.

GENERAL SHAPE OF ORE-SHOOT.

Fig. 7.

SECTION ALONG LINE g–h (FIG. 6).

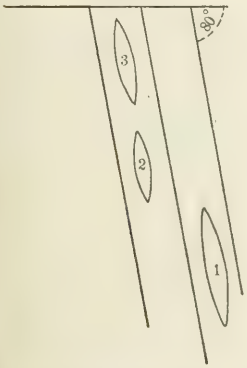


Fig. 8.

VERTICAL CROSS-SECTION IN PETERS MINE, SHOWING POSITION OF ORE-SHOOTS.

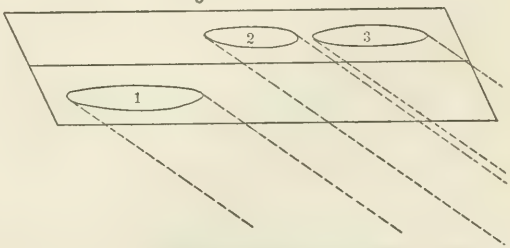


Fig. 9.

OUTCROP SHOWING RELATIVE POSITION OF ORE-SHOOTS AT PETERS MINE,
(Dotted lines show pitch)

forces us to either of two conclusions. First, that there are no deposits between these two points, or, secondly, that such deposits exist but are too deeply buried to manifest themselves either to the eye or by means of a dipping needle.

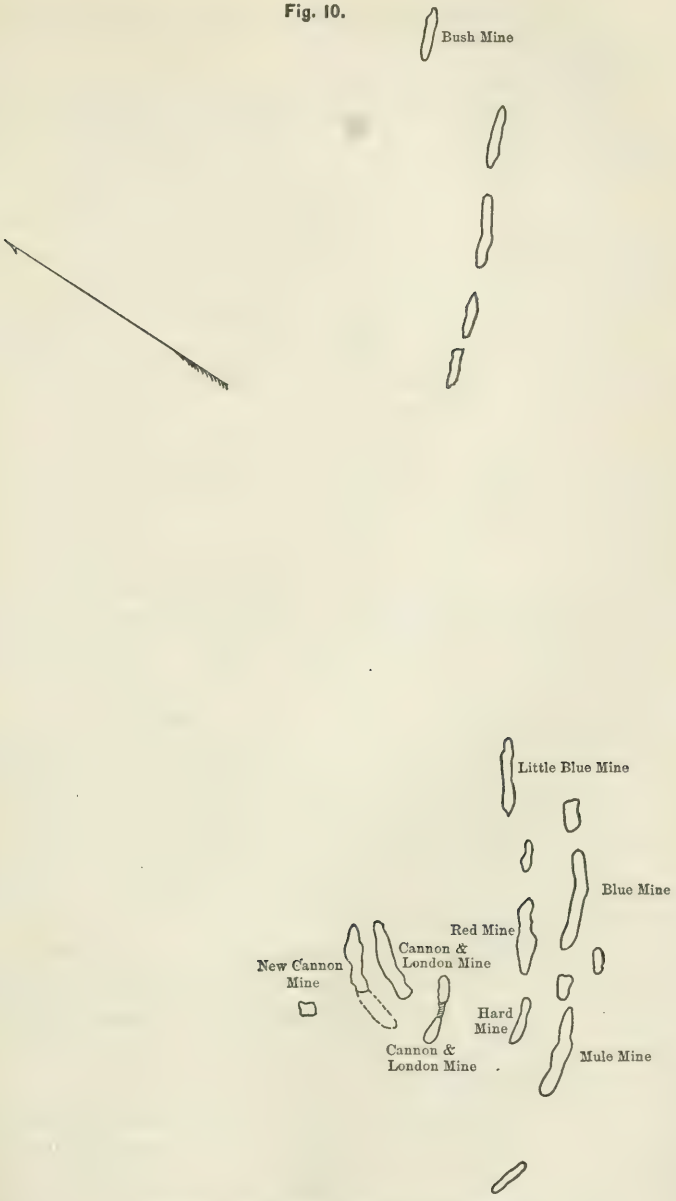
At the Blue Mine group, as has already been stated, the pitch of the ore bodies is 60°. The section-line crosses this group 600 feet from the southern point of the group. On this pitch the mines at the

section-line ought to be at a depth of 1155 feet. At the point of intersection, they come to the surface, or at least there are ore-deposits at the surface. One of two conclusions must certainly be accepted in this case. First, that there are several shoots of ore lying one above the other, as is shown at the Peters mine; or, secondly, that the same shoots of ore are repeatedly brought to the surface by faulting (see Fig. 5). From an *a priori* standpoint the first position is as tenable as the second. Even by analogy there are facts to warrant this conclusion; for it is indisputably true that there are many mines in the State where it is very evident that there are several shoots of ore lying one above the other, as at the Peters mine. Taking all of the facts together, however, the second conclusion seems to be the more tenable. The strongest point in favor of this is the fact that at the Blue mine the pitch is 60° while at the Bush mine, 900 feet distant, the pitch is 43°. Several test-pits, less than 500 feet distant from the Blue mine, show a pitch of about 40°. This sudden and great change in the amount of pitch, taken in connection with known slips or faults in the workings of the older mines (the Blue, Hard, Mule, Cannon, etc.), and the generally disturbed condition of the rocks, makes the second conclusion almost the only admissible one.

Between Whaleback and Hope mountains one would naturally expect to find some evidence of faulting. If faults do exist, however, they are south of the Peters mine, and the evidence is deeply buried beneath glacial drift. There is certainly no evidence of faulting, except on a small scale in the Peters mine. This shoot of ore, as shown in Fig. 4, has been worked for 900 feet on the slope to the point D. Along the eastern slope of Hope mountain, also shown in Fig. 4, there are numerous openings so related to each other as to suggest the possibility that each was but the faulted end of the other, by this means successively brought to the surface. However this may be, there are no signs of such faults, either in the small workings, or in the extensive rock-outcrops on the mountain.

To sum up briefly in regard to the structure of the Ringwood mines, it is shown that the Blue Mine group lies in a tightly compressed synclinal fold, which pitches to the N.E.; that the St. George and Keeler mines lie on the western slope of an anticlinal fold; and that the Peters Mine group lies on the western slope of a syncline. These folds are overthrow-folds so that all of the mines are either vertical or have a S. E. dip. It is farther shown that the Blue Mine group is broken by faults, and that, probably, no such faults exist

Fig. 10.



PLAT OF THE BLUE MINE GROUP.

Showing that the beds of ore do not lie in the same plane or stratum, and indicating the curve or fold platted in Fig. 2.

Scale 1 Inch = 630 Feet.

in the Peters group. The thickness of the iron-bearing strata is upwards of 600 feet.

The practical conclusions which may be drawn from these facts are as follows:

In the first place, if it is true that the anticlinal and synclinal folds reached the vertical elevation shown in Fig. 3, the Blue Mine group must have also suffered erosion in the same proportion as the others. The outcrops of this group show that when denudation ceased, it left surface-outcrops of large dimensions. It would seem tolerably safe to conclude that at least 50 per cent. of these large shoots were swept away.

Secondly, the Blue Mine group is probably very near the bottom of the iron-bearing strata. Add to this the fact that the rocks are badly shattered and faulted; and it is doubtful if any large deposits of ore are left. There may be some comparatively large pocket-like deposits, but none of them would be apt to exist of sufficient size to warrant either extensive prospecting, shaft-sinking, or expensive plants to win the ore.

Thirdly, while the Bush mines, together with the St. George and Keeler, have not the points which make against the Blue Mine group, yet their positions in the folds are such as to make prospecting very difficult and precarious, at least, except within narrow limits, close to the known ore-bodies.

Fourthly, the conditions which make prospecting undesirable at the above-named localities are all reversed in the Peters group. Here the long reach of ore-bearing rock which stretches for over 3300 feet along the eastern foot of Hope mountain seems to give promise of other ore-deposits than the Peters mine, which has been a large producer for so long a time. The steady pitch and dip of the outcropping ore-shoots and the enclosing rocks give exceptional advantages to the engineer for calculating the depth to which either drill-holes or test-shafts would have to be sunk in order to prove any given outcrop. In addition, the chance is by no means small that among these numerous outcrops there may be large bodies of ore which make no surface-showing.

The junction of two or three of these shoots beneath the surface (and there are indications that the Hope mine shoot will thus run into the one south of it; see Fig. 4, Hope mine) would seem almost sure to result in an enlarging of the joined shoots to a workable size.

At least, if business conditions ever resume a more favorable aspect, the chances of success will warrant the outlay for prospecting.

In conclusion, I wish to acknowledge the assistance of Mr. R. K. Painter, who located the mines on the accompanying maps by transit survey. I am also indebted to him for the drawings for this paper, which he made from sketches furnished by the writer.

NOTES ON THE STRUCTURE OF THE FRANKLINITE AND ZINC-ORE BEDS OF SUSSEX COUNTY, NEW JERSEY.

BY WILLIAM P. BLAKE, NEW HAVEN, CONN.

(Bridgeport Meeting, October, 1894.)

In the recent litigation for the possession of franklinite by one party and of zinc-ore by the other party, it was shown by the evidence of experts familiar with the original condition of the outcrops that at Mine Hill a more or less distinctly defined layer of red zinc-ore underlay the heavier and larger body of the franklinite, so-called; this layer or bed of zinc-ore dipping under the franklinite upon the western cropping, while at Stirling Hill, two miles south, the conditions were apparently reversed, the zinc-ore overlying the franklinite.

An explanation being required, it was shown, first, that at both places, at Mine Hill on the north and at Stirling Hill on the south, the ore-bed lies in a synclinal or trough-like fold with the red zinc-ore under the franklinite; and secondly, that at Mine Hill, we see chiefly the outcrop of the western part of the folded bed and at Stirling Hill the eastern part; the Mine Hill cropping being on the western slope, and the Stirling Hill cropping on the eastern slope of the hill. Thus the "back vein" at Stirling (the western outcrop) corresponds with the front-vein at Mine Hill, and, like it, has the red zinc-ore on the foot-wall, while the front-vein or eastern cropping at Stirling corresponds to the back-vein or eastern cropping at Mine Hill in the part of the bed generally known as the "buckwheat-field vein." If the ore-bed were continuous in one synclinal trough between the two localities, no further explanation would be required. But it is not so; and the synclinal folds at both places pitch steeply to the north, bringing the beds to the surface in horseshoe-like curves at their southern ends or croppings, and carrying the beds far below the surface northwards. This justifies a doubt

whether the two localities were ever connected in ore, and raises the question whether or not the deposits were formed under similar conditions in independent basins. The possibility of a series of transverse faults is also suggested by the steep northward pitch of the trough at Stirling, which, for a part of its length, is over 60 degrees. But neither of these hypotheses will so satisfactorily explain the conditions as the assumption that we find in Mine Hill and Stirling Hill the remnants, only, of a much more extended bed of ore, which formerly existed in a series of synclinal and anticlinal folds, and the greater portions of which have been denuded and swept away, leaving only the deeper seated portions of the lowest folds or troughs—the roots, so to speak, of the deposits, as they originally existed when first upraised and plicated.

The plicated or folded structure of these deposits was recognized and announced by me as early as 1858 as follows:

"This last examination [in November, 1858], leads me to conclude that these ores are in vast beds like coal beds, rather than in veins or lodes. They present evidences of plication or folding as in the anthracite coal region. . . . . The ore is evidently metamorphosed, as are the rocks in which it is enclosed. The extent of this metamorphism is such that the minerals appear to have arranged themselves in parallel belts or layers, generally lenticular in form, as in all highly metamorphosed sediments. This fact at once shows the uncertainty of any particular character of ore being continuous from one end of the beds to the other, or up and down in depth. The masses of willemite which we find on Mine Hill, and on Stirling Hill also, have this lenticular character, and thin out in some places to mere sheets. On Stirling Hill, the segregation of minerals is more perfect than on Mine Hill."

In the geological section accompanying the above testimony as to the structure of the ore-bed and the distribution of the ores in lenticular masses, the folded structure is indicated by the synclinal trough nearly as it has been proved to exist by subsequent developments. A short, sharp anticlinal fold is also shown upon the eastern margin of the croppings, near the surface, or where work in the back-vein or eastern outcrop had commenced. It has since been mined away at that place; but the same fold is still visible in the ore-bed at a greater depth upon the pitch, as a highly-compressed or collapsed anticlinal, without any intervening rock, as shown by the drawings in the memoir by Prof. Kemp,†

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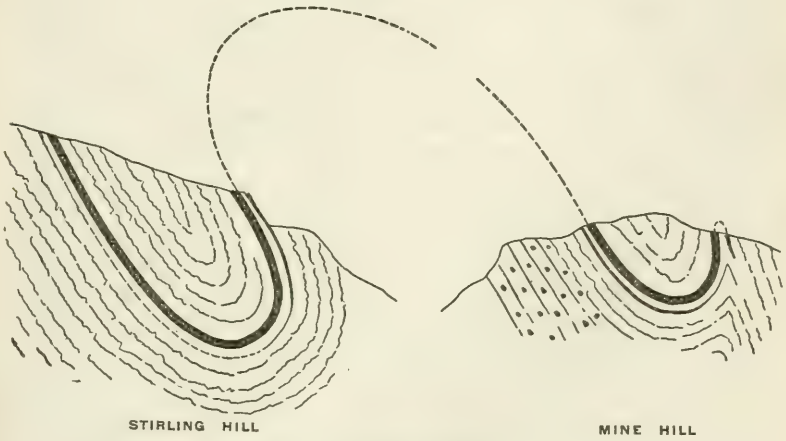
* *The Franklinite Case.* Testimony of W. P. Blake before Washington B. Williams, Master in Chancery. Exhibit No. 4, pages 338-346. December 11, 1858.

† J. F. Kemp: "The Ore Deposits at Franklin Furnace and Ogdensburg, N. J." *Trans. N. Y. Acad. Sci.*, xiii., 76-98, November 20, 1893.

and in the paper by Prof. Nason,* both of which may be consulted to advantage.

In a review of the subject upon the ground in the month of May last, the conditions seemed to indicate the former existence of at least one anticlinal fold between the two localities and their former continuity. A section corresponding to that given in Fig. 1, was placed upon the blackboard before the court, in illustration of the probable former conditions and of the appearance of the red zinc-ore above the franklinite at Stirling Hill and under it at Mine Hill. The section is diagrammatic, not to scale, and projects the section at Stirling and that at Mine Hill upon the same plane, although, as stated, they are widely separated. While there may have been sev-

FIG. 1.



Composite Diagrammatic Section.

eral minor folds between the two places, only one is indicated, for the sake of simplicity in illustration. By following the broken lines it will be seen that the bed of red zinc-ore which overlies the franklinite at Stirling would of necessity underlie the franklinite at Mine Hill.

All the rock formations and ores which formerly existed to an unknown height above the present surface are supposed to have been swept away to the southward by the ice in the glacial period. That there is an enormous quantity of well-rounded boulders of ore in the

* F. L. Nason: "The Franklinite Deposits of Mine Hill, Sussex County, N. J." *Trans.*, xxiv., 12'. See also *American Geologist*, April, 1891.

glacial drift is well known. They are spread for miles southward throughout the valley, and even upon the flanks of the granitic mountains, to a height of 500 to 600 feet. Considerable quantities have been gathered up and shipped away. Shipments are now being made from an immense accumulation of boulders at the southern portion of Stirling Hill. One mass near Sparta, a quarter of a mile from the limestone rocks, and resting upon the granitic rocks, contains, by general estimate, thousands of tons. Considerable quantities have been blasted from it and taken away. We may be sure that for every transported boulder of ore found on the surface there are hundreds buried from view in the moraines and the alluvions of the valley.

In the section the representation of the intrusive crystalline rocks has been purposely omitted. The rocks shown are the crystalline white limestones, both above and below the ore-bed, and in addition, an indication of the "syenite," so-called, upon the western slope of Mine Hill. This rock is gneissic but very hard and compact, and is doubtless a highly metamorphosed, mechanically-formed rock, Archæan in age, and conformable with the limestones, which are, apparently, of the same ancient age. It is important to note, that in the open cut upon the Passaic Company's property at Stirling Hill there is an outcropping ridge of gneissic rock, the relations of which to the ore-bed are not clearly made out, but which appears to be to the eastward of the syncline shown in the section. It is lithologically identical with the syenitic gneiss of Mine Hill.

The bed of magnetite underlying the franklinite and zinc-ore of Mine Hill has been shown to be conformably folded. Its cropping extends nearly half a mile south of the south-end of the franklinite syncline. There are indications of an outcrop of magnetite towards the southwest end of the Stirling Hill syncline, but it is not certain whether the croppings are in place or are merely boulders, as no excavations have been made. The bed of magnetite is not represented on the section, but its presence at Mine Hill, nearly on the plane of contact between the white crystalline limestone and the gneissic rock, and its plication with the limestone, favor the belief of the complete conformity of the formations and their identity in age. It is not, however, the purpose of this present paper to discuss that question, which is reserved for another communication.

ALUMINUM-BRONZE.

BY LEONARD WALDO, BRIDGEPORT, CONN.

(Virginia Beach Meeting, February, 1894).

PROBABLY some of the views advanced in this paper will appear, from a metallurgical standpoint, little less than revolutionary. It is with considerable hesitancy that I venture to offer a few thoughts concerning what seems to me to be perhaps the most important development of metallurgy since the introduction of Bessemer steel.

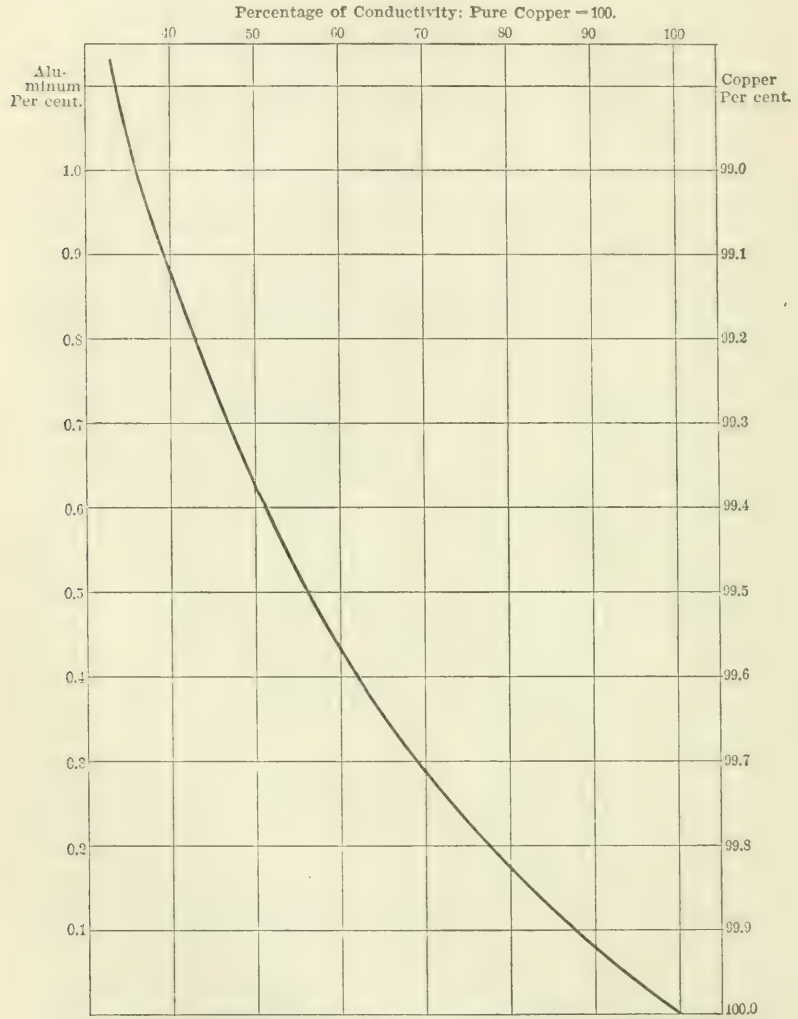
Dr. John Percy, with that rare insight which characterizes the epoch-making man, described in a communication to the Royal Society, about 1855, an alloy of aluminum and copper which he called aluminum-bronze. The name has "stuck." The term "bronze" has been so variously used that I confess I do not know exactly what constitutes a bronze; but I know that in the great materials of engineering simplicity of composition is a cardinal excellence; and I wish to venture the suggestion that aluminum-bronze is not an alloy of aluminum and copper in any such sense as the compounds of tin and zinc with copper (except the copper-tin compound $SnCu_3$) are alloys. There are several reasons which lead me to this view.

In the first place, if a series of bars be prepared containing varying percentages of aluminum and copper, and the electrical conductivity of these bars be carefully determined, a curve of conductivity will be obtained, as shown in Fig. 1, which represents the result of such experiments. While I am not sure, at this time, that the values of conductivity shown in this diagram are wholly dependent upon the percentage of aluminum added, the assumption is a probable one; and the curve indicates that an extremely small percentage of aluminum affects the conductivity in a ratio out of all proportion to the amount of that metal.

Another reason is drawn directly from experience in the foundry. If we take from the furnace a crucible containing melted aluminum and one containing melted copper, both at a little more than a red heat, and pour them together, the reaction is so intense that the

metals are brought to a state of ebullition, the crucible becomes white hot from it, and you have evidence of intense chemical activity.

Fig. I.



CURVE SHOWING THE EFFECT OF SMALL PERCENTAGES OF ALUMINUM ON THE ELECTRICAL CONDUCTIVITY OF COPPER.

A third reason which I have for thinking that chemical union has taken place between the aluminum and the copper is that there is no known simple method of separating the aluminum from the copper when they have been thus combined. If you take a large billet of copper to which less than ten per cent. of aluminum has been added,

you can find no evidence whatever of the segregation of distinct granules of aluminum in copper, or of any uncertain composition, from one end of the ingot to the other.

There are reasons for supposing that the basal combination which is formed would correspond to the formula $AlCu_4$. That is what is commonly called ten per cent. bronze. My belief is that it is an aluminide of copper and that its solution in copper gives us the various qualities of aluminum-bronze. The little specimens in the form of souvenir spoons distributed at this meeting represent this formula diluted with an equal amount of copper. To remedy some of the existing confusion in regard to the alloys of aluminum and copper (I use that word alloy with a reservation, and only because this is the common nomenclature) I venture to propose that we consider this $AlCu_4$ as aluminum-bronze; that when it is diluted with an equal amount of copper we call it one-half aluminum-bronze; and so on to one-quarter bronze, etc. In a little circular explanatory of the specimens of aluminum-bronze exhibited in connection with this paper, I have ventured to use the names, one-quarter, one-half, three-quarters and full aluminum-bronze. It is a common foundryman's idea that all the aluminum which can be taken up by copper is not in excess of three per cent., and it is quite important that the nomenclature be clear and established at the beginning, because I see no limit to which the use of aluminum-bronze is not to go in the future.

As to the qualities of the compound which is formed in this way, it is at once evident, in view of the high melting-points of aluminum and copper, that if we have a true aluminide of copper formed we have got a set of conditions, respecting corrosion, which does not exist in any alloy of copper. The combinations between copper and tin and zinc, and the very little understood relations which exist in manganese-bronze and that class of compounds, are very different from the action between aluminum and copper, and are not chemical unions at all; but in the case of aluminum-bronze you have a definite compound analogous in its mechanical qualities to a high-grade steel.

I take it that the steel people have virtually agreed that steel is a carbide of iron, and it is only a question of a solution of this in larger masses of iron. In an analogous way the aluminum-bronze is an aluminide of copper, deriving various properties from different degrees of its solution in the molten copper. This solution seems to be a very perfect solution. It is somewhat affected by the per-

centages of impurity in the copper. Aluminum is one of the most powerful deoxidizing agents known, and it is said that no oxygen can exist in the presence of molten aluminum. I think that must be modified to the statement that none can exist at very high temperatures. When the aluminum-bronze has been formed and when the chemical union has been effected between the aluminum and the copper, and in the absence of disturbing elements, we have a metal showing the fracture-qualities of the highest grades of steel, and equalling steel in elongation, elastic limit, and tensile strength of the qualities of steel, and possessing also a different class of properties. This series of metals are non-magnetic. They are very permanent. The oftener they are remelted the better they are, providing they are kept free from oxygen, iron and silicon. The value of the material is just as great after the tool has outlived its usefulness as it was at the beginning, and for all cases in which magnetic properties, or high conductivity, or extremely uniform and permanent behavior are required you have the ideal metal.

One of the chief reasons why steel and iron hold their high value is the fact that they are commercially workable and the metal does not lose its mechanical properties throughout the range of the temperatures of ordinary use. Steel that is twice as hot as boiling water, for instance, is still excellent steel. That is not true of any of the alloys of copper made with tin or zinc; but it is true of aluminum-bronze.

We have been working in a quiet sort of a way, and rather on a toy scale as compared with the great steel plants of the country; but we have been trying to accumulate a little knowledge in our foundry work. We saw from the start that the fundamental question was the question of the ingot. There were plenty of rolling-mills in the country where the ingots could be worked if furnished in uniform quality. I exhibit here some of the results of our work in this direction. We have been filling a number of specifications for the United States Engineer Department. The particular pieces, exhibited with this paper, are the hold-down bolts for the plates of mortar-batteries along the coast. This case requires a submerged bolt to take a strain of 200,000 pounds per bolt, to remain years in place, and yet to afford a guaranty that at the end of years and after any number of firing-shocks that bolt shall be unchanged. I do not know of a severer test than that. Large forgings (hot) are accomplished in the same material.

It is also interesting that we have been highly successful in the

preparation of metal which will far surpass the present requirements for cartridge-shells. I mention the cartridge-shells because, as is well known, a cartridge is made from a flat disk, spun and swedged, and thus gives, perhaps, the severest test that we have of the complete working-qualities of sheet-metal.

The difficulties in casting aluminum bronze are very great. I believe them to be more serious and perhaps less understood than those encountered in the casting of any known metal. The large contraction which takes place in the actual formation of the pattern-casting and the so far unexplained and unknown chemical actions which occur, have called for ingenious and complicated devices for obtaining sound castings. So far we have hesitated about undertaking castings weighing over two tons, and, of course, those are small castings in comparison with the work done in steel-works at present.

The aluminum seems to have a wonderful capacity for occluding hydrogen-gas, and the liberation of this gas at the moment of the union of the aluminum with the copper, and the unknown relation which hydride of copper may sustain to the oxygen which exists in the pores, present difficulties which have still to be thoroughly studied. With regard to some of them, I have to pay the compliment to Mr. Howe's work on steel, that it is the best work on aluminum-bronze which we have yet seen.

In this manufacture the purity of both aluminum and copper is of the utmost consequence. Those small percentages which almost defy the reagents of the analytical chemist become extremely important when one is working in the dark in regard to aluminum-bronze. In making up our crucible-charges we cannot depend upon the chemical formulæ only because we must take careful account of the loss and the destructive combinations which take place between aluminum and silicon and iron.

In a perfect ingot or casting the solution of aluminum-bronze and copper seems to be perfect. The analysis from the top, bottom and sides shows no variation due to segregation. Of course, the mechanical mixture must have been made complete, so as to secure both the chemical combination and the uniform solution of it in excess of copper.

THE INACCURACY OF THE COMMERCIAL ASSAY FOR SILVER, AND OF METALLURGICAL STATISTICS IN SILVER-MILLS, WITH SPECIAL REFERENCE TO THE TREATMENT OF ROASTED ORES BY AMALGAMATION AND BY THE RUSSELL PROCESS.

BY C. A. STETEFELDT, OAKLAND, CAL.

(Bridgeport Meeting, October, 1894.)

THE purpose of this paper is to convince the managers of silver-mills that their customary reports in regard to the percentage of silver saved are far from the truth. It may be unpleasant for them to have the proofs of this fact brought before the public, and for this reason the author refrains from giving the names of the mines and mills from which the statistics here recorded were obtained. It may be asked: What is gained by informing the stockholders of our silver-mines that they receive incorrect reports? Will this increase their dividends or lessen their assessments? In the first place, it is always better to publish honest facts than fiction. In the second place, many superintendents believe in the correctness of their reports, and have no idea how much they deceive themselves. Finally, the knowledge that hitherto unexpected losses of silver take place may lead to the adoption of methods by which these losses are greatly reduced. These remarks refer also to smelting-works. The present paper, however, is concerned exclusively with amalgamation and lixiviation.

The first and most important error is found in the inaccuracy of the commercial assay; the second in the incorrect sampling of tailings in amalgamation. Further, mills not doing custom-work, but reducing exclusively ores from mines to which the mills belong, are in the habit of weighing the ore only approximately and without making moisture-tests. Thus, a correct basis for calculating the actual percentage of silver extracted is lacking. The superintendent, then, finds it convenient to report the apparent extraction, *i.e.*, the extraction indicated by comparing the value of the roasted ore, minus soluble salts, with the value of the tailings. How many errors are thereby committed it is not necessary to point out.

Under "commercial assay," we understand the direct result of either a crucible- or a scorification-assay for silver. The losses taking place in the assay are caused by: (1) Silver remaining in the slag. (2) Silver absorbed by the cupel. (3) Silver volatilized in the crucible-melting or scorification and in cupellation. The first two losses can be ascertained with practical accuracy by re-assaying the slag and the cupel. By adding the silver thus obtained to the result of the commercial assay, we obtain the *corrected assay*. The loss by volatilization for ores and products like sulphides, it is not practicable to ascertain, even by synthetic methods; such as are used, for example, to determine the fineness of high-grade silver-bullion by a check-assay. Hence, this loss is an unknown quantity; but we are justified in assuming that it is comparatively slight, so far as most ores are concerned which do not exceed 50 ounces of silver per ton in value, higher grades going by preference, with few exceptions, to smelting-works and not to mills.

I.—Inaccuracy of the Commercial Assay.

In the following pages, numerous statistics will be given, exhibiting the difference between commercial and corrected assays. The author does not intend to criticize here different methods of assaying with respect to the composition of fluxes, etc. It is assumed that each assayer employed in the mills to which reference is made knew by experience which composition of flux was, in a general way, best adapted to the character of the ore. A few remarks, however, will be made regarding the merits of crucible- and scorification-assays.

In older treatises on assaying, decided preference is given in *all cases* to scorification; recent text-books, however, acknowledge in many instances the superiority of the crucible-assay. Yet such an authority as Professor H. O. Hofman says in his *Metallurgy of Lead* (p. 70):

> "Silver assays are uniformly made by scorification, as it is adapted to all kinds of ores and gives excellent results unless organic matter is present."

This is misleading in two ways: (1) Scorification-assays are not uniformly made; I do not know of any silver-mill where such a practice is in vogue for regular work. (2) For all so-called dry silver-ores, containing antimonial and arsenical sulphides or their products of decomposition, the crucible-assay deserves the preference. I may cite in support of this assertion the experience of Al-

bert Arents,* whose statement is of importance because he has been a government-assayer at Clausthal, Germany, where scorification is exclusively used, and hence cannot be charged with prejudice or with unskilful execution of the method. My own experience in assaying ores from Reese River and other mining-districts of Nevada (of which I have not kept a detailed record) was in harmony with that of Mr. Arents. In addition, a comparison between the two methods is given in Table I.

TABLE I.—*Crucible and Scorification Silver-Assays at Mills A and B.*

Description of Ore.	Method of Assaying.	Commercial Assay. Ounces per Ton.	Corrected Assay. Ounces per Ton.
Raw Ore, No. 1.....	Scorification.	33.0	35.0
	Crucible.	34.6	37.1
Raw Ore, No. 2.....	Scorification.	16.5	18.4
	Crucible.	19.0	21.4
Raw Ore, No. 3.....	Scorification.	18.0	19.6
	Crucible.	22.6	24.5
Mixture of Raw Ore, Nos. 1, 2 and 3.....	Scorification.	32.3	37.6
	Crucible.	32.4	37.8
Mixture of Roasted Ore, Nos. 1, 2 and 3.....	Scorification.	23.1	28.3
	Crucible.	26.0	30.1

NOTE.—Ore No. 1 is very base, containing about 75 per cent. of sulphurets. Ore No. 2 is entirely oxidized and ore No. 3 is intermediate between Nos. 1 and 2. The figures given do not refer to single assays, but for raw ores, No. 1, No. 2 and No. 3, to averages of daily assays for several months, and for the mixtures of Nos. 1, 2 and 3 of raw and roasted ore, to averages for one week.

Granting even that the assayers at mills A and B did not possess the skill of Mr. Arents in making scorification-assays, the differences are too great to be explained on that ground alone. Taking the averages of these assays, it appears that the commercial crucible-assay is 2.3 ounces or 9.3 per cent. higher than the corresponding scorification-assay; and the corrected crucible-assay is 2.4 ounces or 8 6 per cent. higher than the corresponding scorification-assay. Comparing the commercial with the corrected assay, the latter is 3.2 ounces or

* *Berg. und Hüttenmännische Zeitung*, 1867, p. 85.

13.0 per cent. higher in scorification, and 3.3 ounces or 12.2 per cent. higher in the crucible-assay.

Mr. F. A. Bird, a highly skilled assayer at the Anchor mine, Park City, Utah, finds that for ores carrying silver principally in galena, the commercial scorification-assay gives higher results than the best method in crucibles. In corrected assays, however, the opposite result is again obtained, as shown in Table II. So far as the commercial assay is concerned, this is not a new fact for silver-ores rich in galena and other lead-minerals.

TABLE II.—*Scorification and Crucible Assays of Lead-Ores.*

Method of Assaying.	Ounces of Silver per Ton.		Character of Ore.
	Commercial Assay.	Corrected Assay.	
Scorification.	13.80	13.80	Containing 18.5 per cent. Pb and 28.8 per cent. Zn.
Crucible.	12.20	13.98	
Scorification.	29.40	29.97	Containing 46.3 per cent. Pb and 15.3 per cent. Zn.
Crucible.	28.92	30.46	
Scorification.	13.00	13.50	Containing 14.6 per cent. Pb and 34.7 per cent. Zn.
Crucible.	12.80	13.60	

The products from lixiviation—regular sulphides, base sulphides from wash-water, and carbonates—are usually assayed by scorification, and the results are higher than by crucible-assay. For further information on this subject, see *Trans.*, xvi., 378.

In the following tables numerous statistics are given to illustrate the difference between commercial and corrected assays of ores, tailings, and products from lixiviations. The different ores are designated by numbers (1, 2, etc.); the mills by letters (A, B, etc.); the names of assayers also by letters (M, N, etc.). In these tables all assays refer not to single samples, but to averages of many samples, and these averages are arranged under heads showing the limits between which the values varied. In this way personal errors in making single assays are eliminated. This arrangement also enables us to see at a glance the relation existing between the values of ores, etc., and the degree of incorrectness of the commercial assay. All assays of ores and tailings were made in crucibles, and those of lixiviation-products by scorification.

Some samples of ores and tailings, belonging to special experi-

TABLE III.—*Commercial and Corrected Assays of Raw and Roasted Ores and Tailings; Mills A and B, Assayers M and N.*

Description of Material Assayed.	Grade of Value. Ounces.	Average Value Per Ton. Commer. Assay. Ounces.	In Cupel. Ounces.	In Slag. Ounces.	Corrected Assay. Ounces.	Commercial Assay lower than Corrected Assay. Per ct.
Raw Ore, No 1.....	20 to 25	24.1	5.4	0.5	30.0	19.7
	25 to 35	30.7	3.3	0.6	34.6	11.3
	35 to 40	37.4	3.5	0.8	41.7	10.4
	40 to 45	44.7	3.9	0.4	49.0	8.8
Raw Ore, No. 3.....	25 to 30	27.0	3.6	0.4	31.0	13.0
	30 to 35	31.6	3.0	0.4	35.0	10.0
Raw Ore, Nos. 1, 2 and 3	20 to 25	24.3	2.6	0.7	27.6	12.0
	25 to 30	28.4	2.2	0.8	31.4	9.6
	30 to 35	31.9	3.1	0.8	35.8	10.9
Roasted Ore, Nos. 1, 2 and 3.....	20 to 25	23.1	1.4	0.6	25.1	8.0
	25 to 30	26.6	2.1	0.4	29.1	8.6
	30 to 35	30.4	3.0	0.6	34.0	10.6
Roasted Ore, Nos. 1, 2 and 3, after leaching with water.....	15 to 20	17.6	1.6	0.2	19.4	9.0
	20 to 25	23.8	3.4	1.4	28.6	16.8
	25 to 30	27.2	2.0	0.9	30.1	9.7
	30 to 35	32.3	3.0	0.7	36.0	10.3
Tailings, Nos. 1, 2 and 3	5 to 8	6.2	0.9	0.3	7.4	16.3
	8 to 11	9.7	1.2	0.4	11.3	14.2
	11 to 20	17.5	1.3	0.3	19.1	8.4

NOTE.—The grade of value in this and the following tables is taken on the basis of the commercial assay. Ore No. 1 is very base, containing as much as 75 per cent. of sulphurets. Ore No. 2 is completely oxidized, and No. 3 is intermediate between No. 1 and No. 2. The mixture of Nos. 1, 2 and 3 is somewhat variable, and this accounts for the greater irregularity of the assays.

ments made in mills for comparing the extraction of silver by amalgamation and lixiviation, were submitted for re-assaying to a very skilful and painstaking assayer. By trying various compositions of fluxes, best adapted to the character of the ore, and by careful manipulations, he succeeded in obtaining corrected assays several per cent. higher than those reported by the regular assayers of the mills. In the metallurgical statistics, which will be given in another section, I shall refrain, however, from making use of these higher corrected assays, not because I consider them unreliable, but for practical reasons. The assayers in our silver-mills (unlike those in smelting-

TABLE IV.—*Commercial and Corrected Assays of Raw and Roasted Ore and Tailings. Mill C, Assayer O, Ore No. 4.*

Description of Material Assayed.	Grade of Value. Ounces.	Average Value per ton. Commercial Assay. Ounces.	In Slag and Cupel. Ounces.	Corrected Assay. Ounces.	Commercial Assay lower than Corrected Assay. Per cent.
Raw Ore.....	35 to 40	37.65	2.25	39.9	5.6
	40 to 45	42.3	2.3	44.6	5.2
	45 to 50	45.5	2.4	47.9	5.0
Roasted Ore.....	30 to 35	33.6	3.2	36.8	8.7
	35 to 40	36.9	3.3	40.2	8.2
	40 to 45	42.1	3.0	45.1	6.7
Tailings.....	1 to 3	1.8	0.5	2.3	21.8
	3 to 5	3.9	1.0	4.9	20.5
	5 to 7	6.0	1.3	7.3	17.8
	7 to 9	7.5	1.4	8.9	15.7
	9 to 11	9.6	1.6	11.2	14.3
	11 to 13	12.2	1.8	14.0	13.0

NOTE.—Ore No. 4 is a dry silver-ore, the principal silver-mineral being fahlore or its products of decomposition. The ore is more or less base, and variable in that respect. The assays refer to samples taken for one year, and they were made on monthly averages of daily samples.

works) are generally not men of scientific education; their monthly salaries rarely equal and never exceed the wages of the most skilled laborers in the mill; the amount of work they have to perform is already onerous, and if the superintendent should introduce the corrected assay, the assayer's work would be doubled so far as the assaying of ores and tailings is concerned. Moreover, the character of the ore treated changes frequently. We can scarcely expect that the assayer should constantly change his fluxes, or re-assay the same sample several times, or that he should be constantly on the lookout to cupel his lead-buttons with the formation of feather-litharge. Sufficient practical accuracy will be secured by making what we have called corrected assays obligatory, but without attempting to obtain the highest result possible. Besides, a slight inaccuracy in the assay is not the only or the worst source of error in the metallurgical statistics. The sample itself may not represent the true average; and then, there are the weights, moisture-tests, etc. How difficult it is, for instance, to obtain a correct tailings-sample, will be shown later on.

TABLE V.—*Commercial and Corrected Assays of Raw and Roasted Ore and Tailings. Mill D, Assayer O, Ore No.* 5.

Description of Material Assayed.	Grade of Value. Ounces.	Average Value per ton. Commercial Assay. Ounces.	In Slag and Cupel. Ounces.	Corrected Assay. Ounces.	Commercial Assay lower than Corrected Assay. Per cent.
Raw Ore.....	20 to 25	22.4	1.9	24.3	7.8
	25 to 30	29.7	2.8	32.5	8.6
	30 to 35	33.1	2.8	35.9	7.8
	35 to 40	38.2	2.8	41.0	6.8
Roasted Ore.....	30 to 35	31.8	2.3	34.1	7.0
	35 to 40	36.7	3.1	39.8	7.8
	40 to 45	42.4	2.4	44.8	5.4
Tailings.....	1 to 3	2.8	0.4	3.2	12.5
	3 to 5	3.0	0.6	3.6	16.7
	5 to 7	5.5	0.6	6.1	10.0
	7 to 10	9.8	1.3	11.1	11.8

NOTE.—Ore No. 5 is similar in composition to Ore No. 4, coming from the same vein; but it is more oxidized.

TABLE VI.—*Commercial and Corrected Assays of Raw Ore and Tailings. Mill I, Assayer U, Ore No.* 13.

Description of Material Assayed.	Grade of Value. Ounces.	Average Value per Ton. Commer. Assay. Ounces.	In Slag and Cupel. Ounces.	Corrected Assay. Ounces.	Commercial Assay lower than Corrected Assay. Per cent.
Ore.....	10 to 15	12.5	1.3	13.8	9.5
	25 to 30	28.9	1.6	30.5	5.3
Tailings.....	3 to 5	4.0	0.5	4.5	11.2
	7 to 9	8.3	0.9	9.2	9.8

NOTE.—I is a concentrating mill treating ores averaging 18 per cent in lead and 28 per cent. in zinc.

These tables will be sufficient to illustrate the incorrectness of the commercial assay for ores and tailings. We now turn to lixiviation-products.

In Table VII. a summary of average results is given compiled from the statistics of four mills.

TABLE VII.—*Commercial and Corrected Assays of Lead Carbonates, Base Sulphides from Wash-Water and Regular Sulphides.*

Description of Material Assayed.	Grade of Value. Ounces.	Average Value per Ton. Commer. Assay. Ounces.	In Slag and Cupel. Ounces.	Corrected Assay. Ounces.	Commercial Assay lower than Corrected Assay. Per cent.
Carbonates.....	50 to 100	64.8	3.2	68	4.8
	100 to 500	447.6	12.4	460	2.9
Base Sulphides.....	500 to 1000	766	41	807	5.1
	1000 to 1500	1227	36	1263	2.9
	2000 to 3000	2527	88	2615	3.4
	3000 to 4000	3289	113	3402	3.4
	4000 to 5000	4962	116	5078	2.2
Regular Sulphides...	1000 to 2000	1624	83	1707	4.9
	2000 to 4000	2807	60	2867	2.1
	4000 to 6000	5291	98	5389	1.8
	6000 to 8000	7362	110	7472	1.5
	8000 to 10,000	8604	128	8732	1.5
	10,000 to 12,000	10,538	150	10,688	1.5
	12,000 to 14,000	12,842	218	13,060	1.7
	14,000 to 16,000	14,866	180	15,046	1.2

Finally, Table VIII. contains a summary of Tables III., IV., V., VI. and VII. The reader must have noted already that for groups of different materials of approximately uniform character the percentage of inaccuracy of the commercial assay decreases with the increased value of the material assayed. If this law does not always hold good with perfect uniformity, it must be considered that the samples assayed and averaged in the tables were more or less variable in their composition.

TABLE VIII.—*Summary of the Differences Between Commercial and Corrected Assays of Ores, Tailings and Lixiviation-Products.*

Description of Material Assayed.	Grade of Value. Ounces per Ton.	Range of Inaccuracy. Per cent.
Tailings.....	1 to 20	21.8 to 8.4
Raw Ore.....	20 to 50	19.7 to 5.0
Washed Roasted Ore.....	15 to 35	16.8 to 9.0
Roasted Ore.....	20 to 45	10.6 to 5.4
Lead Carbonates.....	50 to 500	4.8 to 2.9
Base Sulphides.....	500 to 5000	5.1 to 2.2
Regular Sulphides.....	1000 to 16,000	4.9 to 1.2

I conclude this section with a few remarks about gold. Silver-ores treated by amalgamation or lixiviation often contain small quantities of gold. The gold-value of the raw or roasted ore, being too small for correct determination, is generally not reported in the daily samples. In the bullion or sulphide produced, however, the gold may form a considerable part of the value. Thus, for example, the base sulphides from the Marsac mill contain from 1.6 to 3.9 ounces and the regular sulphides from 11.1 to 12.1 ounces of gold per ton. If a sample of gold-bearing silver-ore is assayed for gold by different persons, their reports rarely agree. This is largely due to the character of the sample, and partly to the methods employed. In assaying steam-dried unroasted sulphides, however, which are more uniform in character than a sample of ore, widely differing results are more largely due to the method used.*

Mason and Bowman† have found by synthetic experiments that the loss of gold and silver in scorification is about the same; but that in cupellation the loss of silver is 6.7 times as great as it is for gold. They state the average losses in numerous scorifications and cupellations as follows:‡

	Silver. Per cent.	Gold. Per cent.
Average loss by scorification,	0.55	0.574
Average loss by cupellation,	1.99	0.296
Total,	2.54	0.870

II.—The Difficulty of Obtaining Correct Average Samples of Tailings in Amalgamation.

To obtain a true average sample of tailings in lixiviation is comparatively an easy matter. At the Marsac mill, for instance, four different samples are taken: The first is taken, before the charge is sluiced out, by means of a long brass tube, $1\frac{1}{2}$ inches in diameter, run vertically down through the charge in various places from top to bottom, whereby cores of tailings are obtained. The average of these cores constitutes the average tube-sample. After one-half of the charge has been sluiced out, leaving a vertical face across the

* On the greater discrepancy in assays of sulphides for gold than for silver, see the author's paper, "Product and Economical Results of the Marsac Refinery for the Year 1892."—*Trans.*, xxiv., 221.

† *Journ. of the Am. Chem. Soc.*, May, 1894, p. 313.

‡ It would have been of interest if in their experiments the silver and gold absorbed by the slag and cupel had been determined, thus arriving at the loss by volatilization.

center of the tank, the sampler gets into the tank and takes three samples as follows: (1) across the face about 1 foot below the horizontal surface of the tailings; (2) across the face about 1 foot from the bottom; (3) an average sample, consisting of one set across the top, one across the bottom, and one across the middle of the face, this mixture being called the average regular sample. Nos. 1 and 2 are merely for the information of the men in charge of lixiviation; their work being considered well done if the assay of the bottom tailings does not differ materially from that of the top. The assays of the average tube-sample, and of the average regular sample are reported in the metallurgical statistics of the mill. The average of these two samples for nearly two years, showed a difference of only 0.03 of an ounce of silver per ton.

Formerly, a dipper-sample was taken from the tail-race while sluicing out the tailings from the tank. This practice proved to be unreliable for two reasons: first, a true average could not be obtained, because the sluicing is very rapid in the beginning when the tank is full, and very slow at the end of the operation; and secondly, the slimes are liable to be partly lost in taking dipper-samples. In proceeding as first described above, the natural proportion of slimes and coarse material is preserved. The whole sample is then dried and quartered down.

In amalgamation the only feasible practice is to take dipper-samples from the settler. It is not necessary to describe this in detail. It is evident, however, that the method is liable to be incorrect from a mechanical standpoint. Another error is introduced by the practice, in vogue in most mills, of pouring off the water after the tailings-samples have settled in a bucket or pan. The possible loss of fine slimes thus occasioned may be neglected (provided sufficient time was allowed for settling); but there is something else present, namely, finely divided silver chloride, which does not settle easily; or there may be silver in solution. In pan-amalgamation of roasted ores the hot, concentrated brine dissolves silver chloride, which is not completely reduced by contact with iron. When the pan-charge is diluted with cold water in the settler this silver is mostly precipitated. But in fact, it makes no difference whether this precipitation is complete; this silver is lost when the water is poured off from the tailings-sample. How much silver may be lost in this way, has not been accurately determined. The above-named loss could only be avoided by evaporating the whole sample, tailings and liquid, to dryness.

Attention was first drawn to the unreliability of the customary tailings-samples in amalgamation by the following observation:

In making comparative clean-ups on the same ore by amalgamation and lixiviation, it has been found frequently that the actual saving in lixiviation corresponded much closer to the apparent extraction than was the case in amalgamation, *i.e.*, in the latter case the percentage of silver actually saved was much less than that expected according to apparent extraction, using corrected assays as a basis for calculation.

III.—The So-called Plus Clean-up in Amalgamation and Lixiviation when Statistics are Based upon Commercial Assays.

All statistics in this section are based upon the values of roasted and not of raw ores. The great difference between commercial and corrected assays of ores has been pointed out above. If the former assays are used in a mill, in calculating the percentage of silver saved by an actual clean-up (which is the practice now in vogue) the phenomenon of the so-called "*plus* clean-up" appears, *i.e.*, the silver obtained in bullion or lixiviation-products (the values of which are usually determined by corrected assays) is largely in excess of the amount expected, taking into account the silver remaining in the tailings; or, in other words, the actual extraction is much greater than the apparent extraction. This is illustrated in Tables IX. and X.:

Table IX.—*The Plus Clean-up in Amalgamation.*

Description of Ore.	Mill.	Apparent Extraction. Per cent.	Actual Extraction. Per cent.	Plus Clean-up. Per cent.
No. 4.....	C	66.1	73.9	7.8
No. 5.....	D	91.0	104.1	13.1
"	"	89.1	95.5	6.4
"	"	86.7	92.9	6.2
No. 9.....	H	86.3	88.9	2.6

Note.—The character of ores No. 4 and No. 5 has already been given in connection with Tables IV. and V. Ore No. 9 is more or less base and contains lime. Mills C and D crush by stamps and roast in Stetefeldt furnaces. Mill H crushed by stamps and roasted in Howell furnaces. The unusually low extraction in Mill C was caused by low chlorinations in roasting, the ore being entirely oxidized and without sulphurets. This was subsequently remedied by adding a small percentage of sulphur to the pulp before roasting.

TABLE X.—*The Plus Clean-Up in Mills Using the Russell Process.*

Description of Ore.	Mill.	Apparent Extraction. Per cent.	Actual Extraction. Per cent.	Plus Clean-up. Per cent.
No. 8.....	H	87.5	99.0	11.5
No. 9.....	"	89.4	101.1	11.7
Eight clean-ups on No. 5..	D	90.6 to 94.3	94.0 to 102.7	3.4 to 10.6
Two clean-ups on Nos. 1, 2 and 3.....	A	80.0 to 81.3	89.9 to 91.7	9.9 to 10.3
Ten clean-ups on No. 7.....	E	85.8 to 90.3	93.9 to 99.6	6.3 to 10.0

NOTE.—Ore No. 7 is a very dry silver-ore with lime, magnesia, and heavy-spar in the gangue. It is crushed in Mill E by stamps and roasted in a Stetefeldt furnace with addition of iron pyrites. Mill A crushed by rolls and roasted in a Stetefeldt furnace. The silver-values per ton of the ores were as follows: For No. 8, 43 ounces; No. 9, 48 ounces; No. 5, 30 to 37 ounces; mixture of Nos. 1, 2 and 3, 23 ounces; No. 7, 21 to 28 ounces.

In comparing Tables IX. and X., it will be noted that, on the whole, the plus clean-up is greater in lixiviation than in amalgamation. This is due to the incorrectness of the tailings-sample in amalgamation, which is liable to be too low, as pointed out in the preceding section.

IV.—COMPARISON OF THE APPARENT AND THE ACTUAL EXTRACTION OF SILVER, BASED UPON COMMERCIAL AND CORRECTED ASSAYS.

TABLE XI.—*Statistics of the Apparent and Actual Extraction of Silver by the Russell Process, based upon Commercial and Corrected Assays.*

Value of Roasted Ore.		Value of Tailings.		Apparent Extraction.		Actual Extraction.	
Com. A. Ounces.	Corr. A. Ounces.	Com. A. Ounces.	Corr. A. Ounces.	Com. A. Per cent.	Corr. A. Per cent.	Com. A. Per cent.	Corr. A. Per cent.
30.34	32.2	2.6	3.0	92.2	91.4	100.1	94.9
37.05	40.2	3.0	3.6	92.4	91.6	100.7	92.8
36.47	39.4	2.8	3.4	92.9	91.8	100.0	92.6
34.44	37.19	2.95	3.6	92.1	91.1	102.7	95.1
33.18	35.6	3.4	4.2	90.6	88.9	94.0	87.1
34.23	36.8	2.1	2.4	94.3	93.4	99.8	92.8
37.03	39.43	2.75	3.4	93.2	92.1	100.6	94.5
34.66	37.60	3.1	3.6	91.7	91.1	99.4	91.6

NOTE—The clean-ups were made on ore No. 5 in the experimental plant of mill C. From 19 to 37 tons of roasted ore were treated for each clean-up. The

value of the roasted ore, *minus* soluble salts, from which the apparent extraction is calculated, is not given in the table.

It will be noted, in comparing the apparent extraction based upon corrected assay with the actual extraction, that the latter is, in all cases except two, higher than the former. This would indicate that the corrected assay is still too low. In fact, it could not be otherwise, since this assay does not take account of the silver lost by volatilization.

TABLE XII.—*Statistics of the Apparent and Actual Extraction of Silver by Amalgamation, based upon Commercial and Corrected Assays.*

No.	Value of Roasted Ore. Ounces per Ton.		Value of Tailings. Ounces per Ton.		Apparent Extraction. Per cent.		Actual Extraction. Per cent.	
	Commer. Assay.	Corr. Assay.	Commer. Assay.	Corr. Assay.	Commer. Assay.	Corr. Assay.	Commer. Assay.	Corr. Assay.
1.....	32.65	36.0	3.2	3.6	91.6	90.75	104.13	94.45
2.....	32.64	35.6	3.2	5.4	89.10	86.2	95.5	87.56
3.....	34.25	37.8	4.9	6.4	86.73	84.2	92.9	84.42
4.....	43.43	46.96	.....	.....	85.5	.....	86.3	79.6
5.....	48.22	53.08	.....	.....	86.3	.....	88.9	80.8

NOTE.—Nos. 1, 2 and 3 are clean-ups from ore No. 5 in mill D. The quantities of ore run through the pans were 703, 795 and 831 tons respectively. The very high extraction of 94.45 per cent. of No. 1 seems somewhat doubtful. Nos. 4 and 5 refer to clean-ups of 50 tons each from ores Nos. 8 and 9, in mill H. The corrected value of the roasted ore from which the apparent extraction is calculated, does not appear in the table.

V.—THE EXTRACTION OF SILVER BY THE RUSSELL PROCESS, AND BY AMALGAMATION, COMPARED ON THE BASIS OF CORRECTED ASSAYS.

TABLE XIII.—*The Extraction of Silver by the Russell Process and by Amalgamation, Compared on the Basis of Corrected Assays.*

No.	Actual Extraction.		Difference in Favor of Russell Process. Per cent.
	Russell Process Per cent.	Amalgamation Per cent.	
1.....	92.7	88.1	4.6
2.....	91.5	79.6	11.9
3.....	91.8	80.8	11.0

NOTE.—No. 1 gives the average clean-up on the eight lots of Table XI. by the Russell process and the average clean-up on lots Nos. 1, 2 and 3 of Table XII. by amalgamation. Nos. 2 and 3 give the clean-ups on 50-ton lots of ores No. 8 and No. 9, both worked in mill H.

Mr. Daggett, in his paper on the Russell process, *Trans.*, xvi., 486, has given numerous statistics comparing, on the same ores, the extraction of silver by the Russell process with that by amalgamation. These statistics, however, are all based upon commercial ore-assays. In Table XIII. I add a few figures based upon corrected assays.

Appendix.

Note on the Extraction of Gold.—Ore No. 5 contains from $0.78 to $1.34 of gold to the ton. The actual extraction of gold by the Russell process, in the eight clean-ups of Table XI., varied between 45 and 124 per cent., and in the three amalgamation clean-ups of Table XIII. (Nos. 1, 2, and 3) it varied between 46 and 82 per cent. The roasting seems to influence these variable results, assuming that the ore-assays were correct.* Ore No. 5 being almost free from sulphurets, is sometimes roasted with the addition of $\frac{1}{2}$ to $\frac{2}{3}$ per cent. of sulphur. It has been observed that the ore so roasted yielded a much larger percentage of its gold than ore roasted without sulphur. In order to prove this definitely, further experiments would be necessary. Statistics on the percentage of gold extracted are somewhat unreliable in cases where the ore contains only one dollar or less of gold per ton, and the obtaining of a correct sample and a correct assay becomes difficult. There is no doubt, however, that the Russell process extracts, in most cases, a higher percentage of gold from gold-bearing silver-ores than amalgamation.

THE ORE-DEPOSITS OF BUTTE CITY.

BY R. G. BROWN, BUTTE, MONTANA.

(Bridgeport Meeting, October, 1894.)

This paper will present a general description of the different mineral belts in the Butte City region (limited, however, to the copper and contiguous silver veins) such as may serve to give an intelligent idea of the distribution and characteristics of the deposits, forming thus a groundwork of information, which it is hoped will have value, when later writers enter into particularized detail.

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* In the clean-up of 124 per cent. the gold-assay of the ore was evidently too low.

It will further be a great satisfaction if the existence of this groundwork shall suggest to others the need for particularized detail, and prompt the furnishing of such. It is truly a surprising thing that from a region, the seat of such mining activity and where the developments have been so extensive there should have come scarcely an instance of the study—or at least the recording of a study—of local conditions by local engineers, that is by those presumably best qualified for such work. It must not be understood that the writings of Emmons, Pearce and Blake have been ignored; but they do not belong to the above category.

General Remarks.

The mining district of Butte City, Montana, is roughly seven miles long, from the continental divide on the east to the gravel flats beyond the Blue Bird on the west, by four miles wide.

The country-rock is in general granite of two clearly-defined varieties: a dark syenitic in the eastern portion, and a pale acid granite, composed of feldspar and quartz, with an occasional minute amount of mica, in the western portion.

These have been called, with more or less appropriateness, the "Butte" and "Blue Bird" granites, though small bodies of each occur in the territory of the other.

Through these occur dikes of rhyolite, probably connected in some way, as yet undetermined, with the mass of the Big Butte, itself a cone of eruptive rock, scarcely fissured, and showing no mineralization.

There are also local occurrences of quartz-porphyry, possibly in the form of dikes, showing on the Anaconda hill and in the railroad cuts below the St. Lawrence mine, and suggested, though not clearly made out elsewhere.

This paper will describe only that portion of the region included within Butte City, and between it and its immediate environs of Centerville, Walkerville and Meaderville.

Topography.

The city of Butte lies about three miles west of the continental divide, in Silver Bow county (formerly a part of Deer Lodge county), Montana.

A meridional valley, evidenced in Fig. 1 (a photograph taken from the top of the Big Butte) as a source of smoke, widens southward and westward into an extensive flat, and further to the west,

FIG. 1.

View of Butte City Mines, from top of Big Butte. 1, Mt. View; 2, Anaconda; 3, Never Sweat; 4, Parrot; 5, Lloyd Tunnel (just south of Pennsylvania); 6, Silver Bow; 7, Gagnon; 8, Oro Butte; 9, Steward (dump); 10, Acquisition (gallows-frame); 11. Mt. Moriah; 12, Anselmo; 13, Missoula Gulch; 14, Dublin Gulch.

the flat narrows again to a valley. Silver Bow creek, flowing southward through one valley, circles to the right and west over the flat, and effects an exit through the other valley.

Northward from the flat rises the Butte City slope in an ascent which is as abrupt for a city as it is gradual for a hill, in our western country.

Two principal gulches cut this slope, and innumerable small ones furrow it. One of the two main gulches is Missoula gulch, in the western portion and to the westward of which rises the Big Butte, the source of the name (and, as some think, of the riches) of the region. This gulch extends northward from the flat until cut off by a notable cross-ridge. The other is Dublin gulch, nearly 5000 feet to the east. The latter is a deep gash almost severing the Anaconda hill from the main slope.

The main Butte slope, a couple of miles or so north from the flat, absorbs into itself a cross-ridge—the same that cuts across the head of Missoula gulch; spreads out eastward into the irregular semblance of a plateau, and then falls, more or less sharply, to the first-mentioned meridional valley, the eastern wall of which is the continental divide, overtopping the Butte City slope by many hundred feet.

A homely illustration may assist this description. If the open right hand be laid, palm down, on a table, the three middle fingers being held well together and pressed down, while the back of the hand is raised about one inch at the wrist, and the tip of the little finger be separated half-an-inch from the others and the thumb bent so that the tip is half-an-inch from the hand, both tips being against the table; then, the table in front (south) may represent the flat; the table to the left (east) the meridional valley, to the left of which rises the divide; the three middle fingers, rising to greater steepness with the back of the hand, the main Butte City slope; the crack between the little finger and the rest, Missoula gulch, with the Big Butte just to the right of the middle joint; and the thumb, Anaconda hill, separated from the rest by Dublin gulch.

This is further made plain by Figs. 1 and 2. Fig. 1 looks east, with the divide for a background. The Anaconda shaft house, with its multiplied stacks, shows in the center; and crowning the top of the hill is the Mountain View mine; while on the hither side and below them twists Dublin gulch. Quite in the foreground is Missoula gulch. The main portion of the city is to the right of the picture.

Fig. 2 is a somewhat sketchy map in which an attempt has been

made to show the supposed limits of the various belts and their relation. A few of the most interesting claims are roughly located by the intersection of lettered vertical and numbered horizontal lines.

Surface Characteristics.—The general appearance of the surface is rounded, in marked contrast to the higher rocks of the divide (which are always before the eye, to accentuate the difference), but in complete uniformity with the foot-hills (Fig. 1).

The innumerable small gulches are comparatively broad and shallow, bearing out the general appearance; but there are a great number of granite boulders, showing as a rule in groups, and occupying prominent ridges and slopes; these, apart from their typical weathering, show no signs of the disintegration, which is the common fate of the underground granite, when exposed to the air. It is to be noted, however, that there is one variety of the underground granite, very hard, greenish and watery, which does not quickly decompose and which these boulders somewhat resemble.

In places (particularly up from Missoula gulch on the east), the granite boulders have weathered rough, leaving great lumps of feldspar in relief on their surfaces. This granite is noticeably coarse in all its constituents.

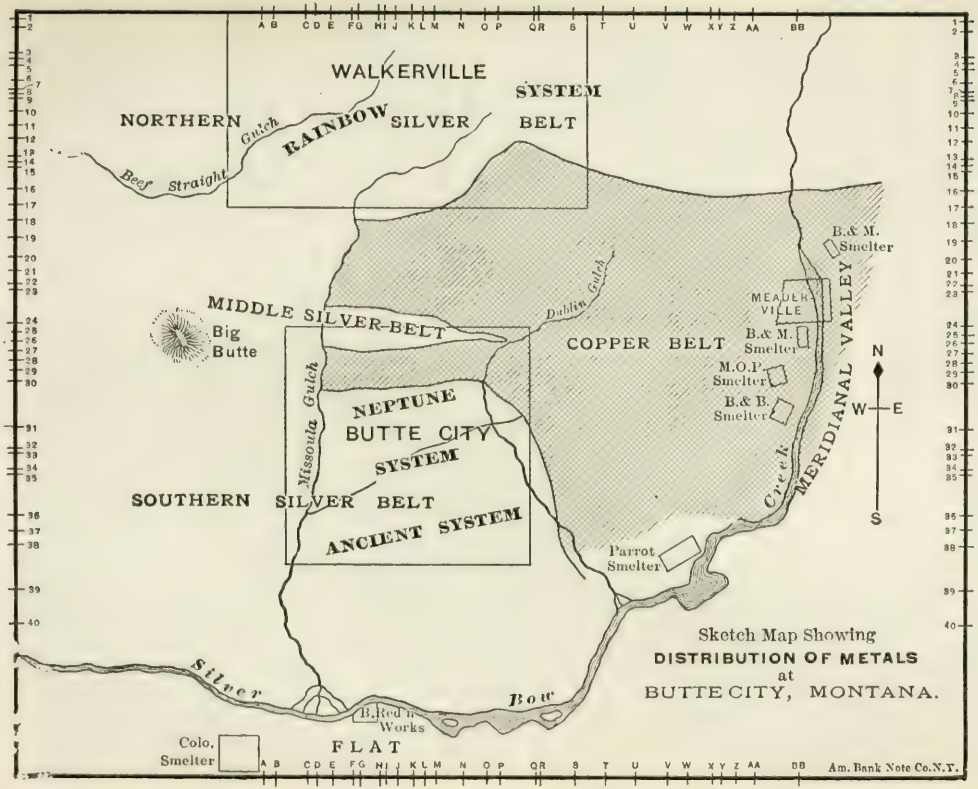
The region to which this paper is limited divides itself naturally into several belts: a northern silver-belt; a southern silver-belt, and a copper-belt, partly contained between the others and containing, in turn, a third, or middle, silver-belt (Fig. 2).

The copper-belt, to which is due the fame of the town, is not only bounded on the north, and in part on the south, by silver-belts, but is completely replaced on the strike to the west by silver-veins, though the small, middle silver-belt (the Mount Moriah-Acquisition-Steward belt, F 25; J 25; O 26, in Fig. 2) which is darted, tongue-like, into it from the west, extends across the line of change, *i.e.*, the line of Missoula gulch. It must not be understood that fissures, containing copper on the east of Missoula gulch contain silver to the west; for surface indications seem to negative the view that the fissures on the two sides are identical, and underground developments do not touch the question.

Northern Silver-Belt.

Characteristic of the northern silver-belt is a manganous and siliceous gangue, showing frequently blackened outcrops of great prominence. Such prominence, in fact, may be considered as typical of the silver-veins of the region, though some escape the distinction.

FIG. 2.



INDEX TO PRINCIPAL CLAIMS.

J 25, Acquisition.
M 9, Alice.
S 27, Anaconda.
E 36, Ancient.
C 26, Anselmo.
B 13, Belcher.
U 19, Bell.
O 11, Belle of Butte.
T 32, Belmont.
V 6, Black Rock.
I 14, Blue Wing.
N 21, Buffalo.
AA 22, Colusa.
U 8, Elm Orlu.
M 34, Emma.
Z 3, Enterprise.
L 21, Estella.
D 16, Eveline.
N 9, Fraction.
K 28, Gagnon.
F 1, Glengarry, No. 1.
Y 35, Glengarry, No. 2.
D 7, Goldsmith.
Q 17, Gray Rock.
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A 26, Hope.
N 15, Lexington.
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J 2, Margaret Ann.
BB 17, Mat.
L 33, Morning Star.
H 23, Moscow.
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F 25, Mt. Moriah.
U 24, Mt. View.
E 34, Neptune.
W 4, Niagara.
M 28, Original.
G 23, Oro Butte.
I 21, Pacific.
Q 29, Parrot.
W 30, Pennsylvania.
S 9, Poser.
X 4, Raymond.
D 12, Rising Star.
M 40, Shonbar.
AA 31, Silver Bow.
U 18, Speculator.
U 27, St. Lawrence.
E 39, Star West.
G 34, Stevens.
O 26, Steward.
F 38, Travona.
R 9, Valdemere.
A 29, Volunteer.

The great silver-lode, the "Rainbow," if not the "Mother lode," in the somewhat misleading symbolism of the hills, occupies the axis of this northern belt, and sketches a doubly curving bow, through twelve claims at least, from the Belcher on the west into the Enterprise on the east, a distance of eleven thousand feet, with a general strike a few degrees north of east.*

Through its whole length it has been largely explored, and has disclosed in one claim at least, the Raymond (X 4, Fig. 2), a width of over one hundred feet, almost entirely of rhodochrosite and quartz.

It is characterized by many faults, some of which appear along the strike on the surface, noticeably on the Poser, while others are developed by the underground workings only. The dip is northerly in the western portion (an exception to the habit of the district) and southerly in the eastern; but the point of flexure, where one turns into the other, has not been developed. The walls are well defined, particularly the foot.

The vein appears sometimes as several members, either uniting in depth or along the strike, or branching away into the country rock and perhaps dying out.

The western part is hard and massive, the eastern part often crumbly, giving a mined rock of the size of chestnut coal.

Mineral Character.—As already observed, the regular gangue is rhodochrosite and quartz, finely banded in places. The accessory mineral occurrences are barite, in small mineral crystals (very rarely); sphalerite and fine-grained, sometimes pulverulent galena, in patches; small bodies of very siliceous chalcopyrite (noted once or twice), fine-grained and totally different in appearance from the same mineral in the copper-belt; also a small chute of copper-ore, which is probably a zinc-blende carrying a variable percentage of copper, and which also resembles none of the Butte copper ores.

The silver-bearing mineral on the eastern end of the lode, at least, is a very finely disseminated highly argentiferous tetrahedrite, occurring in pin-point specks and fine thread-like filagrees, following often definite lines of deposition and giving locally a banded appearance, sometimes concentric, as if deposited in small cavities of the quartz and rhodochrosite. With this is associated a small, but appreciable, amount of gold.

* The claims on the Rainbow are to be located on Fig. 2 as follows: Belcher, B 13; Rising Star, D 12; Moulton, H 10; Alice, M 9; Fraction, N 9; Magna Charta, P 9; Valdemere, R 9; Poser, S 9; Elm Orlu, U 8; Black Rock, V 6; Niagara, W 4; Raymond, X 4; Enterprise, Z 3.

There is not enough copper in this gray-copper—or rather there is not enough gray-copper in the ore—to make a noticeable percentage of copper, but in the oxidized ores it sometimes shows as films of malachite or azurite.

Pyrargyrite has been found a few times in extremely rich pieces of the ore, and pyrite is a constant, but valueless accessory. The smelters who have handled the material from parts of the lode remark the presence of bismuth in the product, but no bismuth-minerals have been detected in the ore. Stephanite has been reported from other veins of this belt, but has never been identified by the writer. In the region of a fault, above water-level, in the Black Rock (V 6, Fig. 2) gypsum was a frequent occurrence in cracks of the granite, sometimes giving $1\frac{1}{2}$-inch crystals, and evidently of recent formation.

In general, the characteristics of the Rainbow lode are those of the whole northern belt. To this belt belong, besides the mines enumerated as on the Rainbow lode, the following mines located on Fig. 2: The Blue Wing (I 14), Belle of Butte (O 11), Lexington (N 15), and the lately exploited Eveline (D 16) to the south of the western end of the Rainbow, while the number of minor properties of some probable value is legion. Here belongs also the Amy-Silversmith, famous as the subject of a recent decision of the United States Supreme Court.

Southern Silver-Belt.

Closely akin to the northern is the southern silver-belt, or, more accurately, the southern part of that belt, from, and including, the Ancient (E 36, Fig. 2) southward. There are several lines of jagged manganese-outcrops, which break across Missoula gulch westward, and continue visible for some miles, one or the other of them outcropping at intervals, entirely outside of the area covered by the present paper.

Followed to the east they gradually sink into the placer-ground south of the city, exhibiting, however, divergent strike, so that the southern veins, the Star West, for instance (E 39, Fig. 2), bearing 15° to 20° S. of E. are lost under the heavy wash of the flat, while those next on the north, bearing about as much N. of E., as represented by the Ancient (E 36), would naturally be looked for again on the thinly sanded slopes below Anaconda hill. Here, however, appear copper-veins, with a S. E. strike. How the one set of veins is related to the other is an interesting question never yet

answered; but it is scarcely to be supposed that a vein as large as the Ancient, measuring 75 feet across the apex, where it is cut by the grade of South Washington Street, and traceable for miles to the westward, would suddenly die out. This problem involves, perhaps, a definite answer to the question of the relative ages of the silver- and copper-deposits; and here we must eagerly look for light from the future developments of mining.

This system of veins has been, until very recently, notably productive, constituting also the paradise of small lessees by reason of the high grade of the surface-ores. Deep workings, on the other hand, have not proved so promising as in the Rainbow system.

One of the rhyolite dikes already mentioned cuts through this system, just east of Missoula gulch, and is developed in the underground workings of the Star West and Travona mines (E 39 and F 38, Fig. 2). This dike, which is, roughly, 100 feet thick, with a southerly strike, and occupies a line of small faulting, shows that the southern silver-belt antedated the rhyolitic eruption.

The general dip of the "Ancient" system is southerly, and the veins included in it are of all sizes, from the 80 feet of quartz and manganese-minerals in the Ancient to veins of a few inches.

Here, as in the northern system, the proportion of gold is quite appreciable (if anything, a little higher than in that system) while the manganese is accompanied with less silica. The veins are well-defined, and exhibit many small faults, of 3 feet and thereabouts.

The amount of manganese in the ore has made it valuable to the smelters as a flux, particularly in the oxidized ores, which were furnished in considerable quantities in earlier days.

Mineral Character.—The minerals in this belt differ from those of the northern belt rather in details than in kind. Thus the rhodochrosite is more massive and has a coarsely pearly fracture as compared with the smooth, dull face exhibited in the other belt. It is, as a rule, of a darker rose-color, and does not fade so soon upon exposure. The galena is more plentiful and gives cubical cleavage of a large habitus. Zinc-blende appears in resinous, pea-sized crystals, in the massive rhodochrosite, or as irregular patches shading into galena. The pyrite is somewhat coarser and more abundant, and carries the precious metals. Calcite is at times found as crystals in cavities, or as crystalline plates in cracks, showing double comb-structure in section, and evidently of recent formation.

The silver and the subordinate gold, besides occurring in the pyrite, occur in the galena and sphalerite patches, but not in the pea-sized

crystals. Wire-silver is not infrequent, even below water-level, in vugs partly filled with clay. Indeed, many of these lumps of clay are bound together with silver wires, as mortar is with hair.

North of this system, and strongly differentiated from it, both by surface-indications and by mineral characteristics, is another silver-system, extending northward to the copper-belt, westward across Missoula gulch, and eastward up to the copper-region. Here also we would naturally look for some clue as to the comparative ages of the silver- and copper-veins, but no recorded developments have given it thus far. Towards the east, the silver-veins become impoverished, and are not developed.

The most important members of this system, so far as now known, are the Neptune, Stevens, and Morning Star (E 34, G 34, and L 33, Fig. 2), all now idle. Though the veins appear west of Missoula gulch, they have never been productive in that region.

These veins are characterized by scanty croppings of brown quartz, under a wash so thin as to insure their early discovery and exploitation. Manganese is not present in sufficient amount to give the black, oxidized ores, so marked in the southern system, though spots of the pink silicate and carbonate occur in the rock. The minerals are substantially the same as in the other system. The veins are well defined, but there are no large ones, the average width being 2 or 3 feet.

The ore is essentially of higher grade, and the proportion of gold to silver is perhaps a little higher than in the other systems.

Middle Silver-Belt.

A very remarkable silver belt remains to be described, which projects itself right into the heart of the copper country north of the Gagnon-Original-Parrot system (K 28, M 28, and Q 29, Fig. 2). This terminates in a point near the eastern end of the Steward claim (O 26), while westward it crosses Missoula gulch, and is cut off by the rhyolite of the Big Butte.

The ores of this belt are of essentially higher grade, again, than those of either of the other silver-belts, though this is not quite so noticeable on the western end, and are characterized by their high gold-values, which reach 2 ounces gold per 100 ounces silver in the best region, that is, on the Acquisition claim, just west of Main St. (J 25, Fig. 2).

Crystals of calcite, of the diameter of a lead-pencil, occur in vugs in the quartz; but although this is a marked characteristic, the amount of calcite is not sufficient to prevent the ore from being very siliceous.

The veins range in width from a few inches of quartz, "frozen" to the granite of both walls, to several feet of somewhat crumbly quartz; the narrow ones are the richer.

Manganese-minerals are present in very subordinate amount, so that, while occupying the top of the series as to richness of ore, these veins stand lowest in manganese-percentage.

The only extensive mining west of Missoula gulch and east of the Butte has been on this system, where a great deal of valuable ore has been mined on the Anselmo, Hope, and Volunteer (C 26, A 26, and A 29, Fig. 2), but mainly by lessees and at shallow depths.

Mineral Character.—The mineral characteristics of this belt (besides the calcite crystals, already mentioned, but only found in the middle portion of it) are: a subordinate, but general, occurrence of rhodochrosite, sometimes crystallized in vugs, but more often cemented in with the quartz; sphalerite in small patches and large bunches (the latter slightly contaminated with copper, so as to show stain on oxidation), and of low grade in precious metals; cerussite, of good silver-value, near the surface, passing below water-level into a zincose galena, with a little less silver; and fine-grained pyrite, usually a good carrier of silver and gold.

The precious metals, besides accompanying the pyrite and the lead-ores, are present as a gray cloud or "cast" in the quartz, which has not been mineralogically identified.

In studying this belt it must not be forgotten that while cutting the gulch it is, in turn, cut off by the Butte; so that the gulch, which marks a "break" for the copper, seems to be a line of fault, of older date than the Butte.

Copper-Belt.

The copper-belt, which is vastly more important, economically, than all of the rest put together, is, roughly, 3000 feet wide at its western end, at Missoula gulch, and some 7000 feet at its eastern part, before it sinks under the heavy granitic sands of the meridional valley, 10,000 feet to the east. West of Missoula gulch it does not extend, that whole region being the seat of more or less valuable silver-deposits, as has been explained. Thus it will be seen that the copper-veins do not approach the Butte within 2000 feet.

In general, the strike of the copper-lodes is east and west, though in the southeastern portion it swerves to the south. The general dip is south, 10 to 15 degrees from the vertical; this likewise changes in the same quarter, becoming, in places, as flat as 45 degrees.

The lodes exhibit great irregularity. In many places they have

no defined walls, the only distinction between veins and country-rock being a commercial ore, based on shipping value. Often where walls have seemed to be clearly defined, deposits of value are developed behind them. Masses of unaltered granite occur in close conjunction with ore-bodies; and, again, decomposed granite is found far outside of the fractured zone, and where, by reason of the absence of ores, the effect of decomposing solutions would not have been expected.

The ore occurs in shoots that "finger" or "pinch" out along the strike, though often reaching to several hundred feet, and these chutes may be considered as tolerably continuous in depth, though often subject to small faults. Frequently the fault has effected what appears as a longitudinal movement along the strike, so that the ore-masses overlap. Hence, when a lens is found in mining to pinch out, with a good show of slickensides or clay walls, the natural course of the miner is to cross-cut into the foot- or hanging-wall, as the first step in further development.

The lenticular form is not so characteristic of the larger bodies as of the smaller. The veins vary in size from a foot or two of compact ore (at rare intervals banded), through all gradations, up to the astonishing 100 feet or more in width mined in the Anaconda. Ten-foot stopes are common, 20-foot stopes are by no means rare. Even in these large bodies (perhaps especially in them) frequent prospecting by foot- and hanging-wall cross-cuts must be practiced, to develop the small bodies that branch off into the walls at intervals, as well as to furnish waste to fill the large cavities left by such wide stoping, which can be but temporarily held open by even the best timbering.

It is scarcely necessary to explain that stopes of such great width do not imply solid ore of equal width. The waste (decomposed granite, porphyry and some quartz) in such a stope would commonly be 25 per cent., and might be sometimes much more.

The gangue is rather granitic than quartzose (this term being used in preference to "siliceous," to denote silica in the form of vein-quartz).

The surface-indications of these copper-veins are noticeable, if not prominent, outcrops of dark-red or brown quartz. Just below the surface this is associated with soft red and yellow iron oxide, carrying high values in silver and particularly in gold, but poor in copper. To this last statement, however, there is an exception: there are limited occurrences of copper oxide of high percentage in copper, but of low silver- and gold-value.

In the lower portions of the iron oxide zone patches or streaks of green carbonate of copper (less frequently the blue carbonate) are often found.

These altered products reach to perhaps 40, perhaps 300 feet below the surface, according to the locality, and change promptly at water-level to the normal ores. There follows below a region of varying height, of valuable rock, which again slowly deteriorates in depth, this deterioration, however, being so retarded finally as to be scarcely appreciable.

One of two other things may, however, happen below the zone of the first "base" ore, namely, the ore-body may suddenly become poor, remaining so for 100 or 200 feet, and then recovering, or even exceeding its former value; or the ore-body may suddenly become poor, and not recover at all, but a new body may be developed, either in a different part of the vein, on the strike, or in a parallel, neighboring vein or split. In either of these cases the new body will probably have its top some 100 feet below the bottom of the first body. Very seldom can the phenomenon be traced to faulting, since the bodies will usually die out into the granite. The appearance is rather that of a shifting of the depositing solutions from one set of fissures, or one part of a single set, to another.

In the southwestern portion of the copper-region the Gagnon-Original vein (K 28, Fig. 2) is remarkable for its high proportion of silver; but, although this vein undoubtedly appears further east as one of the veins of the Parrot system, this characteristic of it is lost.

The strike of this Gagnon-Parrot system is a few degrees north of east, which is, moreover, for the most part, the prevailing strike in the copper-region; but in the southeastern portion the veins have swung towards the meridian, giving a strike of nearly 45 degrees. Thus it comes to pass that these veins cut across the strike of the "Ancient" system in the southwest silver-belt, nearly at right angles, although, as before stated, the actual intersection of the two systems has not yet been developed.

This southeastern copper-region has only recently been opened up. It is but a year or two since the discovery of copper below outcrops, almost smothered in heavy wash and generally believed to be too far in the flat for productiveness, suggested an indefinite extension of the copper-deposits in that direction. Extensive development-work was immediately instituted, and several valuable discoveries resulted. It is here that Glengarry No. 2 (Y 35, Fig. 2) has yielded to the Montana Ore-Purchasing Company a large quantity of ore. This is the farthest outpost in the flat.

But upon following the strike, not only of the last-named, but of all, the copper-veins eastward to the foothills, copper-ore is again found to appear, not as yet developed in valuable quantities, but of a quality largely the same as that of the great deposits. This is interesting, but, perhaps, not remarkable. It might rather be remarkable (and, indeed, it is in the case of the western limit of the copper-belt) if such a shattering, as is evidenced by the large deposits, had not extended so far. How thorough this shattering was is suggested by Fig. 3, a sketch on East Granite St. where the section has been well disclosed by recent grading. The occurrence of a dike of what seems to be acid granite* is also to be noted with interest, but it is still to be determined whether this follows the strike of the shattered zone.

Mineral Character.—The mineral characteristics, so far as determined, are varying proportions of the dark sulphides of copper, shading into chalcopyrite, and again into pyrite. These three occur also intimately mixed, so that one often sees yellow, pea-like grains of chalcopyrite embedded in steel-gray, perhaps slightly porous, chalcocite, side by side with whiter grains of pyrite, or even the latter, appearing in smaller grains in the yellow sulphide itself. Only the two pyrites are seen in crystals, and even these occur more commonly in masses. Bornite is frequent.

The gangue, as already observed, is largely granitic, with vein-quartz not prominent. Manganese-minerals are absent, conspicuously so, in view of their predominance in the surrounding silver-region.

The Gagnon (K 28, Fig. 2), has some peculiar minerals, which have been studied

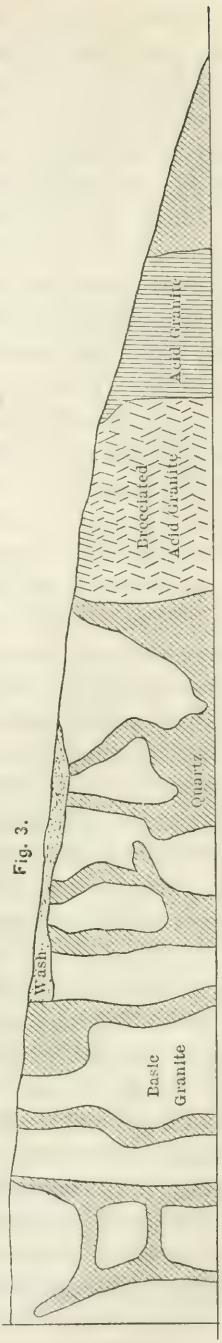

Fig. 3. Sketch on East Granite Street, Butte City. (See footnote.)

* The decomposed rock at the surface resembles the acid granite; but since this paper was written, a further examination has convinced me that it is porphyry.

and reported to the Institute by Mr. Pearce (*Trans.*, xvi., 62). These have been detected in the same vein eastward in the Original (M 28, Fig. 2), but are not known to exist beyond this point.

To the catalogue of associated minerals may be added fluorite, occurring in small crystals in the Estella (L 21, Fig. 2), and considerable quantities of galena and sphalerite in the Moscow and Oro Butte (H 23, G 23, Fig. 2), still further to the northwest. Once in a while a little barite appears. It is noticeable that much of the ore from just north of the middle silver-belt has the same high silver-assays as characterize the veins south of the same belt.

A word or two in closing as to the most notable deposits of the copper-belt. There is scarcely a part of the belt that has not yielded large quantities of ore, and the actually paying region shows constant enlargement, so that a description, true to-day, must be modified to-morrow.

The Anaconda and Chambers Syndicate properties lie in two principal groups, the one surrounding the Anaconda (S 27, Fig. 2), and the other stretching eastward from the Buffalo (N 21).

The Boston and Montana properties can also be classed in two groups, the one surrounding the Leonard shaft, in Meaderville (approximately AA 22), and the other in the Anaconda hill region, including, as best known, the Mountain View (U 24).

The Butte and Boston includes the Gray Rock (Q 17), close to the northern limit of the copper, and the Silver Bow group (AA 31) almost at the other limit.

The Parrot ownership covers the Moscow (H 23) to the northwest, and the group south and west of the Parrot claim (Q 29).

The Colorado Company's properties lie closely around the Gagnon (K 28).

The Clark properties belong to the same system: one just to the east of the Parrot; one just to the east of the Gagnon; a third just to the north, and connecting properties between.

The Montana Ore-Purchasing Company now holds the Glengarry No. 2 (Y 35) and the Belmont (T 32).

Finally, the writer has avoided all theorizing and argumentation, desiring not to maintain a thesis of any kind, but simply to give a sound basis of facts, free from that suspicion of partiality or incompleteness which inevitably attaches to such a statement, offered in support of a particular theory.

THE MANGANESE SLAGS OF TOMBSTONE, ARIZONA.

BY JOHN A. CHURCH, NEW YORK CITY.

(Bridgeport Meeting, October, 1894.)

WHEN, in 1879, I examined the mines of the Tombstone Mill and Mining Company, at Tombstone, Arizona, I found a bed of tailings containing 12,000 tons, which had a value of 9 to 12 ounces of silver per ton and some gold. The ore had been amalgamated in pans, in which each charge had been ground for four hours, having been previously stamped through rough-punched screens, which gave a product varying from 1-100th to 1-20th of an inch, according to the state of wear of the screen. It was certain that a large proportion of the tailings must be in fine slimes, and the question of recovering their value was difficult. Even now the concentration of slimes is an unwelcome task, and at that day very little was known about it. Nevertheless, after considering the results of a test by repanning the tailings, I advised the company to concentrate and smelt, although they had to face the difficulty of making a smelting-product from slimes containing only 3 per cent. of lead.

Limestone was abundant enough, but no iron suitable for flux was to be had for hundreds of miles. One of the company's claims contained an argentiferous manganese-ore, which gave but poor results by amalgamation. This was one reason for recommending smelting; for if manganese would play the part of a precipitant, as iron does, the company could smelt by combining two of its silver-ores, without other flux than the small amount of lime that might be needed. At that time no smelter had ventured to trust manganese as a precipitant, though large quantities of this ore containing silver were bought and used by custom-smelters, who looked upon the manganese solely as a basic flux for silica. I believe that no other furnace has been obliged to trust this metal, and that the manganese practice of Tombstone remains unique among the records of smelting lead-ores in the shaft-furnace.

The bright side of the problem was given by the fact that the mines were shallow and that the enterprise would at least begin with oxidized ores. The tailings contained about 85 per cent. of silica,

and of the remainder 8 or 9 per cent. consisted of iron oxide, alumina and water. The rest was carbonate and sulphate of lead, with a little pyrite, galena and blende. The silver was partly chloride, partly telluride and partly in the undetermined condition in which silver occurs in the carbonates. Gold occurred in combination—certainly, to some extent, as a telluride.

I have already described to the Institute * the method of concentrating this unpromising material. When the furnace started there was no lead to melt into the well, and the start was made with an empty hearth. The first run was made in September, 1882, and the furnace ran, with some interruption, until August, 1887. Its whole record is as follows:

Days run,	1,307
Tons smelted,	41,626
Product: lead (refined weight),	2,734 tons.
Product: silver (refined weight),	914,269.58 ounces.
Product: gold (refined weight),	5,182.02 ounces.

The furnace labored under all the disadvantages that arise from restriction to limited sources of supply. The concentrates were of sufficiently uniform quality, but the manganese was mined as the furnace required it, and the fluctuations in its quality were very great. Some of the company's ores, unsuited to amalgamation, were sent to the furnace. Other ores were purchased, but the supply was so variable as to increase rather than diminish the irregularities with which the furnace had to contend. The percentage of lead in the charge was never above 8.6, and ran down so that for long periods it was only 4.6 per cent. No matte was made. After the furnace stopped, and wages in the district had been very much reduced, the slag-dump was sorted over, and a certain amount of rich slags, matte, speiss, and lead was obtained; but the matte never formed a sufficient quantity to separate in smelting and act to clean the slags.

Under these circumstances, it was inevitable that the composition of the slag was often controlled by chance, and the assays show variations in its silica between 28 and 48 per cent. The variations in all other constituents must have been equally marked, but their limits are not known. Manganese went as high as 43 per cent., but its inferior limit was never ascertained. Pot-samples were taken with a rod, and the sample from each shift assayed. Nothing was

* *Trans.*, xv., 601.

determined but SiO_2, Pb, and Ag, and very few complete analyses were made. All that I can give are the following:

TABLE I.—*Slag-Analyses.*

	1	2	3	4	5
MnO	43.25	25.40	20.20	14.04	12.10
FeO	11.56	12.00	13.30	12.09	12.48
Al_2O_3	6.34	10.80	9.10	11.91	10.52
CaO	7.50	11.60	11.80	16.92	16.96
Pb	1.40	1.48	2.79	0.41	5.28
SiO_2	29.60	31.9	32.80	35.80	31.00
Ag (Oz. per ton)	tr.	1.48	1.20	.....	.....

1. Analysis by Mr. Iles. Slag made in 1863.
2. The manganese used was a mixture of all varieties obtainable.
3. Slag made from concentrates and manganese, without ore.
4. October, 1885. 2.8 MgO is counted in CaO.
5. February, 1886. This ran very slowly.

TABLE II.—*Analyses.*

	Manganese.		Concentrates.	Limestone.
MnO_2	58.80	60.10	0.35	5.20
Fe_2O_3	3.75	8.80	7.25	1.94
Al_2O_3	7.15	6.90	6.05	8.96
$CaCO_3$	.....	3.30	2.85	75.50
$MgCO_3$	.....	.....	5.05	tr.
Pb	1.50	1.60	.....	.....
$PbCO_3$	.....	.....	40.20	.....
SiO_2	10.80	13.60	30.90	7.20
Ag (Oz. per ton)	.....	.....	.....	5.2

At first, cast-iron jackets were used, but they gave great trouble at times by cracking at the shoulder. A small curved crack, such as would be made if the thumb-nail could be punched through the iron, would open and deluge the hearth with water. For that reason some campaigns were very short and gave bad slags. The evil was corrected by putting in steel jackets, the construction of which was somewhat unusual, being especially designed for convenience of repair. The narrow sides were formed of a strip of soft iron, with the edges flanged up to a U-shape and bent to give the shoulder or bosh. The inside plate was put on with rivets and the outside plate with tap-bolts. They proved to be extremely convenient and efficient. Several years after, under similar conditions of isolation in China, I had jackets of the same pattern made by Chinese blacksmiths.

TABLE III.—*Records of Blasts.*

No. of Blast.	Charge in Tons.								Products.			
	Concentrates.	Ore.	Manganese.	Limestone.	F. dust.	Slag.	Total.	Coke.	Lead..	Lead-Assay. Ag.	Lead-Assay. Au.	Slag.
									Tons.	Oz.	Oz.	Tons.
1.....	1,000.2	190.3	1,000.0	91.9	104.0	254.2	2,640.6	484.1	188.9	320	2.4	2,060
2.....	1,029.8	843.0	1,697.5	26.0	226.0	356.6	4,178.9	1035.7	321.6	241	1.0	2,869
3.....	1,857.1	22.2	1,466.1	6.8	239.2	204.2	3,795.6	755.2	325.5	248	2.3	2,420
4.....	1,471.6	279.6	1,284.1	.....	257.6	121.3	3,414.2	682.8	255.3	290	1.8	1,880
5.....	2,036.5	372.5	1,133.0	.....	296.5	125.0	3,963.5	709.0	255.5	350	2.1	2,405
6.....	506.0	704.0	916.0	.....	125.0	102.0	2,353.0	545.8	149.9	422	1.1	1,536
7.....	594.6	221.5	546.0	.....	140.0	66.7	1,568.8	345.9	118.6	281	0.9	945
8.....	4,369.0	388.6	3,180.0	.....	797.0	422.0	9,156.6	1876.7	616.8	315	1.8	5,874
9.....	1,252.0	1406.7	1,712.3	.....	660.0	315.4	5,346.4	1072.7	261.7	514	1.7	3,197
10.....	1,019.8	605.1	1,023.8	.....	280.0	297.8	3,226.5	679.4	150.1	402	1.6	2,150
Total.....	15,136.6	5033.5	13,958.8	124.7	3125.3	2265.2	39,644.1	8187.3	2643.9	.....	....	25,336
Averages	.....	.....	.....	.....	.....	.....	.....	.....	.....	329	1.6	.....

Omitting some of the irregular blasts and also the first two, the records of which were not kept in sufficient detail, the data of ten blasts, covering nearly 40,000 tons of charge, are shown in the Tables III. and IV. The record is continuous from May 10, 1883, to August 31, 1887, the entire work of that period being given. It covers, in fact, all but 1982 tons of the material smelted. The weight of lead previously given as 2734 tons is the refiners' return of soft lead. The actual weight of crude bullion at the furnace is not given in the records; but allowing a loss of 5 per cent. in treatment, the weight of crude bullion would be 2878 tons, and all the calculations in this paper are made on this estimated weight, for convenience of comparison with other furnace-practice. The 2644 tons of lead in Table III. are crude bullion.

TABLE IV.—*Percentages and Slag-Assays.*

No. of Blast.	Charge in Percentages.							Lead.	Slag Assay.		
	Concentrates.	Ore.	Mn.	Limestone.	F. dust.	Slag.	Coke.	Per cent.	SiO_2. Per cent.	Pb. Per cent.	Ag. Oz. per ton.
1.....	37.9	7.2	37.9	3.5	3.9	9.6	18.3	7.1	32.70	1.71	1.77
2.....	24.7	20.1	40.6	0.6	5.4	8.6	24.8	7.7	34.42	2.31	1.89
3.....	48.9	0.6	38.6	0.2	6.3	5.4	19.8	8.6	32.22	1.90	1.10
4.....	43.1	8.2	37.6	...	7.5	3.6	20.0	7.5	36.43	1.58	1.04
5.....	51.3	9.4	28.6	...	7.5	3.2	18.0	6.5	37.09	1.78	1.48
6.....	21.5	30.0	38.9	...	5.3	4.3	23.2	6.4	37.61	0.51	2.17
7.....	37.9	14.1	34.8	...	8.9	4.3	22.0	7.6	34.50	0.25	1.45
8.....	47.7	4.3	34.7	...	8.7	4.6	20.5	6.7	34.08	1.40	1.35
9.....	23.5	26.3	32.0	...	12.3	5.9	20.0	4.9	38.56	1.56	2.73
10.....	31.6	18.8	31.7	...	8.7	9.2	21.0	4.6	38.06	2.89	2.68
Average...	38.2	12.7	35.2	...	7.9	5.7	20.6	6.7	.....	.....	.....

It will be noticed that the percentage of lead in the charge was very low indeed; and when it is considered that no matte was made I suppose it will stand as the very minimum in the metallurgical practice of this country. At times it was necessary to charge back some of the bullion; but this practice is apt to be as wasteful with manganese as it is acknowledged to be with other flux. The lead returned in the form of bullion for the whole period was 6.7 per cent. The slag contained on an average 1.68 Pb, which is just above 1 per cent. on the charge. There can be no doubt that these results are very favorable, considering the circumstances; and later on I

will show that the work done in some months was really triumphant.

The success must be attributed in part to the unusually high percentage of volatile matters in the flux. The coke used was partly English "patent," which is very good, giving about 11 per cent. ash, and partly New Mexico coke which I think always had 20 per cent. of ash and sometimes got to be as bad as coke can be and sell for $15 per ton! It was very friable, the amount of fine stuff rejected being sometimes 10 and 15 per cent.

If 15 per cent. is allowed for the average ash of the coke we can calculate the amount of *fusible* solids going into the furnace:

	Tons.
Charge,	39,644
Ash of coke, 15 per cent.,	1,228
	40,872
Slag made, 25,336 tons.	
Lead made, 2,644 tons.	27,980
Volatilized,	12,892

This is a volatilization of 31.5 per cent. of the charge, omitting carbon. If we omit both the ash of the coke and the slag recharged, the volatilization amounts to 31.1 per cent. of the concentrates, manganese, etc., which contain the vaporizable materials. I do not know what the proportion is in the ordinary work of American smelters; but I think it was unusually high at Tombstone.

When we compare the 2644 tons of bullion made with the 25,336 tons slag and 2644 tons lead, total, 27,980 tons, flowing from the furnace, we get a lead-charge of 9.4 per cent. As the proportion of lead on the gross charge was 6.67 per cent., it is evident that a volatilization of 30 per cent. has taken place, which agrees nearly enough with the calculation made above from other elements.

It may appear that such calculations are somewhat refined, if not finical; but I shall show later on that the lead-charge went down as low as 1.5 and 2 per cent. at times, and averaged 4 per cent. for a month, and that such percentages do not forbid good slags. Remembering that no matte was made, I believe the performance was almost unique; and the circumstances that made it possible must have interest.

In the ninth and tenth blasts, 8573 tons of charge was smelted with the low lead-charge of 4.8 per cent.; and considering the absence of matte, this is a noteworthy performance for runs that cov-

ered together a period of 264 days. When we examine the charge for its volatile constituents we find that the lead-charge was 7.16 per cent. of the fusible solids.

The proportion of volatile matters in the fusible solids of the charge, the percentage of lead calculated on the gross charge and also on the product of slag and bullion are shown in Table V. The weight of the slag is obtained by allowing 300 pounds each to the number of pots drawn.

TABLE V.—*Proportion of Lead to Solid Fused Charge.*

No. of Blast.	Volatile in charge. Per cent.	Lead in gross charge. Per cent.	Lead calculated on Slag and Bullion. Per cent.
1	17	7.11	8.40
2	24	7.70	10.08
3	28	8.57	11.89
4	37	7.48	11.96
5	33	6.45	9.60
6	29	6 37	8.89
7	32	7.56	11 14
8	29	6.73	9.50
9	35	4.89	7.56
10	29	4.65	6.52
Averages	29.4	6.67	9.44

The slag was hot and fluid, and retained its fluidity for a long time in the pot. It was sometimes remarkably free from lead and silver, as the records in Table VI. show. The slag-assays are averaged for each month, and, in addition, the number of days on which they showed merely a "trace" of either metal are given.

TABLE VI.—*Slag-Assays for Five Months.*

1885.	AVERAGE.			TRACE.		Per cent. Lead in charge
	SiO_2. Per cent.	Pb. Per cent.	Ag. Oz. pr ton	Pb.	Ag.	
June	39.4	0.8	1.86	8 days	.....	6.62
July	36.0	0.16	2.49	19 "	.....	6.57
August	34.56	0.18	1.80	10 "	.....	7.76
September	34.26	0.26	1.44	13 "	.....	7.27
October	35.10	0.42	1.14	6 "	1 day	8.20

The months given correspond nearly to No. 6 (May 23d to August

6th), and No. 7 (August 16th to October 9th) runs in the former tables. This was the best work done on lead by the furnace; the best on silver being in blasts Nos. 3 and 4. The silver in the slag never rose above 0.78 ounces for 100 ounces in the bullion, and varied between that and 0.4 ounces. It will be noticed that the cleanness of the slag did not depend upon its silica contents. This fact is shown in detail all through the records. Eleven per cent. of the slag-assays give 40 to 48 SiO_2; 25 per cent. show 38 SiO_2 and over. Apparently, manganese-slags can be very siliceous and still very clean; and if we attribute this result to their extreme fluidity, it would indicate that the losses in very siliceous slags are due rather to their infusibility and consequent viscosity than to the combining power of their silica. A few of the most siliceous slags and the conditions under which they were made are shown in Table VII.

TABLE VII.—*Highly Siliceous Slags.*

Slag Assay.			Charge.	Lead.	Lead.	Bullion-Assay.
SiO_2. Per cent.	Pb. Per cent.	Ag. Oz. per ton.	Tons.	Tons.	Per cent.	Oz. per ton.
46.5	2.0	2.4	32.6	1.87	5.7	311
48.2	0.9	2.4	23.4	1.04	3.7	335
45.2	2.4	0.8	35.5	2.55	7.2	290
47.5	1.4	1.0	33.5	2.65	8.0	.....
46.2	2.4	0.8	36.9	2.70	7.3	312
45.0	0.9	2.8	31.5	1.35	4.3	467

The sorting of the slag-dump yielded a quantity of scrap-lead, matte, speiss and rich slags, the weight and contents of which were as follows:

Weight.	Contents.		
Tons.	Silver, oz.	Gold, oz.	Lead, pounds.
354.174	25,041.63	179.00	94,359*
			Per cent.
Per ton,	70.7	0.505	13.32

This is less than 1 per cent. on the whole quantity smelted, and less than 1.3 per cent. on the absolute solids, omitting the volatile constituents. If we confine ourselves to those blasts in which the percentage of lead was under 7 per cent., the percentage of this material to the gross charge was 1.47 per cent., and on the absolute solids, 2.13 per cent. I consider it exceedingly probable that most

* Equals 47.18 tons.

TABLE VIII.—*Work of May*, 1886.

	Slag-Assay.			Charges, Tons.	Lead, Tons.	Lead, Per cent.	Bullion-Assay. Ag, Oz. per ton.
	SiO_2, Per cent.	Pb, Per cent.	Ag, Oz. per ton.				
3	40.00	1.00	1.00	34.1	1.62	4.7	291
4	40.40	2.00	1.60	20.5	1.20	5.8	300
7	40.40	0.70	1.60	32.1	2.09	6.4	309
11	46.50	2.00	2.40	32.6	1.87	5.8	311
12	41.00	2.00	1.40	33.9	2.29	6.7	352
13	42.00	1.00	2.00	35.7	2.14	6.0	368
14	40.40	0.70	1.40	36.6	1.83	5.0	354
15	40.00	0.60	2.00	37.7	1.78	4.7	360
18	40.00	0.70	0.80	35.3	2.19	6.2	324
21	40.00	1.80	1.20	33.3	2.40	6.5	237
22	41.00	1.00	1.20	31.9	2.14	6.7	230
23	42.20	1.80	2.20	29.7	1.72	5.8	244
27	41.50	2.00	1.60	27.4	2.24	8.2	216
28	40.20	1.00	1.20	31.5	2.19	7.0	222
29	40.20	1.60	1.40	31.4	2.40	7.7	224
Average of month...	39.58	1.22	1.56	1071.0	63.01	5.9	287

of this matte was made in those periods when the bullion product ran down below 4 and 5 per cent.

The percentage of lead in this sorted material was only 13.32 per cent. and as a part of this was in clean lead, the lead contents of the matte must have been very low indeed, under 10 per cent., probably.

May, 1886, is a month in which the work done seems to indicate that high silica had no bad effect on the slag, and January, 1887, is a month in which SiO_2 was high, and Ag also abnormally high. Tables VIII. and IX. show the conditions for these two months respectively, giving in each case the averages for the month and also the days, in detail, on which the slag ran 40 SiO_2, or more.

Make what allowance we will, the record of January, 1887, is a very remarkable performance; 1073 tons of charge, 43.37 tons of bullion, a slag that averaged 39.8 SiO_2, bullion averaging 612 ounces, and with all these trying circumstances a loss of 1.4 per cent. Pb and 3 ounces Ag per ton of slag. The loss of lead was 16.64 per cent. and the loss of silver 6.5 per cent. of the amount charged. Fuel was 20.3 per cent., and the quality of the article is sufficiently indicated by the fact that for 218 tons used in the furnace, 20 tons were thrown away as too fine.

TABLE IX.—*Work of January*, 1887.

	Slag-Assay.			Charge, Tons.	Lead, Tons.	Lead, Per cent.	Bullion-Assay. Ounces Ag.
	SiO_2, Per cent.	Pb, Per cent.	Ag, Oz. per ton.				
2	40.00	1.20	2.60	39.5	1.30	3.4	550
3	44.20	0.50	2.00	38.6	1.30	3.4	580
4	42.00	1.00	2.80	35.2	1.09	3.2	610
5	44.00	1.60	4.00	34.1	1.41	4.1	630
7	41.20	1.20	4.80	34.8	1.04	3.0	650
11	40.80	2.60	5.20	34.3	0.67	2.0	780
12	43.40	3.00	6.00	38.2	0.57	1.5	808
13	40.00	1.80	4.00	36.2	1.18	3.3	720
14	40.40	0.80	2.20	38.8	1.50	3.9	644
16	42.80	0.50	2.60	34.3	1.09	3.2	650
17	42.00	2.40	3.20	33.3	1.40	4.2	650
18	40.00	1.80	2.00	40.4	1.35	3.4	630
19	40.20	1.40	2.00	48.3	1.09	2.3	680
21	40.00	1.00	2.80	36.5	1.35	3.7	600
24	40.00	2.00	4.00	28.6	1.83	6.3	560
30	40.40	1.20	2.00	28.0	1.72	6.2	500
31	40.80	1.40	1.80	33.0	1.61	4.8	560
Average of month...	39.80	1.40	3.00	1072.7	43.37	4.03	612

It is evident that no furnace could run with such extremely low percentage of lead as we find on some of these days. No doubt bullion was charged back, though the quantity so used is not recorded. There was some matte sorted out of the dump, as already said, and though it was not saved at the time it is probable that the greatest make of matte fell on these months of low bullion-product and high silica slags. A furnace that makes only 72 bars in five days' steady run, with 182 tons of charge, has fallen on very hard lines indeed. The bars weighed 103 to 104 pounds each, crude, and gave 99.1 pounds of soft lead.

If we confine ourselves to the 32 days for which the details are given, the amount of bullion made was 51.6 tons and of slag approximately 683 tons, containing 9.57 tons of lead, giving a total of 61.17 tons lead charged, or 8.22 per cent. on a total fused product of 744 tons; and 5.58 per cent. on a total gross charge of 1096 tons. The results obtained with such impoverished ores and a slag containing 41.8 SiO_2 indicate that under more regular conditions and with a charge that yielded matte as well as lead, the liberal use of

manganese will enable the smelter to run very siliceous slags with good results.

The worst months for bad slag, omitting those which were affected by accidents, were December, 1883, and May, 1887. Comparing these two months, we have the following:

TABLE X.—*December, 1883, and May, 1887, Compared.*

	December, 1883. Per cent.	May, 1887. Per cent.
Concentrates,	44.	28.4
Ore,	8.	22.3
Manganese,	40.	38.
Flue dust,	2.	5.
Slag,	6.	6.3
Coke,	20.6	20.6
Lead,	6.17	5.08
Slag, SiO_2,	35.2	38.3
" Pb,	4.97	2.9
" Ag,	2.91	2.5

In December, 1883, the coke was a mixture of $\frac{3}{4}$ English patent and $\frac{1}{4}$ Trinidad. Its character is not mentioned in the other month. In every respect the former month was an exception. It was the only bad month in a series of good ones, and the cause of fractiousness was never clear. The furnace ran slowly, passing only 29 tons daily. The cause was different in 1887. Then the conditions were changed. Concentrates were falling off in quantity, more ore was smelted and the whole blast (No. 10) showed an unclean slag.

The only guide to the cause of this bad work seems to be given by the proportion of lead to the fusible solids of the charge, the bad months showing a falling off in this proportion. Comparing the record before and after the bad month of 1883, we have:

TABLE XI.—*Comparison Showing Relation of Lead in Charge to Losses in Slag.*

	Lead, Per cent.	Slag-Assay.		
		SiO_2, Per cent.	Pb, Per cent.	Ag, Oz. per ton.
November, 1883	10.7	34.8	1.09	1.28
December, 1883	8.5	35.2	4.97	2.91
January, 1884	9.3	32.1	2.70	1.32
March, 1884	10.7	32.4	1.50	1.31
March to August, 1887	6.5	37.8	2.60	2.60

Of course, experience had increased by the latter year, and better work was done, but the record indicates that the practicable minimum of lead had been reached or passed.

Blasts Nos. 9 and 10 are interesting, because they exhibit the evil effects arising from a simultaneous falling off in lead contents and an increase in fusible solids. In No. 9 the percentage of bullion in the gross charge was 4.89 per cent., and in No. 10 it had fallen to only 4.65 per cent.; but, at the same time, the percentage of fusible solids had risen from 65 in No. 9 to 71 in No. 10, and the consequence was that the bullion made formed only 6.52 per cent. of the fusible solids in No. 10, instead of 7.56 per cent., as in No. 9. The lead in the slag almost doubled, rising from 1.56 in No. 9 to 2.89 in No. 10, the silver remaining nearly constant. It appears that under the conditions of Tombstone the work was necessarily bad when the bullion was less than 7.5 per cent. of the total *fused* product.

It is probable, that some of the bad features in the work done were ascribable directly to the manganese. The higher oxides of this metal give off a part of their oxygen very readily; and it is probable that the large consumption of fuel was due to the discharge of free O in the upper part of the furnace, where its combustion could do little good in comparison with the amount of coke it wasted.

On the other hand, it is well known that even unstable compounds may be stable in traces. Ammonia dissolved in water is very unstable even at ordinary temperatures; but, it is said, that a gallon of ammonia liquor boiled down to a few drops, still shows, in those drops, traces of the compound radical. So, I suppose, manganese oxides in the furnace pass through the zone of reduction without losing all their excess of O. Enough is retained and carried to the hearth to exert an oxidizing influence in the moment of fusion. In a charge that contained so little sulphur as the charge at Tombstone, it is quite possible that the small amount of matte formed was roasted while sinking through what we may call the bath of peroxidized slag in the hearth. I think there was enough sulphur in the charge to form a collectable quantity of matte under ordinary conditions, and that its total absence during the period the furnace was under my observation, was due to the liberation of oxygen in the zone of fusion, and the consequent combustion of the sulphur.

It is to this cause that I attribute the occasional increase of lead in the slag without a corresponding increase of silver. The daily

records show that this took place frequently, though the fact is obscured in the averages given in the tables. The oxidizing slag rested directly on the lead-bath, and when the reducing action failed for any reason—absence of S or of C—the lead was oxidized and PbO, poor in silver, entered the slag. A covering of matte would have prevented this loss, and I suspect that in a furnace using manganese copiously and supplied abundantly with sulphur, there would be a noticeable oxidation of the latter, and, as compared with Tombstone, a reduction in the amount of fuel.

Nos. 6 and 7, which covered 131 days, show a very marked reduction of lead lost in the slag, and also the highest expenditure of coke, except in blast No. 2. In the latter case, the early part of the run was made with American coke of atrocious quality, and the expenditure was nearly 26 per cent.; but in the latter half of the run some English coke was procured and the mixed fuel fell off to 21 per cent. In Nos. 6 and 7 this difficulty did not exist, and there appears to have been a direct exchange of about 3 pounds of coke for 1 of lead.

ALUNOGEN AND BAUXITE OF NEW MEXICO.

BY WILLIAM P. BLAKE, MILL ROCK, NEW HAVEN, CONN.

(Bridgeport Meeting, October, 1894.)

At the August meeting of the Geological Society of America, I presented a paper on the occurrence of alunogen and bauxite upon the Upper Gila river, about 40 miles north from Silver City, New Mexico, together with observations upon the geology of that region.*

Inasmuch as the subject is specially interesting to metallurgists and mining engineers, and as our *Transactions* contain recent important papers, by Dr. Hayes, M. Laur, of France and others, upon bauxite, I venture to offer to the Institute, in more condensed form, an account of the deposits described in the paper above referred to.

The only other published account of the alunogen locality which I have been able to find is a prospectus-pamphlet, issued in a small

* The general position may be seen by reference to the small map accompanying my paper on "Zinc Ore-Deposits of Southwestern New Mexico," read at the Virginia Beach Meeting, February, 1894; *Trans*, xxiv., 187.

edition in New York,* in 1892, which is rather for commercial and promoting purposes than intended to be an exact description.

In a region about half a mile square, of nearly horizontal strata of volcanic origin, there has been extensive alteration and change by solfataric action, or possibly, by the decomposition of disseminated pyrites producing aluminous solutions, which, flowing slowly by capillary movement from within outwards suffer decomposition at the surface with the production of sulphate of alumina (alunogen), in crusts and layers upon the outer portions of the rocks, attended by the deposition of siliceous crusts, and the separation of ferric sulphate; while the rocks so traversed appear to be deprived of a part, at least, of their silica and of their alkalies, with the formation of bauxite.

The alunogen is thus an outer deposit, while the bauxite is not a deposit but is an internal residual mass in place. Its color is generally bluish-white; structure amorphous, granular, without concentric or pisolitic grains. When dried in the sun and air it will still lose about 20 per cent. by ignition. It gives only about 1 per cent. of soluble matter by leaching with water; is infusible, and reacts for alumina. The amount of residual silica and alkalies has not yet been ascertained, and no careful full analysis has been made. The composition is no doubt variable in samples from different places; for the original rocks give evidence of a great difference within short distances.

Basic Volcanic Rocks.—The rocks appear to have been originally highly basic volcanic porphyries and basalts, accumulated in massive beds of brecciated fragments, the outlines of which have nearly disappeared so that the mass appears to be homogeneous. Careful observation as the rocks are freshly broken out discloses, however, the outlines of former fragments.

Origin of New Mexican and French Bauxite Similar.—This residual bauxite appears to be in its origin analogous to that of France, which is also residual, while, according to Hayes, Spencer, Branner and others, the Appalachian and Arkansas bauxites are deposits beyond the source of the alumina.

Hayes† suggests that the aluminous supply of the Alabama

* Prospectus: "The Alum Mountain Property, New Mexico," containing statements by A. T. Johnson, Dr. G. H. Sowers, analyses and a map of 60 claims (1250 acres).

† "The Geological Relations of the Southern Appalachian Bauxite Deposits," by C. Willard Hayes, Ph.D., *Trans.*, xxiv., 243.

bauxite came from underlying shales upwards through fractured strata, to be finally deposited in local accumulations above the source. He regards the formation of alum as probable by the action of ferrous sulphate of iron. It will be noted that the phenomena observed by me in New Mexico lend support to his theoretical views and tend to confirm them.

Cause of Concentric Structure Suggested.—Some of the specimens of bauxite with extraneous masses of chert partially investing them have a rude approximation to a concentric or globular arrangement, only observed, however, at the surface or near it. So also, the crystallization of alunogen, in layers, tends to form globular surfaces by the exfoliation of the rock. Is it not possible, therefore, that in some cases, at least, the globular concretionary aspect of some bauxites is due to decomposition in concentric layers?

The difficulties of access and transportation render the New Mexican bauxites commercially unavailable for the present.

NOTE ON THE TAYLOR GAS-PRODUCER PLANT AT THE ONTARIO MILL.

BY C. A. STETEFELDT, OAKLAND, CAL.

(Bridgeport Meeting, October, 1894.)

In my paper on "The Consumption of Fuel in the Taylor Gas-Producer Plants at the Aspen and Marsac Mills" (*Trans.*, xxiii., 134), I mentioned that in the plant which I had designed for the Ontario mill, the gas-tubes were to be covered with thin, corrugated sheet-iron, over which a coating of asbestos-magnesia would be spread in order to effect a good insulation and keep the gas as hot as possible.

At the Ontario mill, two Stetefeldt furnaces and two revolving driers are fired with gas. One furnace is connected with a Taylor producer of 5 feet diameter, and the other furnace and the two driers are heated by the gas from a Taylor producer of 7 feet diameter. In the latter case the gas-tubes are necessarily somewhat long, and the cleaning of them takes time. In order to prevent overheating of the tubes during that operation, it has been found advantageous not to provide them with an insulating covering. If a tube becomes red-hot at any place by the burning of soot, this is at once observed, and the tube is closed until it cools down. The tubes con-

necting the 5-foot producer with one Stetefeldt furnace need cleaning about once in three months; the system belonging to one furnace, two driers, and a 7-foot producer has to be cleaned every two weeks. This is due, according to Mr. A. D. Moffat, superintendent of the Ontario mill, to the fact that in the former case the producer is running at very low pressure, while in the latter case a much higher pressure is necessary, whereby a considerable quantity of fine ashes is carried into the tubes. Since the 5-foot producer consumes $4\frac{1}{2}$ tons, and the 7-foot producer $8\frac{1}{2}$ tons of coal per day, and, consequently, the quantities of coal consumed are proportional to the areas of the producers, it is somewhat surprising that a great difference in pressure should become necessary in operating the two producers. The difference in length of gas-tubes alone does not account for it.

The following statistics have been obtained from Mr. Moffat:

5-Foot Gas-Producer with Stetefeldt Furnace.

	Tons.
Coal consumed in twenty-four hours,	$4\frac{1}{2}$
Ore roasted in twenty-four hours,	40

7-Foot Gas-Producer with Stetefeldt Furnace and Two Revolving Driers.

	Tons.
Coal consumed in twenty-four hours,	$8\frac{1}{2}$
Ore roasted in twenty-four hours,	40
Ore dried in twenty-four hours,	80
Salt dried in twenty-four hours,	12

Formerly the total daily consumption of fuel was:

22 cords of wood at $5.75,	$126 50
Against a present consumption of 13 tons of coal at $4.75,	61 75
Daily difference in favor of gas,	$64 75

I am informed that the roasting is at present conducted at a higher temperature than was formerly the case with wood. This would make the economy of using gas still greater.

In conclusion, I would call attention to the fact that a much greater quantity of coal is used per ton of ore for roasting at the Ontario mill than at the Marsac, namely, 225 pounds in the former against 142 pounds in the latter. This is principally due to the smaller quantity of ore roasted in the Ontario furnaces, namely, 40 against 70 tons.

In drying ore and salt, both mills consume practically equal quantities of coal, namely, 87 and 86.6 pounds per ton respectively.

A UNIFORM METHOD FOR THE ASSAY OF COPPER-MATERIALS FOR GOLD AND SILVER.

BY ALBERT R. LEDOUX, NEW YORK CITY.

(Bridgeport Meeting, October, 1894.)

In Great Britain all analytical chemists are styled assayers, but in the United States a slight distinction is made, assayers being considered those analytical chemists who have chiefly to do with the determination of the precious metals.

There have grown up around the assayers of precious metals a number of customs and traditions which, as a rule, do not fetter American chemists when called upon to determine the composition or constituents of any samples submitted to them for analysis.

In Great Britain, on the other hand, trade-customs even go to the extent of enforcing the employment of an admittedly erroneous atomic weight in certain alkali-determinations. If the American chemist is asked to determine the amount of lime in a water, potash in an alkali, or silica in an ore, the owner of the sample does not presume to suggest what method he shall employ, nor does he question his report on the ground that he should employ another method.

But even in America it is different with the assayer. Unfortunately for the guild, as well as for those who have to base business operations on the reports of assayers, customs and traditions as well as divergent interests seek to circumscribe him in his choice of method and sometimes dictate how he shall perform his operations. This was true at one time of copper, which to-day in England is still determined by the so-called Cornish fire-assay, an attempt to imitate in the laboratory the various metallurgical operations in the smelting-works; but nowhere in this country is the assayer forced to employ any particular method for determining copper so long as his certificate indicates the true total percentage of copper in the sample. It is true that both buyers and sellers demand use of the electrolytic method, but only because it is the most accurate. In America the trade accepts the assayer's statement as final, and makes

its own allowances or deductions from the price asked or paid according as the material is known or supposed to be more or less free from ingredients which would render the smelting and refining more or less expensive and difficult.

But when it comes to the assay of gold- and silver-ores, or copper-mattes and bars containing gold and silver, many of the buyers insist that the assay shall be by fire, while many of the sellers endeavor to enforce special methods known to give higher results than the ordinary fire-assay. It is greatly to the credit of American assayers, as well as an indication of the fairmindedness of our business men, that there is, as yet, no such class distinction between assayers as we find in England, where some men are known as "sellers'" assayers and some as "buyers'" assayers, it being understood that each will employ a method yielding results most in accordance with the interest of the person who employs him.

In 1888, Prof. J. W. Langley, of Pittsburgh, called attention to the desirability of international standards for the analysis of iron and steel; and the result of the movement thus initiated was the general adoption of standard methods in the determination of carbon and other important elements in iron and steel.*

It is the hope of the writer that a similarly desirable result may be obtained by a discussion of the methods for the assay of copper-furnace material containing gold and silver.

In considering this question, we notice in the outset a divergence between the methods usually employed in the east and west of the United States. Most of the eastern public assayers, as well as those employed by eastern smelting-works, use what may be called a wet method, but is strictly speaking, a combination-method of assay. While there are many details incidental to different laboratories, this wet method may be outlined briefly as follows:

For Gold.—One assay-ton of the copper-borings or matte is transferred to a No. 5 beaker with a clock-glass cover. The sample is treated with a mixture of 100 c.c. of water and 50 c.c. of nitric acid of sp. gr., 1.42. When the violent action has ceased, 50 c.c. more of the nitric acid is added, and the solution is gently heated until everything soluble has been dissolved. The contents of the beaker are then raised to the boiling-point, the cover is removed, and boiling is continued until most of the free nitric acid has been expelled. The solution is then diluted with about 400 c.c. of water

* See *Trans.*, xix., 614.

free from chlorine, 5 c.c. of concentrated sulphuric acid is added, and then 10 c.c. of a concentrated solution of either acetate or nitrate of lead. The dense white precipitate of lead sulphate carries down with it the minute particles of gold which may be suspended in the solution. The precipitate is then allowed to settle for some hours—over night, if possible. It is then filtered, washed once or twice with water, the beaker is carefully cleaned, and the filter and contents now practically free from copper, are partially dried, wrapped in thin lead-foil, and transferred to scorifiers; enough test-lead is added to bring the total lead present up to 50 grammes, a pinch of borax glass is placed on top, and the scorification is conducted as usual. It is necessary to raise the temperature gradually until the paper has been consumed and the contents of the scorifier melted down. Cupellation is conducted in the usual manner.

This method is intended for the determination of gold; but enough silver will be present to allow the bead to be parted. When, however, considerable gold, say two or three ounces per ton, is supposed to be present, it is well to add a drop of salt solution to the original nitric acid solution, to precipitate some of the silver along with the lead, or else to add a small amount of pure silver at the time of scorification. It is important not to precipitate all the silver, as in that case, there might be an excess of salt which might liberate chlorine and vitiate the results as to gold.

For Silver.—The usual method employed in the east for the assay of copper bars, mattes, ores, etc., containing silver is likewise modified in different laboratories. These modifications vary, as a rule, with the supposed richness in silver of the sample treated. The sample is dissolved in dilute nitric acid, as described in the above method for gold. To the solution, after the addition of sulphuric acid and before that of lead acetate, a solution of chloride of sodium is added in a sufficient quantity to throw down all the silver, the addition being gradual, and avoiding a great excess (as silver chloride is more or less soluble in sodium chloride solution); then the lead acetate is added, the solution is well-stirred, and the mixed precipitate of lead sulphate and silver chloride is allowed to settle as in the gold determination. The rest of the process is conducted exactly as in the previous case for gold. Where any considerable amount of gold is present it is of course necessary to part the beads and deduct the weight of gold present, which otherwise would be weighed as silver, thus erroneously increasing the proportion of this metal. The gold obtained by this parting is usually less than the

figures obtained by the special assay for gold, because some of the gold is dissolved by chlorine through the excess of sodium chloride employed.

Some assayers determine the gold and silver at one operation by taking the filtrates from the gold and lead sulphate precipitate obtained as above described, precipitating the silver in this solution as chloride, adding more lead acetate, and after filtering combining the two filter papers, one containing the gold and the other the silver, and uniting them for one scorification and subsequent cupellation. This method is more economical for the assayer, and has the advantage also of two filtrations for gold, catching any fine particles which might pass through the first filter; but on the other hand it takes more time, because the same solution is twice settled. In the first method, the settling of the gold and silver precipitates goes on simultaneously.

In the West, the all-fire method is employed almost exclusively, so far as I can ascertain. In the Omaha and Grant works, for example, ten portions of sample, of one-tenth A. T. each, are weighed out and scorified with 50 grammes of test-lead, one-half of which lead is mixed with the sample and the remainder used to cover it in the scorifier. One gramme of borax is added. The lead buttons obtained by the scorification are cupelled separately, but the ten beads are weighed together. The cupels are then ground up and fused in five lots of two each, with the following charge: Litharge, 90; soda, 50; borax-glass, 50, and argol, 3 grammes. The five buttons are cupelled, and the silver is added to that obtained in the first operation, representing the loss in scorification. All the beads are then parted for gold, which is deducted from the total weight as usual.

My experience shows that the determination of gold obtained by this process is usually higher than where the wet process previously described is employed. It may be well to give certain instances in my own experience. On high-grade copper bullion, which contains on an average about 400 ounces of silver per ton, the results were:

	Fire assay.	Combination wet-and-dry assay.
Gold, ounces per ton,	1.06	0.92
" "	1.32	1.24
" "	0.34	0.20

In bullion containing 300 ounces of silver per ton:

	Fire assay.	Combination wet-and-dry assay.
Gold, ounces per ton,	4.06	3.96
" "	2.76	2.56
" "	2.72	2.44

In matte containing 60 per cent. of copper and 60 ounces of silver:

	Fire assay.	Combination wet-and-dry assay.
Gold, ounces per ton,	0.24	0.20

The two processes usually agree very closely for silver, provided the cupel-absorption is determined, when the silver is assayed by the combination wet process. This cupel-absorption is very much less by the wet process than by the all-fire method, because by the former the copper has been eliminated, and is not present to help carry the silver into the cupel. In some instances, where substances are present which would cause volatilization of silver in scorification, the wet assay gives higher figures, because the interfering substance has been removed by the acid.

The Western all-fire process for mattes is similar to that employed for bars, except that a second scorification is sometimes necessary before cupellation. The second scorification is usually performed in a small $2\frac{1}{2}$-inch scorifier, enough test-lead being added to the button obtained from the first scorification to make the lead present not less than 35 grammes.

The above descriptions, as will readily be seen, are in the baldest outline; and it must not be inferred by those interested that all precautions are not adopted to make the results correct; such, for instance, as igniting and dissolving any sulphur-balls which may form when the matte or sulphuret-ores are dissolved in acid, and adding the product to the main solution before precipitating the silver with lead. This precaution is hardly necessary, however, as the very small amount of matte or ore held by the sulphur would be decomposed in the scorification.

Each of these methods in the hands of assayers skilled in its application, will produce very uniform results; and yet, as will be seen from the few comparisons given above, any assayer running the two, side by side, will get divergent figures for gold.

The subject of determining gold and silver in copper-furnace material has, of course, received careful attention from all assayers charged with such duties, and there are many excellent papers on this subject, notably, one by Professor Cabell Whitehead, Assayer

to the Mint in Washington, which is published in vol. vi. of the *Journal of Analytical Chemistry*, p. 262, etc. Professor Whitehead recommends a combination method, in which the silver is precipitated by sodium bromide instead of chloride, on account of the greater insolubility of silver bromide. He says, however, that in his experience, it is better to precipitate the lead also as bromide, rather than sulphate. I will not give his method in detail, as the object of this paper is not to discuss the merits of various methods proposed, but to draw out suggestions from other assayers interested, as I am, in the adoption of a uniform standard method that will be as accurate as possible on the one hand, and will secure on the other hand, the confidence of buyers and sellers, whether west or east of the Mississippi.

The problem is complicated at best, and especially so when mattes and ores are likely to contain zinc, arsenic, nickel, etc., as well as copper and iron.

Western practice has very naturally adopted the all-fire method as an outcome of the first requirements of lead-silver assaying before copper was produced to any great extent west of the Mississippi, or had become, as it is to-day, an important commercial vehicle for gold and silver.

The wet method, on the other hand, was naturally adopted in the East, where at first analytical chemists were infrequently called upon to assay materials containing the precious metals, but were in general employed rather upon chemicals, waters, iron-ores and other substances, to which a wet method only is applicable.

The difficulty, also, of getting correct results from mattes and ores containing zinc or other metal which tends to aid the volatilization of silver has had much to do with the adoption of the wet method. This method is used by many of the copper-refineries in Montana and other western points, as well as in the East.

In my own laboratory we are constantly called upon to mediate between producers in the West and buyers in the East, or in Europe, and, like all assayers, we stand between two fires; but, to the credit of the metal business, it is pleasant to record the fact that very rarely has any one ever suggested to us that we should employ a method which would give higher or lower results than the actual amount of gold and silver present in the material assayed.

I have purposely refrained from stating the method employed in our own laboratory. This will be fully described at the proper time, in connection with the plan suggested below.

It is my hope that when satisfied as to accuracy the trade will recognize the desirability of applying to the assay of gold and silver the principles which now prevail in the copper-trade when assaying for the latter metal; that is, that the assayer shall be asked only how much total gold and silver are present, not how much he could produce by applying different methods of assay.

Whatever method is adopted, there are many incidental points which need to be considered, especially that of temperature in the furnace. It is always quite possible by any method to obtain very different results, if the cupellation is conducted at the maximum temperature, on the one hand, or at so low a temperature, on the other, as barely to escape freezing. Unless some standard precautions are taken to remove this difficulty, it will not be so much a question of cupel-absorption as of volatilization. In the East, where gas-furnaces are principally employed, it is, of course, much easier to maintain uniform temperatures in the furnace.

Without discussing the subject further in this preliminary paper, I venture to propose a plan of co-operative action for members of the Institute engaged in this business, with the view of arriving at an ultimate agreement upon a standard method. I am aware that the Institute as a body cannot officially endorse any particular theory, method or opinion. It is only a forum for free discussion. But its value as an organized medium for such an interchange of views and experiences as will result in substantial technical progress is unquestionable, and such an interchange is beyond doubt the necessary preliminary to any professional *consensus* of opinion, however authenticated.

As the first step in such a discussion, I would propose that a number of samples of a gold- and silver-bearing copper-material should be distributed to as many assayers as are willing to join in a "symposium;" each assayer to send his results, together with a minute description of the method employed and of any circumstances possibly affecting the results announced, to the Secretary of the Institute for publication *without the names of the assayers or works.* The results thus published could not be used to affect the commercial interests or the professional standing of any one, and would be available as a basis for general, thorough and really profitable discussion.

I wish, by this plan, to place myself upon an equal footing with all others who may co-operate in the proposed comparative test of methods, and have, therefore, suggested that the parties shall report to the Secretary, and not to me. I will cheerfully undertake to pre-

pare the necessary samples and to furnish a description of the method of preparation.

It gives me pleasure to announce that the Omaha and Grant Smelting and Refining Company and the Baltimore Copper Smelting and Refining Company have signified their willingness to participate in this undertaking, and I have no doubt that many others will be glad to lend their aid.

THE PHOSPHATES OF TENNESSEE.

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(Substantially a portion of a Post-Graduate Thesis, presented for the degree of C.E., at Vanderbilt University.)

(Bridgeport Meeting, October, 1894.)

I.—Historical.

The rock now known to almost every Middle Tennessee farmer as "phosphate," was but recently recognized as such. The existence of a stratum of black siliceous rock in the hills surrounding the Nashville Basin has been known for many years to Dr. Safford, the State geologist. Nothing about it, however, would lead to the conclusion that it contains a considerable amount of bone phosphate of lime. The only feature that might attract attention is its very dark color; and since it occurs near the "black slate," one would naturally infer nothing more of interest to be connected with that feature than that this rock and the "black slate" derived their carbonaceous matter from a common source.

Many of the country people have mistaken it for coal, or for an indication of coal; this impression being strengthened by the fact that, when a piece of the rock was thrown into a blacksmith's forge, a slight combustion was observed.

As will be more fully explained, the phosphate-rock is of two kinds: (*a*) loose or separate boulders, occurring in a stratum of clay or blue shale; and (*b*) solid rock occurring in a continuous stratum. The boulders have been known for a long time as "balls," "kidneys," "stink-stones," etc.; and as early as 1887, an analysis made under the direction of Dr. Safford, at Vanderbilt University, showed one of these boulders to contain 64 per cent. of bone-phosphate of lime. This information awakened no great interest in the matter, for the probable reason that at the same time it was known, or supposed to be known, that the boulder-beds were of limited extent as

well as thickness, which rendered their occurrence insignificant as a source of fertilizing-material.

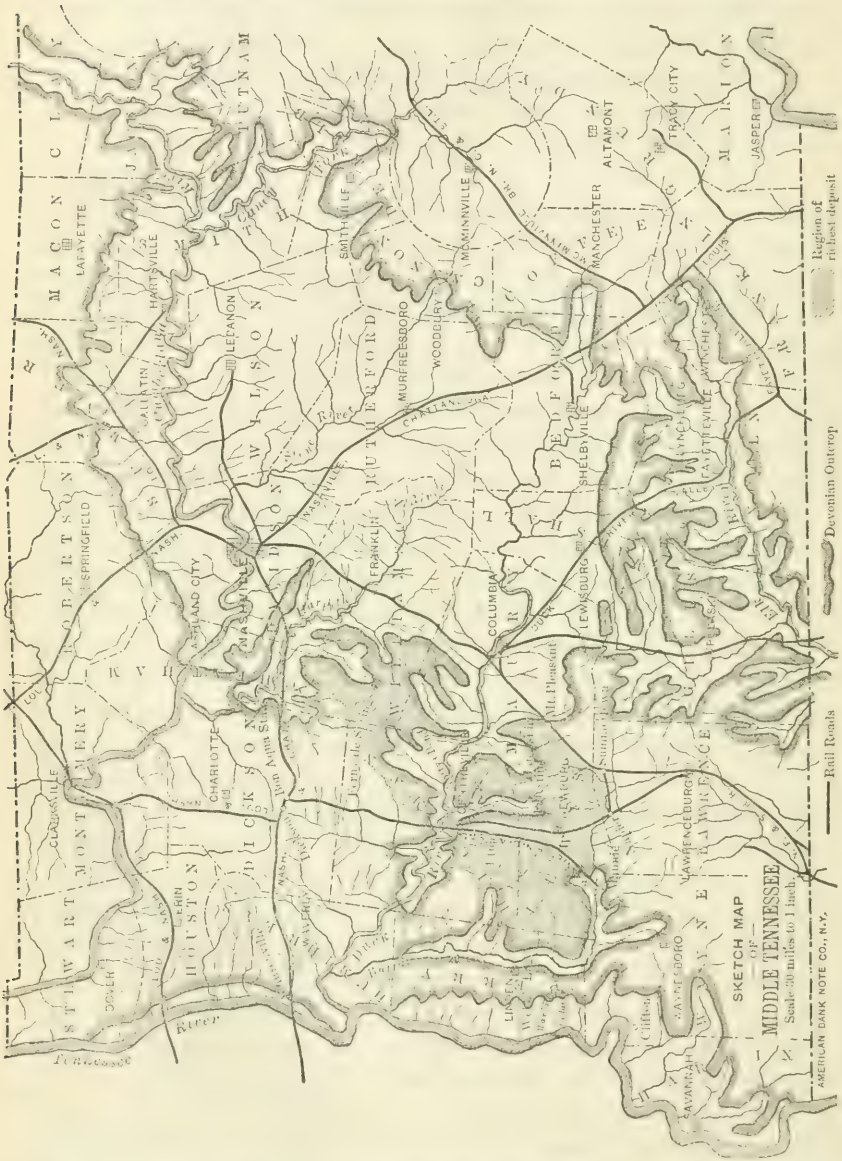
In the fall of 1893, however, Messrs. Craik and Arnold, of Columbia, found, near that place, beds of boulders, reported to be about 1 foot 6 inches in thickness, and sent samples to the laboratory at Vanderbilt for analysis, which showed 65 per cent of bone-phosphate. Leases were immediately taken on land containing the nodules, and some talk of "phosphates" followed.

The solid rock-phosphate beds, as yet not thought of, were discovered soon after, probably more by accident than by reason of any impetus given to prospecting through the knowledge of the nodules. In November, 1893, Mr. R. W. Childs, of Linden, Tenn., sent to Nashville some samples of rocks, among which was the black rock which had been mistaken by so many, Mr. Childs among the number, for coal, or rather a sign of coal. An analysis of this sample by Mr. J. C. Wharton revealed the presence of phosphoric acid in considerable quantity, and led the way to more conclusive work, which showed that some of the samples yielded 75 per cent. of bone-phosphate of lime. Since that time much careless prospecting has been done, and many acres of land have been "optioned," leased, or even bought, for the most part with a view to speculation. Many false reports as to thickness and extent of strata have been published in the newspapers, arising, no doubt, from the fact that the phosphate-stratum is in juxtaposition with the "black slate" of this region and has been confused with the latter, by reason of a similarity in color and appearance, misleading to a casual observer.

The regularity with which the stratum of phosphate-rock occurs in respect to other rocks of this region has rendered prospecting a simple and easy matter, particularly when aided by the geological map of the State prepared by Dr. Safford, which shows with surprising accuracy, so far as experience has gone, the outcrop of the "black slate." But much experience as regards the chemical composition of the material is required for judging at sight of the value of the rock; and it is through lack of such experience that many very favorable reports of various localities are untrustworthy, unless the prospector's views are supported by analyses.

Not sufficient time has elapsed for any organized effort to be made in mining; but enough of the rock has been taken out to afford a manufacturer's test. This was made by the Nashville Fertilizer Company, a firm working heretofore with imported raw material. The test was very satisfactory, with both nodules and solid rock-samples. The former gave a product which dried rapidly and gave 15 per cent.

of available phosphoric anhydride; the drying of the solid rock was not so thorough, while it afforded 13 per cent. of available phosphate. Of a sample of the solid rock sent to the Pratt laboratory, Mr. Pratt



says: "From the small amount sent us, we made here in this laboratory a small quantity of acid phosphate, which quickly set and dried out, leaving a fine friable and beautiful acid phosphate, fully up in appearance and working-qualities to any we have ever seen."

II.—Geographical.

The locality of the phosphate-deposits of Tennessee is shown on the accompanying sketch-map. Not quite all of the region indicated on the map has been fully explored; but wherever the Devonian outcrops have been examined, more or less phosphate has been discovered. The heavy black line on the map represents the line of Devonian outcrop, and the hatching indicates the direction in which the Devonian and phosphate-beds extend. This map indicates that the phosphate-area in the State is very large; but not all the indicated area contains phosphate-rock thick and rich enough to work. In some places the phosphate-layer is rich enough to work, if it were only thick enough to pay for mining; but more generally the thin beds are also poorer in bone-phosphate than the thicker ones. To give some idea of the geographical distribution, we will take up each of the counties in the phosphate-district separately.

Hickman County.—Here we seem to have the best display of phosphate that has yet been discovered. At Totty's Bend the stratum of phosphatic material is 54 inches thick, and the analyses show an average of between 70 and 80 per cent. of bone-phosphate throughout a thickness of 40 inches. The remaining 14 inches is variable in bone phosphate, some parts not containing more than 30 per cent. At no other point, even in this county, has such a thickness been discovered. The stratum of phosphate-rock outcrops in Swan creek bottom for nearly its entire length, and, in nearly every place so far opened, is found to be thick enough and rich enough to warrant exploitation. On Fall Branch, a tributary of Swan creek, the mean of five analyses shows 71 per cent. of bone-phosphate, and the average thickness was between 2 feet 6 inches and 3 feet. The outcrop on Nunn's place is likewise very rich. Further up Swan creek, at the mouth of Indian creek, the phosphate is very sandy and contains not more than 40 per cent. of bone-phosphate. In some places we find from 6 inches to a foot of phosphatic nodules above the "black shale;" and this is usually the case in Hickman county. The following are the results of some analyses:

	Thickness. Feet.	Inches.	Bone Phosphate. Per cent.
On Fall Branch,	2	9	63.98
Nunnelee's, on Indian Creek,	3		38.39
Nunn's Place,	3		66.54
Nunnelee's Place,	3		32.45
Nunnelee's Place,	3		58.51
On Fall Branch,	3		70.11
Mouth of Swan Creek,	1	8	60.41

Lewis County.—Just above the junction of Little Swan and Big Swan creeks, on the main creek, we find on Mr. Mayfield's and Mr. Weatherly's farms the outcrop of a very rich stratum of phosphate which yields more than 70 per cent. of bone-phosphate on analysis. There are other places on Swan creek above Mayfield's that have valuable phosphate; but it seems that they all show more pyrites and sand than the samples from Mayfield's farm. The average of eight samples taken from different openings on that place by Dr. Meninger are reported to have given 77 per cent. of bone-phosphate. This region is, without doubt, second only to that of Totty's Bend. Lewis county lies mostly too high to permit the expectation of very extended outcrops of phosphate-rock, and we fail to find it anywhere in this county except in Swan creek bottom. Doubtless the bed of phosphate extends under nearly the whole of the county; but it can only be mined from the outcrop. Some analyses follow:

	Thickness. Feet.	Inches.	Bone Phosphate. Per cent.
Mayfield's Place, ...	3		71.92
Mayfield's Place, ...	3		70.06
Weatherly's Place, ...	3		68.00
Napier Iron Co.'s Land, ...	1	8	40.09

Perry County.—High up in the hills, six or eight miles back from the Tennessee river, an outcrop of phosphate, 2 feet thick, may be seen at the headwaters of Cedar, Marsh or Crooked creeks, or in fact, almost any of the little streams flowing into the river. Analyses do not show, as a rule, more than 50 per cent. of bone-phosphate in the samples from this section. The same stratum may be seen outcropping with about the same thickness and richness in the Buffalo river-bottom, in the eastern part of the county.

Wayne County.—Along Buffalo river, as in Perry county, there is an outcrop of about 2 feet of phosphate-rock which does not run above 50 per cent. in bone-phosphate.

Lawrence County.—The entire county lies too high for outcrops of phosphate, except in the extreme southeastern corner, where, near the Giles county line, we have observed a one-foot stratum of 50 per cent. phosphate.

Giles County.—Along the western part of Giles county, at the proper geological horizon, may be found outcropping a one-foot stratum of phosphate. Analyses gave: (No. 1) 49.4 per cent. and (No. 2) 51.42 per cent. of bone-phosphate.

Sample No. 2 was taken from a locality about ten miles north of

No. 1, on Gimlet creek. No explorations of importance have been made in the eastern part of Giles county; but certainly not more than one foot of phosphate is to be expected there.

Maury County.—The phosphate-rock in the western part of Maury county is apparently the same stratum as outcrops in Swan creek bottom, on the other side of the ridge. The thickness of the phosphatic material here is 3 feet, with a layer of nodules above the black slate about 10 inches thick. This region has not yet been thoroughly explored, but some analyses have been made which show as much as 65 per cent. of bone-phosphate. As in other localities, however, some of it is sandy, yielding not more than 30 per cent. This locality seems to be worth exploring, as it is just across the ridge from Swan creek, and should be expected to contain valuable phosphate in some places. Analyses show:

	Thickness. Feet.	Bone Phosphate.
On Hughes's place,	3	65.94
" "	3	60.28
" "	3	30.73
" "	3	31.30
" "	2½	20.40

Cheatham County.—The phosphate-outcrops here are very poor in quality and not more than eighteen inches thick. Analysis from samples on Landreth's place show (No. 1) 7.51 and (No. 2) 7.54 per cent. of bone-phosphate.

Williamson County.—The western part of this county contains much good phosphate. At Fernvale Springs it is three feet thick, and analyses show it to be nearly as good as the average in Swan creek bottom. Between Fernvale and the mouth of Swan creek much good phosphate should be expected; but that region has not been very thoroughly explored. Analyses show:

	Bone Phosphate Per cent.
Sandy specimen,	53.14
Blue "	64.40
Analysis of Nodules.	**Per cent.**
Average No. 1,	66.05
" No. 2,	64.36

Some samples run above 70 per cent. of bone-phosphate.

Other Counties.—The counties east of the Louisville and Nashville R. R. that would be expected to show phosphate-outcrops have

not all been explored; but where investigations have been made, not more than 6 or 8 inches of phosphate has been found. The stratum is remarkably persistent, however, and has been observed even in the region around Chattanooga.

In Davidson county, out on Paradise ridge, we seem to have the northern limit of phosphate-outcrops. Here the phosphate-bed is only 6 inches thick and very sandy. One analysis gave 30.15 per cent. of bone-phosphate. The sample came from Baker's station; but about the same formation has been observed on White's Creek.

It appears from the foregoing, that the only region likely to be of importance is comprised within the counties of Williamson, Hickman, Maury, Lewis and Perry, the richest deposits being in Hickman. It apparently fades out gradually in thickness and richness from an axis running NE. and SW. through Williamson, Hickman and Perry counties.

In Swan creek bottom, and also near Fernvale springs, a second layer of phosphate 2 feet thick has been discovered about 40 or 50 feet below the black shale, lying between two strata of limestone. So far, this second layer has not been found of sufficient thickness to be of importance, but it has not yet been studied much. It gave 37 per cent. of bone-phosphate as a mean of two analyses.

III.—Geological.

The geology of Middle Tennessee is of a simple nature, and has been investigated and worked over very thoroughly. The rocks are all of sedimentary origin, and offer no field for the perplexing speculation sometimes indulged concerning the classification, origin, etc., of the crystalline and metamorphic rocks.

The lowest rocks of the Nashville Basin and of Middle Tennessee belong to the Trenton series of the Lower Silurian, and above them, in various parts of the phosphate-field, occur sometimes the Niagara and Lower Helderberg series of the Upper Silurian. As a general statement, we may say that the Trenton rocks underlie the phosphate in the western part of the region, which is nearer the Tennessee river, while towards the east they give place to the Niagara rocks. The lower Helderberg limestone occurs under the phosphate stratum at Linden, in Perry county. The rocks which underlie the phosphate-group are always limestone, of one or the other series mentioned above.

The phosphate-bearing stratum itself forms a part of the thin representative of the Devonian system in this section. The black

shale, locally called "black slate," which overlies the phosphate, generally completes the Devonian as it appears here; but in the counties near the Tennessee river we have seen, underlying the phosphate, a stratum of sandstone 3 or 4 feet thick, which also doubtless belongs to the Devonian.

Just above the black shale begins the Sub-carboniferous series, which presents, first, a stratum of blue shale, sometimes as much as 200 feet thick, and contains the stratum of "nodules;" then follows a stratum of chert, sometimes broken by a layer of cherty limestone, known as "burr-stone." The tops of the hills are usually capped with rotten and broken-up chert. The thickness of rock above the black slate depends of course upon the height of the hills and on the elevation which this shale occupies in them.

An account of the Devonian shale and a "bituminous sandstone" which underlies it, written by Dr. Safford, some thirty-five years ago, is very interesting, and shows how early the existence of both the nodules and the phosphate-stratum proper (which was thought to be a bituminous sandstone) was known.

"West of the Cumberland table-land in most of the counties the formation does not consist alone of the characteristic black shale. The latter has at its top a thin layer of argillaceous, very fetid, concretionary bodies, which I have sometimes called 'kidneys,' a name which has already been used with reference to them. These bodies are round, oval, kidney-shaped, and usually more or less flattened. They vary in size from that of a peach-stone to thick, cake-like masses two feet across, and occur packed, often closely, with blue shale, in a layer from 2 to 12 inches thick. The kidneys vary in size with the thickness of the layer.

"In addition to the 'kidneys' at its top, the shale has, immediately below it, very generally a dark gray, bituminous, fetid sandstone, usually fine-grained and from a few inches to 15 feet in thickness. In Wayne, Hardin, and in the southern part of Lewis, this sandstone is often thick enough to be quite conspicuous, forming ledges along the slopes of the hills and sometimes small local plateaus.

"North and east of Wayne and Lewis the sandstone below the black shales is not so thick; in fact, it very often measures but the fraction of a foot."

This supposed sandstone is what has turned out to be our Tennessee phosphate. In many localities it is in reality a true sandstone, in which the cementing material is mostly calcium phosphate. But where the sand disappears wholly, or in great part, we have calcium phosphate to the amount of from 40 to 75 per cent.

The thickness of both the phosphate-stratum and the black shale varies considerably within the limits which have been designated as the phosphate-field. The thickest phosphate-rock that has yet been observed is near Centreville in Hickman county, where it attains

a thickness of 54 inches. In the Swan creek valley the stratum varies from 3 to 2 feet through Lewis and into Hickman county. At Fernvale springs, Williamson county, there is a depth of 3 feet; in Giles and Lawrence it is 1 foot thick; in Perry, 2 feet; in Maury, 3 feet; and in Cheatham, 1 foot 6 inches. Near Centreville it has been reported that below the phosphate-stratum proper there exists another stratum in the limestone, which is 2 feet thick. Analyses of this rock give about 36 per cent. of calcium phosphate. This occurrence is very puzzling, since it is wholly disconnected with that above, and belongs to a different formation.

Physical Characteristics of the Phosphate-Rock.—About the nodules there is not much to be said. The smaller ones approximate to spherical shape, some of them being about the size of a marble and almost perfectly spherical. The largest, however, are spread out or flattened laterally, and rounded only on the edges—a shape which calls to mind pressure in a vertical direction, which, during the formation of the nodule, limited its growth. The interior of a nodule freshly broken presents a fine-grained structure, and a black or dull brown color, while the broken edges are clean and sharp. They weather to a color much lighter than the natural, and when subjected long to the action of the weather, as those which one would pick up at the outcrop or in an old field, the fractured surface shows a banded appearance, the lightest bands being near the surface and parallel to it. We have never noticed any fossil remains in any of the balls, or any nucleus about which concretion may have taken place; but it is difficult to account for their origin as other than concretionary bodies. No pyrites has been seen in a nodule; but some of them, on being struck with a hammer, separate easily along certain planes which show the rusty dye of iron oxide.

The greatest thickness of nodule-stratum we have yet seen was not over one foot; yet we have reason to believe that in some localities the stratum is several feet thick. As an exception to the general rule, two strata of nodules may occur, one above the black shale, the other below the solid phosphate-rock. An example of this may be seen on Indian creek, Hickman county. As already remarked, this is exceptional and strictly local; and about this, as also about the second or lower stratum of phosphate-rock which occurs at Centreville, we can only say in explanation, that they are mere fore-runners of what occurred to a much greater extent in a later age.

The stone of the solid stratum varies somewhat in appearance and constitution. As a rule, however, we may say that it is of a dark

color, sometimes quite black, dark gray, brown and dark green ; the green being caused by the copperas resulting from decomposing pyrites. It is granular and tough, although easily bruised, and of a specific gravity of 2.80 to 2.85, which gives a weight per cubic foot of about 175 pounds.

The texture of some of the stone has the appearance of oölitic limestone, being largely composed of very small rounded grains, among which often occurs a tiny spiral shell, smooth and consisting of two turns. This shell can be perceived with the naked eye, and conveniently studied with the aid of the pocket-microscope, when it closely resembles the *Spirorbis amphilodes* (Dana) characteristic of the Devonian. The whole of the animal, shell included, has been replaced by the black rock-material, having left simply its form behind. To explain the little grain-like bodies is hardly possible on any hypothesis as to organic origin ; since, if an organic structure was ever present, it has been lost in the replacement which has since taken place. It has been observed that the interior of one of these little bodies appears very strongly to be made up of concentric rings, which would lead to the conclusion that they are of concretionary origin.

Another shell which also occurs is undoubtedly a species of *Lingula*. It is very small, and approximately elliptical, yet more acute at the junction of the valves. The marking on the outside of the shell is in curves running parallel to the contour, such curves as would be cut on the surface of the shell by planes parallel to that which separates the valves. Guided only by the size, and to some extent by the form, we believe the shell to be the *Lingula spatulata* (Hall); it is not quite so sharp at the apex as that shell, however, but resembles more in form the *Lingula ligea* (Hall). But the latter is much too large to be the same as ours. They occur in great abundance in some specimens, the little shells shining probably as bright and as perfect as when their owners were alive. In all samples observed which contain this shell, there is a large amount of sand, and the small percentage of calcium phosphate doubtless comes largely from the *Lingulæ* themselves, as they are among the few shell-fish whose shells are phosphatic.

When not much siliceous matter is present, the phosphate-rock weathers to a rotten stone of brown color, the weathered portion being thin. When siliceous matter is present, the weathered portion has all the appearance and characteristics of friable sandstone. The appearance of the weathered stone is one clue to its richness;

but after one has become familiar with the different grades it is an easy matter to distinguish what is valuable with a considerable degree of certainty.

Origin.—The origin of this stratum of phosphate-rock is, of course, merely a matter of surmise, starting from the fact that it was formed as a sedimentary deposit in Devonian times, and therefore from Devonian seas. This age embraces that geological time in which plants first made their appearance to any considerable extent; and it is to the presence of the organic matter contained in this plant-life that the black color of the phosphate-rock and the "black slate" is due. In the "black slate" the fossil remains of plants of the *Lycopodium* family can be seen; but strangely enough, as it seems, none have been discovered in the phosphate. The Devonian age, however, was pre-eminently the age of fishes; and we may say that the world's supply of phosphoric acid was, to a large extent, possessed by the denizens of the sea. The sediment from such water must have been pregnant with the remains of thousands of species of sea-animals. The region of middle Tennessee probably afforded a place where sediment was particularly subject to be carried by the ocean-currents or waves, or was perhaps washed ashore by the warm currents of an ancient Gulf Stream, and thereby invited the presence of swarms of sea-life, successive generations of which left their remains in the mud of the bottom. It is proper to speak here of the pyrites which occurs in almost all of the phosphate of this kind, and in some samples very abundantly. The "black slate" also contains it. In both instances its origin was evidently a precipitation, from solution, by the hydrogen sulphide evolved from the decaying mass of mud which originally constituted our phosphate-bed.

IV.—Industrial and Economical.

In mining the phosphate-beds, the methods of coal-mining by tunnels will doubtless be followed. Since the black shale above the phosphate-stratum can be taken out much more easily and cheaply than the limestone below it, the shale will be taken out for head-room, arched overhead, and the phosphate will be stripped up from the floor formed by the limestone stratum below. This removal of dead material we deem to be unavoidable when the phosphate-layer is less than 3 feet thick, since at least 3 feet will be required for working-space. Coal and some iron-ore has been worked with only 18 inches removal, but the ease of working in these cases is very

different from what will be found true of phosphate-mining when it is begun in this section.

The power to be used in mining operations will doubtless be derived from steam; and if extensive tracts are mined by single companies, the transmission of power by electricity from a central station will be found convenient and economical. So far as we know, there is no promising site for obtaining water-power in proximity to the richest deposits. The streams large enough to furnish sufficient water have no places of natural fall in the locality; and the expensive creation of artificial fall by dams would scarcely be warranted, in view of the unstable nature of the industry in a given locality.

The wide difference in the manner of occurrence of the deposits of Florida and South Carolina, as compared with those of Tennessee, will involve a corresponding difference in the methods of industrial development. The advantages and disadvantages in the two cases are perhaps about evenly balanced. The Florida and South Carolina deposits are beneath alluvium, and therefore tunnelling is out of the question; yet, when the overlying formation is thin, the stripping process is as cheap as it would be to tunnel in hard rock. This last remark is almost universally true of the "land-pebble" of South Carolina, the deposits of which have the excellent feature of uniformity both in thickness and depth below the surface. The Florida rock crops out on the surface, often in great thickness, but the dip generally carries it down rapidly; so that, on account of the excessive overburden, such mines yield enormously at first and less afterwards. The advantages possessed by the Tennessee layers are their horizontal position, their uniformity in thickness and quality, and the cleanness of the product that will come directly from the mines, rendering unnecessary the costly washing- and screening-processes which the product of our neighbors must undergo.

In the thesis from which this paper is condensed, we have ventured some estimates of the cost of mining the Tennessee rocks. Being hypothetical in character, as they must needs be in the absence of any actual experience yet obtained, they are omitted here as less appropriate to the *Transactions* of the Institute than will be the records of practice when these shall have become available. It may, however, be remarked that upon certain assumptions, believed to be safe, we have estimated the cost of mining at $3 per cubic yard as a liberal figure, which would make the cost of phosphate-rock mined from a 3-foot stratum $1.50 per ton. Assuming the low selling-

price of $3 per ton at the mine, our figures give a profit for all thicknesses of good phosphate-rock above 1 foot 6 inches, which may be roughly set as the limit of profit, since at or a little below that point the cost of excavating head-room in the barren rock will consume the gain from the phosphate. But these estimates are very general in character, and would be greatly modified by local conditions, as well as by market-changes. We think, however, that we have taken the costs high and the profits low.

As to the price of $3 per ton at the mines, we have adopted that figure, although the present price in Tennessee is $8 per ton, because we believe that the result of industrial development on a large scale would be to reduce the market-price, as it has done in South Carolina and Florida.

EARLY DAYS OF THE IRON MANUFACTURE.

BY JOHN FRITZ, BETHLEHEM, PA.

(Presidential Address at the Bridgeport Meeting, October, 1894.)

Gentlemen of the American Institute of Mining Engineers:

I desire to thank you sincerely for the distinguished honor you have conferred upon me by electing me to the presidency of this society, a position which in the past has been filled by some of our most distinguished and ablest members, men eminently qualified by education, practical training and a business ability which has enabled them not only to fill the position with credit to themselves, but as well with honor to the society.

In deference to a custom long established, by which the president-elect is expected to read a paper on some subject with which he is more or less familiar, I have thought that a brief review of the methods employed in the manufacture of iron, as witnessed by myself, and reaching back to 1838, would interest some of our older members and give our younger ones a glimpse into the trials and difficulties encountered in those early days by the pioneers in this great industry.

In 1840, 12 to 20 tons of pig-iron was the make of a furnace per week—at this time from 1200 to 2000 gross tons.

In 1840, 3 to 4 tons No. 4 wire rods per turn. They have lately made 176 gross tons in 11 hours.

In 1840 I have not the quantity of puddled iron made; but it was

small, as puddling was in its infancy. In 1890, there was made 2,518,174 gross tons.

The earliest rolling-mill statistics are in 1856, in which we produced a total of 498,080 gross tons of all kinds of rolled iron.

In 1840, we produced of pig-iron 286,903 gross tons; in 1890, 9,202,703 gross tons, which is more than has been produced by any other nation.

The incidents of which I shall make mention were of such an everyday practical character that they never have found their way into print.

My first practical experience in iron-making was in 1838, while a cub apprentice in a country machine- and blacksmith-shop, when I was sent out to a charcoal-furnace to do some repair work. The furnace was blown by water-power, the motor being an under-shot wheel having a wooden shaft, in the ends of which were secured cast-iron winged gudgeons, one of which had a crank cast on it, in which the crank-pin was inserted that drove the connecting-rod and piston in the wooden blowing-tubs, as they were called. The dimensions of the furnace are unknown, but it made about 12 tons of iron per week when it worked well; and when it did not work well, which was often the case, it made none. The particular job I was sent to do was to put in a new blast-pipe connecting the main blast-pipe with the tuyere of the furnace; and when I got it up in place, to my chagrin, I found it did not reach the opening in the stack by about 10 inches, the conclusion being that some one had made a big blunder, something that happened sometimes, even in those days; and the supposition was that it must have been the man who made the pipe, as the workmen in those days took their own measurements, and in case of a mistake they generally got what we called a "blowing up," and if the error was made by a cub, he got an especially rough one. While I was thinking the matter over, and wondering if I had better take the pipe back to the shop several miles away, or move the furnace up to meet it, the man who ran the furnace, or founder, as he was called, came along, and his appearance just at this juncture was not a pleasant one for me, as I expected that when he saw that the pipe did not reach the tuyere he would give me a blast, and a hot one as well. To my surprise, he looked at it for a moment, and said it was just right. But while this let me out, I could not but think it ought to have reached to the tuyere. I afterwards learned that the connection was made with a leather pipe called a boot. I still thought they ought to go into the tuyere.

In the neighborhood where I spent my younger days there were several mills for rolling boiler-plate, and as a boy I spent much time in watching what at that time was an interesting sight to me. While there were several mills there, the one I propose to speak of was the oldest, and, as it is claimed, the first mill in the United States to roll plates to make boilers, it then having the reputation of making very superior plates, and, I am glad to say, it still sustains its early acquired fame. As a history of this mill reaches back more than fifty years from the time I first knew it, my knowledge of its beginning was obtained from the old people who lived in the neighborhood and from some of the old workmen who had been employed in it, and I hope what they told me will interest you, as it did me to hear it.

In 1790, Isaac Pennock, the great-grandfather of the present proprietors of the Lukens Iron and Steel Company, began the manufacture of iron at a place on Buck Run, Chester county, Pa., called Rokeby, about four miles south of Coatesville. Isaac Pennock was raised as a farmer in the neighborhood, and his parents strongly objected to his going into a business about which he knew nothing, as they felt he would squander his money. The mill he first built was called the Federal Slitting Mill. In 1810, he bought a saw-mill property on the Brandywine, near Coatesville, which he converted into an iron-mill. This mill, which at the time was called the Brandywine, has since developed into the immense plant it now is.

In 1816, Dr. Charles Lukens, a son-in-law of Isaac Pennock, came into possession of the property, and carried on the business of iron-making until his death, which occurred in 1825; and it is claimed that it was between these dates that the first boiler-plates were made in this country and in this mill. At the death of Dr. Lukens, his widow, in accordance with his special request, continued to carry on the business, although handicapped by the fact that there were no railroads in those days and the finished iron had to be teamed to Philadelphia, a distance of 36 miles, or to Wilmington, Del., 26 miles, while the coal used was hauled from Columbia, about 35 miles away; yet, in spite of these difficulties, she carried on the iron-making business, hiring a superintendent to look after the works and the workmen, while she herself managed the business of the office. Mrs. Lukens was considered an extraordinary business woman. She built up a business which has been continuously successful up to the present and which has remained in the same

family for four generations, and it was in honor of her extraordinary abilities that the name of the works was changed from Brandywine to Lukens.

Originally the sheets were made from a single charcoal-bloom, the bloom having been made in the old-fashioned forge-fire, then reheated over an ordinary grate-fire and rolled into plates or sheets, which sheets were shipped without being sheared, the shearings in those days being cut into nails. But afterwards they put up a reverberatory heating-furnace, in which they worked up the scrap themselves. The plate-rolls, as near as can now be ascertained, were about 16 to 18 inches in diameter and from 3 to 4 feet long in the body, and were driven by an undershot water-wheel. It is said that many a time, when it looked as if the mill would stall, the workmen would rush for the water-wheel, climb upon its rim and by their united weight help the pass through the rolls, thus preventing a stall, which meant fire-cracked rolls and, later on, broken ones. This water-wheel was afterwards supplemented by a breast-wheel so geared as to give more power to the rolls. This enabled them to use larger rolls, but the gearing gave so much trouble that they finally abandoned the use of the water-wheel and put in a steam-engine and enlarged their rolls to 21 inches in diameter and 66 inches between the journals. This was again changed to chilled-rolls, 25 inches in diameter and 84 inches long. After several other changes, they at last put in three-high chilled-rolls, 34 inches in diameter by 120 inches long, a large Corliss engine to drive them, automatic lifting-tables, etc.

The weight and size of the early boiler-plates, as made on the oldest mill, I have been unable to get, but it is not supposed that they attempted anything weighing over 500 pounds, and probably 300 pounds was nearer their limit. As an illustration of the changes that have taken place in this one mill I would say that, as now enlarged, they readily roll plates 119 inches wide and 50 to 60 feet long. The little old mill in which the workmen had to climb on the wheel to help make it go round is one of the best plate-mills in the country, and its owners and managers are the great-grandchildren of Isaac Pennock, who, in 1790, built the Federal slitting-mill on Buck Run and, in 1810, on the banks of the Brandywine, what was called the Brandywine mill.

In the year 1845 I went to Norristown, Pa., and assisted in the building of what, at that time, was considered the best mill for making bar-iron in this country; in fact, it was called a model mill,

and in many respects it was so. While it was a geared mill, it was so much better built than any other mill of the kind that it was expected that it would give little or no trouble on that score. But we were sadly disappointed; for, soon after starting, the gears began to give way, the back-lash and the jar of the rolls causing the teeth to break and drop out.

I was given charge of the machinery, and, of course, had to look after the gear-wheels. At times the entire wheels would seem to go to pieces at once, at other times the arms would crack, and then again the teeth would break, each break, of course, stopping the entire mill. Then all hands had to work day and night to get started again. At first we had to go to the foundry to get such parts as had been broken made over. This, of course, caused considerable delay, and to avoid this loss of time we began to keep segments of gearing on hand, and we had separate wrought-iron teeth made ready for insertion and kept clamps ready to strengthen broken arms. With such extended experience, I became quite an expert in inserting teeth, and it was no doubt due to this fact that on the occasion of several hundreds of my friends coming to Bethlehem not long ago I was arrested at the banquet and tried on the charge of practicing dentistry without first having procured a license or diploma!

Soon after the mill started, I was placed in charge of it on the night-turn, including the puddling-furnaces and the few heating-furnaces used for rolling covers. While this added somewhat to my duties, it proved of great advantage to me, as it gave me an opportunity to obtain a practical knowledge of iron-making. Later on, I was given charge of the mill on the day-turn, which practically meant both day and night, as it was during the day that everything had to be arranged for the night-work, before supper could be eaten or rest obtained, and often to the loss of both. In a short time I received, in addition to my other duties, that of having charge of the roll-turning and of seeing that the iron rolled was properly finished. In short, I, who had entered the mill as a machinist, was now in charge as an iron-master; and it was in connection with this new departure that my troubles began.

In those early days the chemistry of iron-making was unknown, at least in this country, and iron-makers were often but the blind leading the blind. At the present time, if there is any trouble with the product of an iron- or steel-mill, the chemist is sent for, and he is expected to carefully analyze the ore, fuel, flux, cinder and even

the furnace-linings, and find and eliminate the troublesome element, whatever it be, that is damaging the product. But in the early days of iron-making we had no such help, and had to feel our way as best we could.

The process of making bar-iron by the use of the charcoal-forge had become too expensive for iron to be used for ordinary purposes, and the art of making bar-iron by the puddling-process was the only other means of any promise to which we could turn for relief. Puddling was at that time done by what was called the fermenting-process, in which white iron only could be used; and we soon learned that only a few brands of pig-iron could thus be worked into merchantable bar-iron, as by reason of being cold-short it often proved worthless; and the worst of this was, we did not know what caused it. As the works were built to make high-grade bar-iron, which must be neutral, we were in a great quandary, not knowing which way to turn; but as the only way out was to keep on experimenting, we did so, sometimes finding a pig-metal that gave good results. Then all at once it would go wrong again, and why, we could not tell, but it was always in order to lay it on the poor puddler and to give him a good "blowing up."

At times we found that by mixing several brands of charcoal-pig we would get good results, but as the price of bar-iron was low, we could not afford to use high-priced pig, and so we began to experiment with anthracite-iron—and with the old-time troubles, or even worse, as we got both cold- and red-short iron. At this time one of the blast-furnaces which had been making charcoal-iron began to use anthracite coal for fuel. In our distress we tried some of their pigs and got quite good results, the bars not being cold-short, but quite inclined to red-shortness, and for many purposes, such as shafting, car-axles, heavy bolts, etc., proving very suitable. But for the use of the blacksmiths, the iron was quite unfit. They then knew nothing about working red-short iron, and, of course, they condemned it.

We have now learned that good fibrous iron can be made from anthracite pig-metal, but for the highest grades of bar-iron we were still compelled to use charcoal-pig, and in the old way. It would occupy too much of your time to relate in detail the long series of experiments, often ending in disaster, we went through, never knowing when the iron would be good, or what it was that made it red, until at last, by accident, we stumbled on the cause of the trouble.

We noticed that when, after making red-short iron for a time, a change was made to neutral iron, the iron was still inclined to red-

shortness. In a day or two the red-shortness would die out, and we would get on to good bar-iron; and it gradually dawned on us that the trouble might come from the cinder that was left in the furnace when red-short iron was being made. So when we next changed over from red-short to neutral iron, we cleaned all the cinder out of the furnace with great care, and refixed it with neutral cinder, and to our great joy found that the secret of our troubles had been discovered, and that we could now make neutral or red-short iron as we wanted to, with a tolerable degree of certainty.

While the experiences and trouble gone through were both perplexing and annoying, they proved of great value to me in after-years, and especially when we began to make steel by the Bessemer process; for I had early learned how a very small percentage of an objectionable element, either in the ore, the metal, or the fuel, would greatly damage the product. In addition to the trouble we were having in making the iron, we were constantly breaking gearing, spindles, or rolls and couplings. In order to reduce the cost of repairs as much as possible, we tried to have some part of the train made strong enough to do the work when everything was going right, and weak enough to break when anything was going wrong. This was, of course, a cut-and-try business; sometimes the part we intended to break would be made of extra strong iron, and then it would fail to break and some other part would give way; then we would reduce the pattern and make it lighter, and the next casting made of that pattern might happen to be weak iron, and it would break too easily, and then we would have to strengthen the pattern again; and so from day to day we went on, with one break after another, varied occasionally by the giving way of a coupling-box, spindle or breaking-box. The latter would let the end of the roll rise up in the housings, and if the roll was a collared one, off would go the collar, and the roll would be ruined. Of course, the breaking of teeth in the gear-wheels was a common occurrence; and so much trouble came from this source, that I remember that, over forty-five years ago, I was almost inclined to register a vow that I would never again have anything to do with a piece of machinery that had a cog-wheel in it.

In the year 1854, David Reeves, together with a few of his friends, leased a works for making iron rails, located at Johnstown, Pa. I was sent there to complete the mill and to superintend its working. As it was at this place where afterward great and important changes in the manufacture of rails were introduced, I have thought that a

brief history of the works would be of some interest to the members of the society.

The works were originally commenced by an organization called the Cambria Iron Company, but after the mill was partially built, their money gave out, and the project was considered a failure. It was at this time that David Reeves, Mathew Newkirk, George Trotter, and a few others, joined together and leased the plant as it stood; Mr. Reeves, Mr. Trotter and Mr. Newkirk being the most prominent in the matter, and Mr. Newkirk acting as the business manager.

Mr. Newkirk then gave me instructions to go to work at once and complete the mill as soon as possible. Having previously examined the works with great care, I can assure you that it was with serious misgivings that I undertook the task. There was a vast amount of new work to be supplied, and I had very serious doubts as to the efficiency of what had previously been done. From what I learned as to the kind of pig-iron that was to be used, the outlook was anything but encouraging, and I came to the conclusion that there was serious trouble in store for me when the mill would be ready to start; and I can now testify that my forebodings were fully verified later on.

When we at last got to work and rolled a few rails, the edges of their flanges looked like saw-teeth, and the head was rough and full of small holes, and everybody about the mill, from the owners to the water-boy, was disgusted and sick. This was especially true concerning the heaters and the men about the rolls, for they were paid by the ton of finished rails. It was the general conclusion that something would have to be done, and right quickly, too. There were three charcoal blast-furnaces that belonged to the company, one of which happened to be in blast at the time, so we got some charcoal-pig and puddled it and rolled it into covers for the bottoms of the rails, the common iron being above them. These piles were rolled so as to put the charcoal-iron on the edges of the flanges. This worked pretty well as far as the flanges went, but it did not cure the trouble with the heads; so we had to roll other covers for the tops of the piles, to make the head of the rail good; and with hot and cold patching, and a liberal use of putty, we managed to get some rails that passed muster. By continually experimenting in the piling of the iron, and changing mixtures, we finally got out some fairly good rails; but the engine and fly-wheel driving the train were of such a construction that it was not safe to run it over fifty revolutions per minute, which was too slow to make rails out of the materials we were using.

One of the most serious troubles was, that the forward end of the pile would split open in the rolls, so that, when we came to enter it in the next pass, it refused to go in, and much time was lost in bunting it in with the buggy, consequently cooling the pile to such an extent that, when the rolls did get hold of it, spindles, coupling-boxes, and sometimes the rolls themselves, would break, causing both expense and delay, which, in connection with the general depression in business, led to troubles that brought the enterprise to an end.

Again a new company was formed, and it was known as Wood, Morrell & Company. It was in part made up by David Reeves, Charles Wood, Mathew Newkirk, George Trotter, D. J. Morrell, John Shoenberger and E. Y. Townsend. Mr. Charles Wood was made president, E. Y. Townsend, vice-president, and D. J. Morrell, general manager. The change in the organization of the company did not, however, change the troubles in the manufacture of the rails, nor increase the output, both exceedingly important matters, which, unless they could be greatly improved, would still leave the handwriting of failure on the wall. Having, in view of the past, and remembering my former doubts, gone over the entire subject again, I made up my mind as to what must be done to make a success; and I was prepared to submit my plans and recommendations to the new company.

My plan was to build an entirely new train of rolls and to make them three-high and 20 inches in diameter.* This involved a new engine with a fly-wheel that could be run at one hundred revolutions, should it be desirable to do so. In fact, it practically meant an entirely new rail-mill. When the plan was submitted to the company, they said at once it could not be done, for the reason that the expense would be too great; and besides, the mill they had was an entirely new mill, which was supposed to be the very best in the country, and they did not see why it could not be made to do good work. Finally, I succeeded in convincing some of the managers that something must be done, and that if they would adopt my recommendations I was certain of success. After consulting together,

* The principle of utilizing, without reversal, the continuous motion of three rolls, had been already applied to vertical rolls, at the Trenton Iron Works, N. J., under the direction of my friend, the late Charles Hewitt, a man whose reticence and modesty deprived him of much of the credit which his ingenuity and skill deserved. I do not doubt that Hon. Abram S. Hewitt, twice President of this Institute, and justly honored at home and abroad, would be the first to acknowledge how much he owed, in those days of bold beginnings, to the courage, skill and wisdom of his brother.

they directed me to go on and build an 18-inch two-high geared train to take the place of the train we had. To this I replied in the most emphatic manner that I would not do it, as it would be money thrown away. To my refusal, they said the position taken was a most arbitrary one, and one I had no right to take, as I was in their employ on a salary to manage their works, and that they had some right to say what should be done. To this I assented partially, but at the same time told them, that if they continued in the line they were in, there would in a short time be held a large funeral, and I did not intend to stay and attend it. At this the meeting adjourned.

In a few days they gave me permission to go on and build such a mill as I wanted, but they thought it would be better to make the rolls 18 inches instead of 20 inches in diameter; and, by way of compromise, I consented (which was a mistake), and began to build the new train and make other important changes about the mill.

About the time we had the patterns for the new train and engine completed, we were brought to a stop by a protest in the form of a legal document, holding the managing partners personally responsible for the building of a new mill. This, of course, was an unexpected stunner, and all work was suspended.

One Sunday morning when, as I now realize, I ought to have been at church, Mr. Townsend came down to the mill, where I was alone, and brought with him the legal protest and read it to me. After all these years, no person other than myself can fully appreciate the trying position the managers were in. On the one hand I was urging them to go on and build a mill on an untried plan, and absolutely refusing to build the two-high geared mill they asked for, feeling that such a mill would only in a small way mitigate the troubles we had gone through, and that the money spent on such a plant would be thrown away. On the other hand, there was a strong party of stockholders protesting in the most positive manner against going on with my plans, and notifying the managers that they would hold them personally liable for all the loss and damage that might grow out of their unwise action, as they considered this action to be, in adopting a new and untried method that was against all practice in this and the old country, from which at that time we obtained our most experienced iron-makers. Besides, prominent iron-makers in various parts of the country had said to Mr. Morrell that the whole business would end in a failure, and that man Fritz would ruin them. The heaters and rollers were also opposed to my plans, and they appointed a committee to wait on the managers and to say to them

that the three-high train would never work; that they themselves would suffer by reason of its adoption, but that if the managers would put in a two-high geared train, which was the proper thing to do, the mill would go all right.

As I look back to that eventful Sunday morning long years ago, when, sitting on a pile of discarded rails, with evidences of failure on every side, Mr. Townsend and myself quietly and seriously talked over the history of the past, the difficulties of the present and the uncertainties of the future, I cannot but feel, in view of what has since come to pass, that it was not only a critical epoch in the history of the Cambria Iron Company, but as well the turning-point in my own life. For, as Mr. Townsend rose to leave, after a long conference, he turned to me and said: "Fritz, go ahead, and build the mill as you want it." I asked: "Do you say so officially?" To which he replied: "I will make it official." And he did so.

I want to avail myself of this opportunity to say that to no other person so deservedly belongs the credit, not only of the introduction there of the three-high roll train, but of the subsequent wonderful prosperity that came to the Cambria Iron Company, as it does to E. Y. Townsend, then its vice-president. Notwithstanding I had now the consent of the company to go on, many of my warmest friends, some of whom were practical iron-workers, came to me and urged me not to try so foolish an experiment. They said I had taken a wrong position in refusing to build the kind of a mill the company wanted; that in all probability the mill I was getting up would prove a failure, and, being a young man, my reputation would be ruined for life. To this I replied that possibly they were right in what they said, but that I had given the subject the most careful consideration, and was ready to take my chances on the result. The work was now pushed on as fast as possible. In the construction of the rail-train I made a radical departure from the old practice, which was to provide breaking-pieces here and there. I tried to make everything so strong that nothing would break. One of the previous methods was to make coupling-boxes and spindles so that they would break when any extra strain would come on them, and the driving-spindle had a groove cut around it so that it would be sure to break before the rolls. The result was the constant breaking of some of these safety-devices. In addition to all these devices there was what was called a breaking-box on top of the rolls which held the roll in position, which was made hollow, so as to crush if the strain was too great. I directed the pattern-maker to make it

solid. The head roller, seeing the pattern was solid, went to the pattern-maker to have it changed and made hollow, as he supposed it had been made so by mistake, but the pattern-maker refused to alter it, as he said the "old man" (as they called me forty years ago) had ordered it to be made that way. "Well," said the roller, "the old man has gone crazy; and if that box is put in as it is, the mill will be smashed to pieces, and I am going to see him about it," which he did, and, of course, I told him the box was going in solid, as I would rather have one grand old smash-up once in a while than be constantly annoyed by the breaking of spindles, couplings and breaking-boxes; to which he replied: "Well, you'll get it."

The new mill having been prepared and ready to put in place, the old mill was stopped on the evening of July 3, 1857, and after the Fourth I commenced to tear the old mill out and put the new one in, and also to put in the new engine, while at the same time I remodeled everything about the rail-department, and raised the floor-line 2 feet. On the 29th of the same month everything was completed and the mill was ready to be started. I need not tell you that it was an extremely anxious time for me, nor need I add that no engraved cards of invitation were sent out, that not being the custom in the early days of iron-making; and, indeed, if it had been, it would not have been observed on that occasion. As the heaters to a man were opposed to the new kind of a mill, we did not want them about at the start. We however secured one out of the lot, who was the most reasonable one among them, to heat the piles for us, and we kept the furnace smoking for several days, as a blind. At last, everything being ready, we charged six piles. About ten o'clock in the morning the first pile was drawn and went through the rolls without the least hitch, making a perfect rail. You can judge what my feelings were as I looked upon that perfect and first rail ever made on a three-high mill; and you may in part know how grateful I felt toward the few faithful men who were about me, and who had stood by me during all my trials and difficulties, among whom were Alexander Hamilton, the superintendent of the mill, and Thomas Lapsly, who had charge of the rail-department, Wm. Canam and my brother George. We now proceeded to roll the other five piles. When two more perfect rails had been rolled, we were obliged to stop the engine, for the reason that we were so intently watching the rolls that the engine had been neglected, and, being new, the eccentric strap, for want of oil, got hot and bent the eccentric rod so much that the engine could no longer be worked. As

it would have taken some time to straighten the rod and reset the valves, the remaining piles were hauled out from the furnace on the mill-floor. About this time the heaters, hearing the exhaust of the engine, came into the mill in a body and from the opposite end to where the rails were. Seeing the unrolled piles lying on the floor, they took it for granted that the new train was a failure; and their remarks about it were far from being complimentary. Mr. Hamilton, coming along about that time, and hearing what they were saying about the mill, turned around, and using language more pointed than polite, told them that if they would go down to the other end of the mill, they would see three handsomer rails than had ever been made in their country. The next day, which was Friday, we ran all day, and at night put on the regular night-turn. Everything worked well up to noon of Saturday, it being our custom to stop rolling at that time. About six o'clock in the evening, Mr. Hamilton and myself left the mill, and on our way home we congratulated each other on the fact that our long line of troubles and disappointments was now over. About an hour later I heard the fire-alarm whistle blow, and rushing back to the mill found it one mass of flame from one end to the other. In less than one hour's time the whole building was burned to the ground, and a story started that the new machinery was a total failure, and that we had burned the mill to hide our blundering mistakes.

The situation of affairs on that Saturday night was such as might appall the stoutest heart; the result of our labors and anxieties lay there, a mass of black and smoking ruins, and the money that had been so hard to get, with which to build the new works, was gone. The prospect was gloomy, but there was one gleam of light amid all the darkness; and that was the pile of new and perfect rails which, as Hamilton had said, had never been beaten by Wales, from which country most of the iron rails used here came. Above all, the mill had been tried and found to work magnificently, and it was these two facts that gave us all fresh courage, and enabled us to rebuild the mill.

The following day, Sunday, was devoted to rest and to thinking over the matter; at any rate it was not spent in the mill. On Monday morning we commenced to clear up the wreck and to begin the work of rebuilding. In four weeks from that time the mill was running, and made 30,000 tons of rails without a hitch or a break of any kind, thus making the Cambria Iron Company a great financial success, and giving them a rail-mill far in advance of any mill in

the United States, a position they held unquestioned until the revolutionary invention of Sir Henry Bessemer came into general use, and steel rails pushed to the wall the rails previously made of iron. I do not now intend to speak of the wonderful change this invention of Sir Henry Bessemer brought about in this country, nor of the enormous increase in the production of rails it made possible. It is but just to say that some credit for this great increase is fairly due to the introduction of the three-high roll-train first erected, amid the most discouraging conditions, in the mill of the Cambria Iron Company at Johnstown thirty-seven years ago.

The use and advantages pertaining to the three-high train were by no means confined to the making of iron or steel rails. Let any practical man go into the iron- or steel-mills of this country, and he will see, not only how they have served to increase production, but also how in many ways their use has necessitated other improvements, all of which have brought about more perfect work.

If the knowledge we had in the early days of making bar-iron and rails was incomplete and crude, it was not more so than the knowledge displayed in making pig-iron. About 1838 or 1839, Mr. Kunzi, at that time a member of the firm of Farr & Kunzi, large manufacturing chemists in Philadelphia, and one of the ablest chemists of the time, made some experiments with a view of smelting iron with anthracite coal, and about 1842 or 1843 he built a blast-furnace on the Schuylkill River at Spring Mill, and after several unsuccessful attempts to make iron in it he sent for Benjamin Perry, a practical furnace-man, to come and take charge of his new furnace, which he did, and succeeded in getting it in good working-shape and making fairly good iron.

Mr. Kunzi was thereupon congratulating Mr. Perry on his success, and said, that while he himself knew all about the chemistry of iron, he knew nothing about the making of it. To this Mr. Perry replied, that he knew nothing about chemistry, but he did know how to make iron. Shortly afterward Mr. Perry thought he could do better by going elsewhere and blowing in other anthracite-furnaces, and asked Mr. Kunzi to let him off. This Mr. Kunzi did not wish to do, and he invited Mr. Perry to come up to his house with a view of trying to induce him to remain. In connection with this, quite an amusing story is told. During the interview Mr. Kunzi talked about the chemistry of iron-making, and of the combustion of coal, etc., and consequently had a good deal to say about oxygen and hydrogen, all of which became rather tiresome to Mr. Perry, who sup-

posed that he had been invited there to have a drink, and he said to Mr. Kunzi: "I don't know a d——d thing about hydrogen or oxygen, but if you have some good Holland gin, I'll take some of that."

Some fifteen years later it was my fortune to have the same founder in charge of the blast-furnaces at Cambria, as even at that time he was looked upon as being the most practical blast-furnace man in the country. While he was with me, my friend, John Griffin, of Phœnixville, paid me a visit, and he wanted to meet Mr. Perry. So I had him come up to my house, where they soon got to talking on blast-furnace practice; and among other things Mr. Griffin asked him about the coal they were using for making coke, to which he replied that it was bad, being full of brass. Mr. Griffin said: "Mr. Perry, you mean iron pyrites." "Well," said Perry, "you may call it what you d——d please, but I tell you it's brass," and the manner in which he spoke was so emphatic that Mr. Griffin wisely concluded not to pursue any further that branch of the subject. Yet the speaker was the best practical furnace-man that I knew at that time.

Gentlemen, I have already taxed your patience far beyond what I intended when I began this paper; but the subject is one in which I have been greatly interested all my life, and perhaps it is not strange that I have dwelt upon it to the extent I have. Yet, after all, I feel that I have come far short of showing you the real condition of the iron business when I first became connected with it, fifty-six years ago. I would like to have described the shops and the tools we then had; but time will not permit. The younger members who visit the immense iron- and steel-plants of the present day will never know how the old-time iron-maker managed to get along with only the commonest and crudest tools and appliances, many of which have long since gone out of existence. In the machine-shops in which we built our engines and mills, there were very few tools other than the hand-hammer, cold-chisel and file; and I must say, that in the hands of the skilful, hard-working mechanics of those days there was not much they could not accomplish.

I would not feel that I had done my whole duty in my reference to the iron-making of the past, in which I had a part, did I not place on record my admiration of, and my obligation to, the trusty, faithful and stalwart men whom during these many years, from time to time, I had about me. They were, for the most part, uneducated young men from off the adjoining farms, or had received their training as woodsmen or as workers in the collieries, charcoal-furnaces,

or bloomeries scattered about in the hills; they knew little of science or of school-training; but they were courageous, faithful, hard workers, who knew nothing of short hours or of resting when there was important work to be done, and they had lots of good common-sense, which helped them and me out of many a tight place. There were, in addition to the men I have spoken of, and on whom I so much relied in times of break-downs and disasters, a large number of puddlers, who, for the most part, in the early days of iron-making, were Welshmen, and in addition to their being skilful iron workers, generally good men and good citizens.

It is on such an occasion as this that the roll-call of memory brings back to me the faces and forms of my early associates who were engaged with me in the various enterprises of which I have spoken. Nearly all have passed away; but I honor the memory of those who have gone, as I thank those still living for all they did to help and encourage me through the trials and anxieties of the past.

CONNECTICUT WORK AND WORKMEN.

BY DR. GEORGE L. PORTER, BRIDGEPORT, CONN.

(Address of Welcome at the Bridgeport Meeting, October, 1894.)

Mr. President and Gentlemen of the Institute of Mining Engineers:

Unofficially representing the people of the vicinage, it is my pleasant duty to welcome you to the city of Bridgeport, to the valley of the Naugatuck, to the State of Connecticut.

The importance and wide-reaching influences of the investigations for which your Institute is organized, the beneficial results which have directly and indirectly blessed mankind from your and kindred studies, the distinguished position which has been achieved in the scientific world by the Institute as a society, and the personal reputation which is the proper recognition of the ability and knowledge of its individual members, assure all that in attempting to honor you we are ourselves honored.

In the republic of knowledge no fictitious rewards dignify the incompetent and the ignorant—the attested success of inventions and discoveries in the workshop of nature differentiates her real citizenship.

Your present place of meeting is wisely selected; for this senti-

ment regarding the personal ability of the man himself, and his individual responsibility for the welfare of all, was the determining cause in the founding, has been the prolific instrumentality in the development of our commonwealth. The design of the original colonists, the purpose of their institutions, both educational and political, the intentional tendency of their domestic and social life, was to furnish to all in their community an opportunity to acquire what John Milton considered a complete education. "A complete education," said the blind poet of England, "I hold to be one that prepares a man to perform wisely, skilfully, magnanimously, all duties, public and private, in peace and war."

Connecticut has contributed much to the general prosperity.

On May 31, 1638, two years after the beginning of the Colony, the Rev. Thomas Hooker enunciated the doctrine "That the foundation of authority is laid, firstly, in the consent of the people; that the people or their representatives are supreme; from them originate the laws, the establishment of office, its duties, powers and limitations, and the selection of rulers." This sermon in the wilderness, where now is Hartford, developed into the constitution of the Colony: "the first written constitution, ... as a permanent limitation on governmental power, known to history, and the first American constitution of government to embody the democratic idea." It awakened the spirit of independence which culminated in the organization of the nation, and furnished to the constitutional convention of 1787, in Philadelphia, the model of a pure republic, upon which was patterned the Constitution of the United States. If Faneuil Hall was the cradle, the log meeting-house of Parson Hooker was the birth-place, of American liberty.

The first political condition of Connecticut was a typical aristocracy, using the primary meaning of the word which is now "nearly obsolete and very rare," signifying a governing body composed of the best men of the State. In political power, possessions, and allotment of land, they were equals. It was a militant community. In 1680 it had an organized militia of 2507 foot soldiers and a troop of horse—a force as large as that of to-day. In 1713, with an estimated population of 25,000 to 30,000, the effective military organization was over 4000 men; a similar proportionate soldiery to-day would outnumber 100,000 fighting men. When a new settlement went out from the older towns, the pioneers were, in the quaint language of the time, "fastened down" to the locality, and if they failed to stay and defend their new possessions against the Indians

and other foes, their property was forfeited to the Colony. In the War of the Revolution, Georgia furnished 2679 men; South Carolina, 6417; Pennsylvania, 25,678; New York, 17,781; while Connecticut furnished 31,939.

In the Colony the town was the unit, each of the original towns sending two representatives to the assembly. Each family was largely a self-dependent, patriarchal assemblage, living mainly upon the products of the farm, the women skilled in all domestic labor, and familiar with loom and spinning-wheel; the men practiced in outdoor work, dexterous at the forge, the anvil and the carpenter's bench. No idle person was tolerated; every one became an adept in some of the household industries. Hence, a Connecticut colony organized itself at once into a complete body-politic, with equitable laws, with capable officers in harmony with other colonies, and with members educated for self-support. The men thought to be best equipped for the offices were elected, and if they did not serve, they were fined. While in office they were cordially supported; when their time had expired, they were equally loyal to their successors. Until this century, Connecticut was mainly an agrarian commonwealth; but the constitution of 1818 abrogated all special rights of the land-owner, and gradually changed the State from an agricultural to a manufacturing community. Notwithstanding the difference of occupations, the spirit of independence, inherited from a brave ancestry and developed by duties faithfully performed, distinguished the workman. Equal in Church and State, employer and employees realized that skill in workmanship, diligence in labor, honesty in business, and personal ability, determined their individual future. The road to success was closed by no bar of privilege. Fortune and preferment could be better acquired by cordial co-operation than by a niggardly and grudging performance of contracts. The sagacious artisan was quick to see, that in manufacturing articles experienced work upon single parts, similar and interchangeable, cheapened and improved the product, and that if machinery could make these parts, the result would be still more favorable.

Allow me to read a single incident from Johnston's *Connecticut* (*American Commonwealths Series*), page 359:

"Chauncey Jerome began the manufacture of brass clocks about 1821, and with this the modern field of Connecticut ingenuity was opened. The parts of the clocks were soon made interchangeable, so that one workman could give his entire time to the production of each part, while increased production made the whole clock cheaper; and the application of machinery to the production of the parts soon made

prices still cheaper. In 1840, the value of the clocks produced in the State, almost entirely for home consumption, was over a million dollars, and the manufacturers were ready to reach out for foreign trade. So low had they driven cost and prices, that their first exports paid more than 2000 per cent. profit. The story goes that the first cargo of Connecticut clocks for the English market was invoiced so low in spite of this abnormal profit, that the custom-house officers, suspecting undervaluation, enforced their right to take the cargo at its invoice value. This suited the exporters so well that they immediately shipped another cargo, which met the same fate. A third staggered the custom-house, and it went out of the clock business. Since that time, the world has been supplied with machine made Connecticut clocks and watches."

The testimony of the trained and experienced English observer, Mr. Daniel Pigeon, is most interesting :

"The Connecticut workman is usually a Yankee of Yankees by birth, and of a temperament thoughtful to dreaminess. His natural bent is strongly towards mechanical pursuits, and he finds his way early in life into the workshop. Impatient of the fetters which trade societies forge for less independent minds, he delights to make his own bargain with his employer, and, whatever be the work on which he is engaged, bends the whole force of an acute but narrow intelligence to scheming means for accomplishing it easily. Unlike the English mechanic, whom a different education, and different circumstances, have taught to believe his own interests ill-served by facilitating the operations of the workshop, the Connecticut man is profoundly convinced to the contrary. He cherishes a fixed idea of creating a monopoly in some branch of manufacture by establishing an overwhelming superiority over the methods of production already existing in that branch. To "get up" a machine, or a series of machines, for this purpose, is his one aim and ambition. If he succeeds, supported by patents, and the ready aid which capital gives to promising novelties in the States, he may revolutionize an industry, forcing opponents who produce in the old way altogether out of the market, while benefiting the consumer, making his own fortune at the same time. The workshops of Massachusetts, Rhode Island, and especially of Connecticut, are full of such men. Usually tall, thin, reflective, and taciturn, but clever, and, above all things, free—the equals, although mechanics, of the capitalist upon whose ready alliance they can count—they are an element of incalculable value to American industry. Their method of attacking manufacturing problems is one which, intelligently handled, must command markets by simultaneously improving qualities and cheapening prices. We ourselves certainly aim, as they do, at the specialization of manufacture, but one scarcely treads upon the threshold of clock-land before feeling how much more universally the system is being applied in the States than here in England. Tools and processes which we are inclined to consider as exceptionally clever, are the commonplaces of American shops, and the determination to do nothing by hand which can be done by a machine, is a marked characteristic of the workman there, while it scarcely exists among operatives here."

Immigration may in time remove this contrast; but in the past, when strikes have separated labor and capital, the American workman of colonial ancestry, honoring his own independence, has not endorsed the imported barbarism of foreign laborers by violently

preventing the employment of others. Whenever he is deprived of accustomed work, he does not willingly trust to the grudging dole of unions, or supinely depend upon soup-houses, waiting for "the clouds to roll by," but he turns to other labors. When business depression paralyzed the industries of the Naugatuck valley, the mechanics became, temporarily, truck-farmers, and made the enforced summer holiday contribute to their health, pleasure, and resources.

I need not remind you, gentlemen, who are far more conversant with these matters than I can be, of the innumerable inventions, in all departments of usefulness and ingenuity, and especially in those pertaining to mechanical improvements, that have originated in Connecticut, a State that surpasses all others in the percentage of its patentees. Many of these have a native or adopted home here in Bridgeport—of this, however, others can speak more intelligently and familiarly than I.

Early in our history, the probability of mineral deposits stimulated the traditional and inherited desire for wealth. The copper-mines of Simsbury, the property of the town, were "farmed out" to various people, who brought over miners from Germany and spent their money freely, but got little of it back. The ore contained from 15 to 20 per cent. of copper, but was too refractory to do more than lure speculators into bankruptcy through assayers' reports. Governor Belcher wrote in 1735 that he had spent more than £15,000 in fifteen years. It is characteristic of the place and time that the lease provided that one-tenth of the receipts should be devoted to education—one-half of which should be paid for a good schoolmaster for Simsbury, the other half given to Yale College. The enterprise was a failure, and in 1733 the mine was turned into a prison, of which there are many romantic and curious traditions.

The iron-deposits of Salisbury, worked from 1730, were of great value before and during the Revolution, and have been ever since. "The cannon for the army and navy, the heavy chains (some of which may still be seen at West Point) which barred the rivers, the materials for gun-barrels, and other military equipments for the Revolutionary armies, came from the works of Salisbury, which (among the mountains of northwestern Connecticut) were never reached by the enemy."

But in the development of our country the availability of the far greater mineral wealth of other sections forced Connecticut to depend upon the mechanical ingenuity and manufacturing skill of her sons. The expectation was not misplaced. To the mills and work-

shops and factories of her busy villages and cities come from distant states the product of their fields and mines, to be returned manufactured into the necessities and luxuries of the world's daily life at the end of the nineteenth century. The sturdy, self-reliant spirit of the colonists of 1636, nurtured by the worship of God, the love of home, the pursuit of liberty, and the stimulus of an equal and a common opportunity, made possible the prosperity of 1892.

> "For I doubt not, through the ages
> One increasing purpose runs,
> And the thoughts of men are widen'd
> With the process of the suns."

Upon broader lines, however, has the State contributed to the national development. In 1728 Samuel Higley petitioned the General Court, stating that "he hath with great pains and costs, found out and obtained a curious art, by which to convert, change, and transmute common iron into good steel, sufficient for any use, and was the very first that ever performed such an operation in America, and praying for an exclusive right to manufacture the article for a term of years." His request was granted for ten years on condition that he and his associates should prosecute the business and bring it to a good and reasonable perfection within the period of two years. Upon this corner-stone—the recognition of private right in personal invention—was the patent-office department of the United States government erected.

Local tradition affirms that to Stephen Burroughs, a townsman of ours—by the bequest of whose descendant our public library received a "local habitation and a name"—is due the credit of originating the change from the pounds and shillings of the English to the dollars and cents of the American system of money. He was a man of marked ability and of much self-acquired knowledge, and he is said —for the proceedings of the Senate at that time were behind closed doors and not made public—to have influenced our Senators to propose the change and secure its adoption during the third or fourth Congress.

If you were to select the one factor which exerts the most important influence upon our great manufacturing and commercial enterprises, and our colossal railroad systems, you would probably agree that it is the employment of the aggregation of capital under the direction of wise organizers and administrators—in other words, the principle of joint-stock companies.

"The joint-stock act of Connecticut—1837—framed by Theodore Hinsdale, a manufacturer of this commonwealth, introduced the corporation in the form under which we now generally know it. Its principle was copied by almost every State of the Union, and by the English limited liability act of 1855, and the effects of its simple principle upon the industrial development of the whole modern world is quite beyond calculation."

I have attempted to demonstrate that our home is not an inappropriate place for your convention, because we modestly claim that Connecticut gave to Americans the model of republican institutions; a consciousness of the inherent dignity of all honest labor; the form of money most convenient for their requirements; the example of protected inventions—and taught them how to do business. We might present many other claims of great, if not of equal, importance.

But, gentlemen, the purposes of your Institute are not limited to a nation, a continent, or a century. Your quest is for those fundamental principles by whose agency the elements of the universe have been combined, and their action has been made harmonious—you seek by investigation and experiment to make material things minister to the prosperity of mankind; you solicit from nature knowledge of those pristine truths which Lord Bacon christened "the daughters of time" and men call "eternal."

The wide sweep of your practical studies has but recently been recognized, showing how slowly knowledge grows and how feeble is human intellect compared to divine wisdom. Man, searching, investigating, studying during unknown ages, has found out some of those secrets of nature which were created by Him who called them into being and decreed their eternal laws. The discoveries of science are not creations, but recognitions, of the result of these laws, for "the laws of the universe are the thoughts of God."

A child, wandering in some grand cathedral, in amazement watches the play of the sunlight upon the floor, glances at fresco and painting, peers into the dim recesses of the alcoves or up to the lofty dome, and with awe and admiration looks upon the glory of the altar. Perchance he may realize that the windows are used for the entrance of light, and the pillars for the support of the roof, and flatter himself upon his discoveries, but is ignorant of the wisdom that planned and the skill that erected this "silent prayer in stone."

Few and scattered are the elements of human knowledge. Some laws of gravity, of the conservation of force and of environment; some theories of light, sound, heat; some discoveries in electricity, magnetism, molecular division, and of germ-development are now

within its scope, but how much is still beyond! "One's ideas," says Conan Doyle, "must be as broad as nature if they are to interpret nature." The accurate knowledge of mankind, thanks to modern instruments of precision and the power of the printing-press, has wonderfully increased within our own time. Even now, through the influence of this Institute and of kindred societies, we await a new revelation which will correct the errors of the past, explain present mysteries, unfold new truths, and teach that divine laws, unmarred by the ignorant interpretations of bigotry, are constant and consistent.

The groves were not only God's first temples, but His schools as well. To-day the heavens and the earth, with unnumbered riches for eye and ear and mind, invite all with bounteous promise; the doors of their treasure-house are always open.

"Not only around our infancy
Doth Heaven, with all its splendor lie;
Daily, with souls which cringe and plot,
We Sinais climb and know it not.
Over our manhood bend the skies;
Against our fallen and traitor lives
The great winds utter prophecies;
With our faint hearts the mountain strives;
Its arms outstretched, the druid wood
Waits with its benedicite;
And to our age's drowsy blood
Still shouts the all-inspiring sea!"

Mr. President, Bridgeport welcomes you and your associates as the ministers, the Institute as the priesthood, of theology as revealed by science.

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*NOTE ON A SPECIMEN OF NATIVE IRON.**

BY JOHN BIRKINBINE, PHILADELPHIA, PA.

(Bridgeport Meeting, October, 1894.)

A SPECIMEN of brown hematite, taken from an iron-ore mine near Anniston, Alabama, exhibits a metallic streak or thread running through it. This specimen having been sent to me, I have no personal knowledge of its occurrence; but Mr. Albert E. Noble

* Exhibited at the Bridgeport Meeting.

writes that the metallic threads can be traced for a considerable distance in the open face of the working. A fragment from the sample was submitted to Mr. E. K. Landis, who reported that it was metallic iron, and said: "It gives the same streak on porcelain plate as metallic iron, is strongly magnetic, and the material on which it occurs seems to be chalcedony or jasper. Under the microscope, it shows evidences of having been fused, and I found one perfectly spherical globule, resting on the dark-rock background, which proves that it has been fused." Evidently the metallic portion has in some way been reduced from the oxide which forms the bulk of the ore-deposit, and the specimen seems to add one more to the list of localities which have contributed native iron.

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*NOTE ON A SUPPOSED AZTEC MIRROR.**

BY JOHN BIRKINBINE, PHILADELPHIA, PA.

(Bridgeport Meeting, October, 1894.)

WHAT is believed to be an Aztec, or possibly a Toltec, mirror, is claimed to have been taken from one of the tombs at Cholula, near the city of Puebla, Mexico, where there is a mound of considerable size, in fact a hill, largely constructed of adobe bricks by the Toltecs, or by those who antedated them.

The mirror is formed from a mass of what is evidently pyrite, is nearly a true hemisphere, having a diameter of $3\frac{1}{8}$ inches, and weighs 19 ounces. From its unusual size it is believed to have been hung about the neck of an idol. Other Aztec mirrors of smaller dimensions, and of less weight, so as to be carried about the person, are known; but the one exhibited, being heavy, was apparently intended to be held in the hollow of the hand, in which it fits, and may have been utilized to flash heliotropic signals.

The entire surface, both of the base and of the convex portion of the hemisphere proper, has been beautifully polished, but, for some reason, the mirror has been shamefully abused, the edges having been broken, and the center of the base defaced, possibly by some one testing it for precious metals.

Near the top of the hemisphere a hole, one-fourth of an inch in

* Exhibited at the Bridgeport Meeting.

diameter, enlarged near the ends, has been drilled through the mass, leaving material on top of it of a thickness but little more than the diameter of the hole. This hole served as a means of suspending the mirror.

When the mirror was received in Puebla, it was reported to be a meteorite; the unusual number of meteorites and aerolites found in Mexico encourages the solution of vexed questions in mineralogy on the presumption that any questionable specimens, not *in situ*, are dividends from the dissolution of celestial partnerships. But the apparent density, color, and fracture of the mineral of which the mirror was made did not correspond with numerous samples of meteoric iron which had been examined,* and, on the contrary, suggested pyrite. Subsequently the specific gravity was found to be 4.9, agreeing very closely with that of pyrite, given by Dana as 4.82 to 5.2. It also scratched glass.

Señor Antonio del Castillo, Director of the Geological Survey of Mexico, and an honorary member of the Institute, in his catalogue, *Descriptif des Météorites du Mexique*, gives the specific gravity of various important masses of meteoric iron found in Mexico as 7.7 and 7.8. In only one instance does he give a less density, viz., 7.38.

To determine the constituents of the mirror, without adding to the injury already inflicted on it, a block was prepared into which the convex side was fitted, and the abraded portion near the center of the base was attacked by a drill on a modern drill-press. After half an hour (during which time the drill was frequently removed so as to maintain its temper, and the nerves of all in the vicinity were distracted by the screeching of the drill upon the hard surface), sufficient borings were obtained, about half a gramme, for an approximate analysis, but not enough for a complete analysis. Mr. E. K. Landis, who made the determination, reports that he found 46.59 per cent. of iron and 53.61 per cent. of sulphur, corresponding closely with the composition of pyrite, which, according to Dana, is iron, 46.7; sulphur, 53.3. A borax bead indicated that no nickel, cobalt, or manganese were combined, and hydrogen sulphide gave no precipitate, showing that the heavier metals were not present in appreciable quantity.

The labor of fabricating and polishing this mirror, not the least

* In the museum of the Geological Survey in the City of Mexico, there are numerous meteorites, some of them weighing a ton or more.

of which was the boring of a small hole so near the top of the dome, must have required months, possibly years, with the crude appliances at hand centuries ago. The polishing alone demanded no small proportion of time, if one may judge by the resistance offered under a modern drill-press. This polish, which has withstood atmospheric action for centuries, in a dry, attenuated atmosphere, 7000 feet above the sea-level, shows no loss of luster after having been kept at sea-level for more than a year.

Señor Antonio Garcia Cubas credits the Toltecs with special ability in fabricating, grinding, and polishing metals and precious stones, but says that the mounds at Cholula and elsewhere, while credited to the Toltecs, are by many believed to have been constructed by unknown people who antedated this tribe. Whether the mirror is the work of artizans older than the Toltecs or of the comparatively more recent Aztecs, it is a relic of antiquity as measured in this country, and an interesting exhibition of mechanical skill with primitive tools.

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*NOTE ON A PIECE OF CARPENTER STEEL.**

BY JOHN BIRKINBINE, PHILADELPHIA, PA.

(Bridgeport Meeting, October, 1894.)

This piece of Carpenter crucible steel has exhibited a tensile strength of 116,000 pounds per square inch, an elastic limit of 89,170 pounds, an elongation of 25 per cent., and a reduction in area of 42 per cent. The test-specimen had a length of 2 inches between fillets, and was an inch bar reduced to an area of 0.504 square inch. This unusual tensile strength was determined before the bar had been hardened so as to show the remarkably velvety fracture here exhibited; but a piece of the same, tested after hardening, is reported to have stood a stress of 214,000 pounds per square inch. Another test-piece is said to have shown 136,000 pounds tensile strength, 97,300 pounds elastic limit, 20 per cent. elongation in 2 inches, and 40 per cent. reduction of area.

Mr. Carpenter claims that this steel, containing 2.27 per cent. chromium and 1.04 per cent. carbon, can be welded like iron, and has proved most desirable for the highest class of die-steel.

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* Exhibited at the Bridgeport Meeting.

THE NICKEL MINE AT LANCASTER GAP, PENNSYLVANIA, AND THE PYRRHOTITE DEPOSITS AT ANTHONY'S NOSE, ON THE HUDSON.

BY J. F. KEMP, NEW YORK.

(Bridgeport Meeting, October, 1894.)

THE use of nickel-steel has directed increasing interest of late towards the deposits of nickel, and at the same time the parallel advance in our knowledge of the basic igneous rocks has rendered these deposits a subject of important geological inquiry. Investigations have led to some views, more or less new, on the igneous origin of certain ores, which, though not universally accepted, have a growing number of adherents. In Europe, the mines in Norway have been especially suggestive subjects of study, and others somewhat less generally known have also added their testimony; while in this country the great economic importance and scientific interest of the developments in the Sudbury district of Canada, have furnished the principal texts for the discourses that have appeared among us. The purpose of the present paper is to set forth these views through the medium of one interesting ore-body, of which but little has been written; and, by the description of another, to place in contrast two different geological associations in which nickeliferous pyrrhotite occurs, and to indicate a generalization, based upon these different geological characters, and justified by the experience so far gained.

To Prof. J. H. L. Vogt, of Christiania, Norway, is due the chief credit for the revival of interest in the geology of nickel. His important contributions* of recent date have made generally available

* J. H. L. Vogt: 1. "De Canadiske Förekomster af Nikkelholdig Magnetkis." 2. "Eisennickelkies fra Beiern." 3. "Verdens Nikkel-Produktion." *Geol. Fören. Forhändl.* Translation: 1. "The Canadian Localities of Nickeliferous Pyrrhotite." 2. "Pentlandite from Beiern" (a Norwegian mining district). 3. "The World's Nickel Production."—*Proceedings of the Geolog. Society in Stockholm*, 1892, pp. 313–338, 433–475. The same paper was reprinted with an abstract in German, by the Norwegian Geolog. Survey, Kristiania, 1892, under the title "Nikkelförekomster og Nikkel Produktion," and is on sale at 40 ore (about 10 cents) The paper was also read before the German Geological Society as noted in the *Zeitschr. der Deutschen Geol. Gesellschaft*, xliii., 819. See the Canadian *Record of Science*, April, 1892, for much the same in English.

much valuable experience, gained especially in his own country by many years of mining. Vogt classifies nickel-ore deposits under three heads:*

1. Arsenides and sulpharsenides, chiefly in veins; in part, typical nickel-veins; in part, cobalt veins; in a subordinate way, also, silver-bismuth-cobalt veins; also at Mine la Motte and Bonne Terre, Mo. Occasionally the arsenical nickel-ores occur as fahlbands and other forms of ore-deposits.†

2. Typical deposits of nickeliferous sulphides formed by a process of magmatic differentiation in basic igneous rocks. Sudbury, Canada, the Gap Mine, Pennsylvania, and many Norwegian mines, furnish good illustrations. In a subordinate way the same ores also occur in veins, fahlbands, and various other deposits.

3. Nickel-silicates, clearly alteration-products and most prominently or quite exclusively in veins in serpentine; that is, in serpentinized basic igneous rocks, the original silicates of which reveal an always-present, though small, percentage of nickel. The associated minerals are opal, semi-opal, chalcedony, chrysoprase, various meerschaum-like magnesian silicates, brucite, magnesite, ochers, etc. The ores are best explained as due to lateral secretion from the neighboring igneous rock. New Caledonia, Riddles, in Oregon, and various localities in North Carolina, may be cited as illustrations.‡

Good illustrations of the first type are practically lacking in the United States, except, perhaps, at Lovelocks, Nevada. Although many small veins with these ores have been elsewhere reported, none have yet proved important. Of the last type we have good geological examples in both the American localities cited, but in neither, up to date, has any notable production been attained. Under the second type proper belong both Sudbury, Ont., and the Gap Mine, Pa., which are practically alike in geology, while under the additional group of fahlbands and other varieties come the pyrrhotite ores in gneiss, which have occasionally attracted attention and which include the second deposit forming the subject of this paper.

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* "Bildung von Erzlagerstätten durch Differentiationsprocesse in basischen Eruptivmagmata." *Zeitschr für prakt. Geol.*, 1893, pp. 4, 125, and 257, but especially 125, from which the three types given are taken.

† There is, however, no arsenic at Mine la Motte, or Bonne Terre. The ores are nickeliferous pyrite, and (at the former, in a subordinate way) siegenite, a sulphide of nickel and cobalt with a little iron. The pyrite is disseminated with galena in Cambrian limestone, while the siegenite is in an underlying sandstone. Small amounts of copper also appear in the mattes.

‡ In translating, the original has been somewhat abridged.

The Gap Mine.

The Gap Mine in Lancaster county, Pa., is situated about three miles south of the main-line of the Pennsylvania Railroad, a little over fifty miles west of Philadelphia and fifteen miles north of the Maryland border. It lies in the midst of mica schists, presumably Archean, in what was called the Middle Gneissic belt by H. D. Rogers, and the Georgetown series by Persifor Frazer.* This formation is quite narrow in the vicinity of the mine and pinches out to the westward, from the coming in of the limestone on the north and south sides.

From the mine to Baltimore the region is marked by a great development of basic igneous intrusions. Near Baltimore they have been studied by the late G. H. Williams,† whose recent and untimely death is one of the severest blows that American geological science has received. To the eastward, in Delaware and along the State line in Chester county, Pa., they have received attention from F. D. Chester,‡ while Persifor Frazer§ has written of those in Lancaster county, Pa. In all the exposures the original pyroxenic rock shows marked alterations, secondary hornblende (or even, in extreme cases, serpentine) being the usual result. Chromite deposits have been widely met, doubtless a result of the change to serpentine.

The nickel-mine is associated with one of the smallest of these exposures of basic rock, which is also the most remote northwestern outlier. Beyond it, in this direction, no further exposures of this kind are found; and in those to the south, apart from an occasional appearance of "nickel-bloom" in some of the chromite-mines, no notable amount of this metal has been met. About a quarter-mile southeast of the mine a large and fresh dike of olivine-diabase crosses the country in a northeasterly direction. It is, no doubt, of Triassic age, and there is no reasonable ground for inference that it has any connection with the ore.

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* H. D. Rogers, *Geology of Penn.*, i., 81. Persifor Frazer, Report CCC, *Second Geol. Surv. Penn.*, p. 25.

† G. H. Williams, "The Gabbros and Associated Hornblende Rocks Occurring in the Neighborhood of Baltimore, Md."—*Bulletin* 28, *U. S. Geol Surv.*, 1886. "The Non-Feldspathic Intrusive Rocks of Maryland and the Course of their Alteration."—*Amer. Geol.*, July, 1890, p 35.

‡ F. D. Chester, "The Gabbros and Associated Rocks in Delaware."—*Bull.* 59, *U. S. Geol. Surv.*, 1890. "The State-Line Serpentines."—*Ann. Rep. Geol. Surv. Penn. for* 1887, p. 95, 1889.

§ P. Frazer, Rep. CCC, *Second Penn Surv.*, p. 26, and in "Township Geology" on subsequent pages of same report.

The ore-body was discovered early.* Its presence seems to have been noted as far back as 1718, and was revealed by a ferruginous mineral spring, somewhat charged with copper, which issued from the outcrop. One of the shafts of the present mine, called the "Mineral Spring Shaft," was located near this spot. As in all bodies of iron sulphides, the issuing water was heavily charged with ferruginous matter, and even in the recent working this was shown by the thick deposits of bog-ore in the column-pipes. Just as in the later experience at Sudbury, the mine was originally opened for copper, but proved an unsuccessful enterprise for several companies. Finally, at the suggestion of Captain Doble, the pyrrhotite was analyzed; and in 1852, or 1853, the presence of nickel was recognized. Then, however, even more than now, the metallurgy of copper-nickel ores was no simple problem; so that, it was not until 1862, in which year the property passed into the hands of Mr. Joseph Wharton, that the enterprise became successful. In the course of time, the mine became the chief nickel-producer of its day. Preliminary smelting was done at a works about a mile from the mine, and final refining at Camden, N. J.†

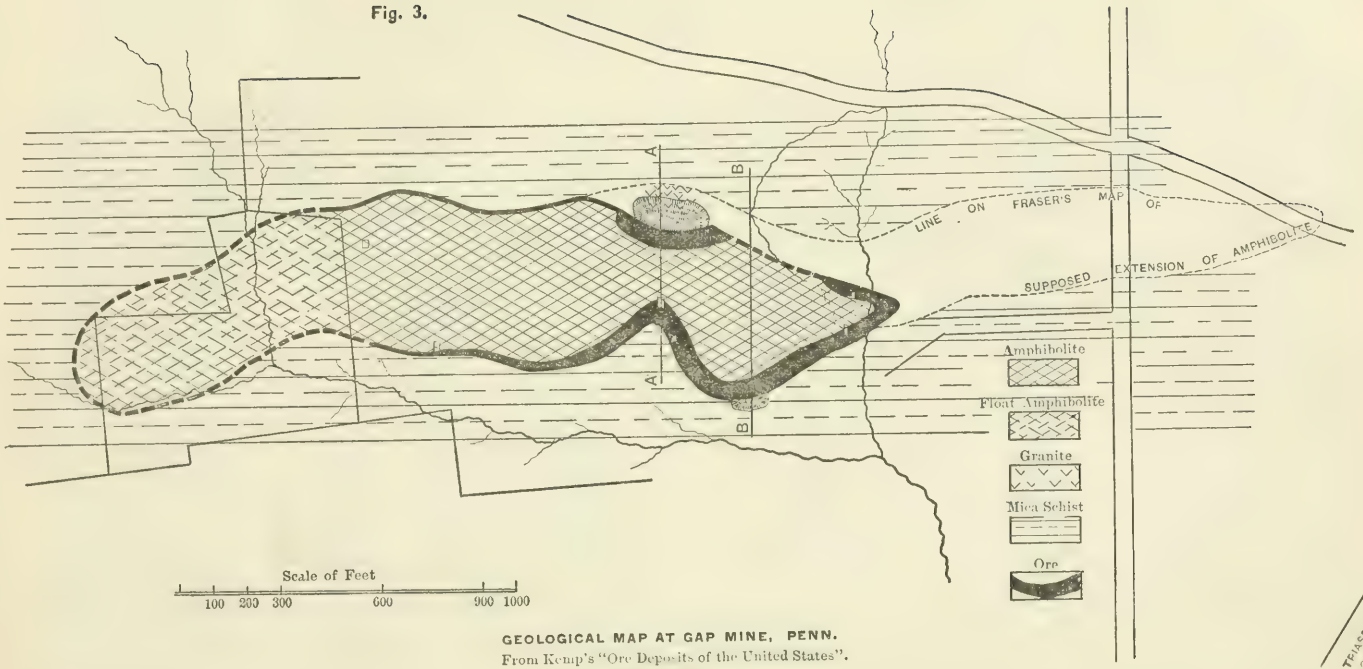
The dark, basic rock, with which the ore is associated, forms a lenticular mass or stock, somewhat constricted near the eastern end, and of outline as shown in the accompanying map (Fig. 3). The identified outcrops make it about 1500 feet on its longer diameter, which is nearly east and west, and 500 feet in its greatest width from north to south. In a sketch-map, given in Frazer's report, the outline is run some distance further east, as is indicated by the broken line on this map, but, according to Captain Doble, this is hypothetical. The assured area is limited in Fig. 3 by the most easterly open cut. To the west, the lens has been traced by test-pits as far as a little cross-brook, and still further by float-specimens, which, in this unglaciated region, are fairly safe indications of the underlying rock. The lens forms at the east a slight eminence, relatively to the surface immediately adjacent as appears in Fig. 1.

The rock consists, most largely, of green secondary hornblende, and often shows almost nothing else than this mineral. It is called

* Capt. Doble, the superintendent of the mine, prepared for Dr. Frazer's report an excellent historical sketch, on which the above statements are based.—*Second Penn. Geol. Surv.*, CCC, p. 163.

† Mr. Wharton presented to the Philadelphia Academy of Science, in '870, some notes on the ores, of which a brief record was made.—See *Proc Phila. Acad.*, 1870, p. 6. Many scattered notes have also been made on the local minerals.

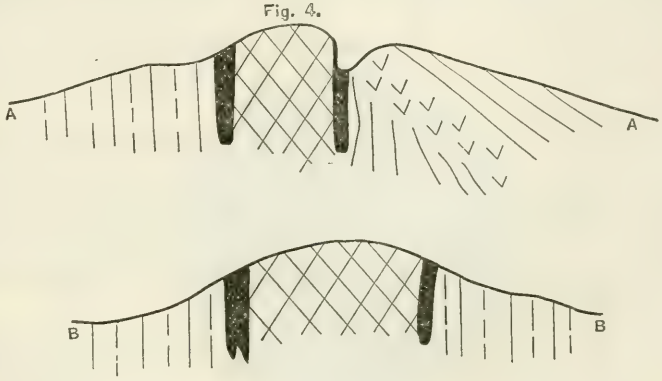
Fig. 3.



GEOLOGICAL MAP AT GAP MINE, PENN.
From Kemp's "Ore Deposits of the United States".
2nd. Edition.

hornblende at the mine, and is best described by the word amphibolite as a rock-name. No one familiar with the microscopic study of rocks, would hesitate a moment in inferring that the green hornblende had been derived from some pre-existing form of pyroxene, or of brown hornblende; and although the change is very thorough, careful search through a number of slides finally brought to light recognizable remains of orthorhombic pyroxene and of olivine, to the former of which we are justified in referring much of the green hornblende. Considerable biotite of a reddish-brown color is present; and, in some instances, considerable plagioclase appears. The common accessory minerals of these basic rocks are not lacking, and it is, therefore, nothing unusual to note irregular bits of titanite.

Fig. 4.



Sections at A A and B B (Fig. 3).

The amphibolite shows a marked laminated or gneissoid structure at the west end, but less prominently at the east. The laminations run parallel with the major axis of the lens, and are evidently due to dynamic metamorphism and shearing. The segregation of the hornblende in richer spots in these gneissoid varieties yields a mottled appearance. The portion associated with the ore, however, is quite massive. The ore favors the eastern end of the line, and disappears, so far as the workings show, on the west. Its general location and distribution are indicated by the shaded areas on the map (Fig. 3). It was found in the outer portions of the hornblendic rock, and, at times, in masses of very considerable amount, but always mixed, more or less, with the amphibolite. This occurrence is illustrated by the view of the open cut in Fig. 2. The ore consisted of pyrrhotite and chalcopyrite in largest amount; but pyrite was not lacking. Crusts

of millerite were also met, and often this mineral furnished a not unimportant portion of the nickel-contents. It is clearly secondary, however, in its nature, and has been formed by infiltrating solutions derived from the pyrrhotite. Other secondary veins of quartz ramified at times through the mass; and, as a rare thing, in some of these were occasionally found beautiful crystals of vivianite. While the amphibolite at the eastern contacts was richly charged with the *kies*, so much so as to have permitted, in at least three places, mining by quite large open cuts, the sulphides seemed to fade out gradually as the contacts were left, and the barren rock was finally encountered. Although some ore has been found in bunches within the lens, practically all the productive ground lay near the walls; and along these, especially on the south side, the chief mining was done. The depth attained was about 250 feet; and, at times, the workings were as much as 30 feet wide. The ore-body was nearly or quite vertical. As mined, it contained from 1 to 3 per cent. of nickel, $\frac{1}{3}$ to $\frac{1}{4}$ as much copper, and $\frac{1}{20}$ as much cobalt.*

Thin sections of the pyrrhotite and gangue reveal relations somewhat as shown in Fig. 5. The pyrrhotite is irregularly mingled with the hornblende, appearing in large and small masses, filling the interstices between crystals of hornblende. Chalcopyrite is intimately associated with it. An especially light yellow sulphide, probably pyrite, occurs in small amounts.

Summing up the geological relations at the Gap mine, it may be said that we have a great lens of amphibolite or hornblendic rock, which yields evidence that it is an altered gabbro or norite or peridotite, with an enrichment of *kies* along a portion of its walls. The amphibolite is an old basic, igneous intrusion, and has suffered some squeezing or dynamic metamorphism, which has developed a gneissoid structure in portions.

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* Vogt gives for the general ratios (not percentages) of Norwegian ores, 5 to 10 of nickel to 1 of cobalt; 1 of nickel to 0.3 to 0.7 of copper ("Nikkelförekomster og Nikkelproduktion").—*Norwegian Geol. Surv.*, 1892, p. 69. D. H. Browne states as the average percentage of a month's work at the following Sudbury mines: Copper Cliff, Cu 4.31, Ni 5.57; Evans, Cu 1.43, Ni 3.74; Stobie, Cu 1.92, Ni 2.36; ratios not very different from the Norwegian (*Eng. and Min. Jour.*, September 16, 1893; December 2, 1893). Mr. A. Blue estimated the total Sudbury output for 1892 as, in the mattes, Ni 2082 tons; Cu 1936 tons; Co 8.5 tons; which numbers give other ratios. The speiss from the ores in southeast Missouri has yielded, for a year's run, Ni 15 per cent., Co 12 per cent, which affords a very high ratio for cobalt. Some 17 tons of Nevada matte from the Lovelocks mines, yielded, Ni 36 per cent., Co 34 per cent., a ratio for Co even higher than Missouri's.

With regard to the probable method of origin, two contrasted views would be at once developed among observers. The earlier and probably most generally accepted one would be, that the ore has been deposited by solutions which have risen from depths along the contact, or have leached the igneous rock itself, and have deposited the ore, impregnating and replacing with the sulphides the hornblendic rock. The subject of origin is not discussed by Frazer, nor has it been directly referred to by others as a result of personal observation, so far as is apparent from the context. Emmons, in his valuable and suggestive paper on the "Geological Distribution of the Useful Metals in the United States" (*Trans.*, xxii., 70) favors this view of origin by solution and secondary concentration in water-ways. Posepny, in his masterly essay on the

Fig. 5.

MICRO-DRAWING OF ORE.
ACTUAL FIELD 1/16 INCH (1.5 MM.)
Hb, horublende. Pyr, Pyrrhotite.

"Genesis of Ore Deposits" (*Trans.*, xxiii., 330), referring to this mine and to Sudbury, describes the igneous view of origin as a "chemical impossibility"—an unwarranted statement; for all familiar with the petrography of the gabbro family of rocks have noted pyrrhotite as one of the commonest of the early crystallizations from the magma. The quantity of the ore in the mines is merely a matter of unusual development in the amount of an original rock-making mineral, the relations of which have been well established. Posepny supports the view of deposition by uprising solutions. W. O. Crosby favors a similar deposition along a crushed zone.*

* "Classification of Economic Geological Deposits Based on Origin and Original Structure," *Amer. Geol.*, April, 1894, p. 259.

It is undeniable that the special abundance of the ores near the contacts, and their general decrease as these are left, give much support to this view; and the analogy with "contact-deposits," an old and familiar type, is strong. There have been also certainly some secondary ("pneumatolitic"* of the late European literature) circulations, which have deposited veinlets of quartz containing included pyrrhotite and pyrite, and which brought about the formation of the coatings of millerite. But it is at least surprising if there have been such extended circulations as have deposited these great masses of sulphides, and have caused them to penetrate the amphibolite in minable richness as much as 30 feet from the contact, that the amphibolite on the one wall, and the mica schist on the other, do not exhibit the effects of corrosion and destruction of their freshness. On the contrary, the amphibolite is not in any degree a rotten rock, such as we so often see alongside of veins or contact-deposits, and such as is so difficult to determine as regards lithology, while the mica schist is sharp and fresh and devoid of ore. This could only be explained by strong metamorphism after ore-deposition. Again, if the ore has been derived from solutions along a crushed zone, it is remarkable that the amphibolite is most massive near the mines, while at the western end of the lens, where the gneissoid (*i.e.*, most sheared) varieties occur, the ore fails.

The explanation of origin by magmatic segregation of sulphides views the original igneous magma as a vast molten mass, quite analogous in its reactions to those of a furnace smelting for mattes. The association of nickel and cobalt with basic igneous rocks which are rich in iron (for instance, meteorites, the Ovifak basalt, peridotites, gabbros, etc.), proves them to be especially at home in such magmas and to be characteristic of their metallic minerals, whether native or sulphides, and of their silicates. That pyrrhotite crystallizes in small amounts from such magmas, and appears as one of the constituent minerals of the earliest generation (the "ores" as petrographers call them), together with magnetite and ilmenite, is, as stated above, one of the commonest facts observed in the microscopic study of the gabbros.† The presence of sulphur in the magma is thus of wide oc-

* By pneumatolitic is meant the deposits from solutions or vapors, especially such as attend the cooling of an igneous intrusion. The older term, "fumarole" deposits, indicates much the same thing.

† While this paper was in press Vogt's latest contribution, "Beiträge zur genetischen Classification der durch magmatische Differentiationsprocesse entstandenen Erzvorkommen," appeared in the *Zeitschrift für prakt. Geologie*, October, 1894, p. 381.

currence; and given a magma rich enough in sulphur, copper, iron, and nickel, there is no "chemical impossibility" in the formation of metallic sulphides, even in considerable amount. Undoubtedly, as Vogt says, the affinities would determine, according to Fournet's series,* the precedence of formation, the chalcopyrite beginning thus before the pyrrhotite. While there is no special difficulty, chemical or otherwise, in understanding the formation of the sulphides in this way, it is less easy to understand how they became concentrated near the outer edge of the flows. It is to be borne in mind that neither at the Gap mine nor in the entirely analogous deposits at Sudbury are they outside of the igneous intrusion, but that they tend to enrich it toward the walls, and that with it they cease. That even narrow intrusions vary chemically from the walls inward, has been known for some years. Thus, A. F. Lawson showed, in 1889, that the outer portion of a large dike in the Rainy Lake region† was much more basic toward the contacts with its walls than in the center; and many observers have noted similar relations in larger intrusions, especially of gabbros. From such observations, and from endeavors to account for the facts, many students of rocks have placed great confidence in the so-called Soret's principle, which has been applied by Vogt to these nickel-ore deposits. According to Soret's principle, in a solution, different portions of which are at different temperatures, the dissolved matter tends to concentrate in the parts of relatively lower temperature, and, in the end, the degrees of concentration of the solution are inversely as the absolute temperatures. To-day, fused rock-magmas are universally regarded as complex solutions, which do not, by reason of their high temperature, fail to obey the laws of solutions. As they cool, various minerals pass the stage at which they can stay dissolved, and therefore crystallize out in succession. The earliest are the metallic oxides, sulphides, and a few others, while afterwards follow the bisilicates, etc. In a gabbro magma the pyrrhotite would be one of the earliest, and would therefore tend, according to Soret's principle, to concentrate toward the walls, which, in an intrusion, would be the portions first cooled. If such a tendency existed in the gabbroic magmas under consideration, it would certainly operate to bring about the condition of things that we see

* Fournet's series is copper, nickel, cobalt, iron, tin, zinc, lead, silver, antimony, and arsenic. It ranges the metals in the order of their affinities for sulphur.

† A. F. Lawson and F. T. Shutt, "Petrographical Differentiation of Certain Dikes of the Rainy Lake Region," *Proc. Amer. Assoc. Adv. Sci.*, 1889, 246.

to-day, although the viscosity of such a fused mass would appear at first sight to be a bar to the free transference of material. It is fair to say, however, that the excessive distances to which in many instances narrow dikes of basic igneous rock have penetrated from their parent mass, indicate that they are practically as fluid as water, and larger intrusions undoubtedly have the same property. But the objection is not without weight, and may be given for what it is worth. While not assured that Soret's principle accounts for the concentration of the ores, yet, after studying the relations at the Gap mine and at Sudbury, I cannot understand how the isolated masses of sulphides could reach their present resting-places in dense dioritic rock and be distributed through it, as we see them, unless they crystallized out as a part of the igneous magma, and at the same time with it. They certainly form an integral part of the rock, and are well within its borders in numberless cases. The diorite, in some of the great open cuts at Sudbury, shows no signs of disturbance, but the ores are distributed through it precisely as are the olivine grains in black basalts. It is significant that those who have studied the district, Bell,* Barlow,† v. Foullon,‡ Browne,§ Adams‖ all unite in support of this view. E. R. Bush¶ is rather doubtful about it, and leans toward aqueous deposition. The paper by J. Garnier practically endorses the views of the Canadian geologists.** The prejudice against an igneous form of origin for any ores is an old and widespread one, but in the minds of many observers it is being gradually broken down. Titaniferous magnetites were first admitted to this category,

* Robert Bell, "The Nickel and Copper Deposits of Sudbury, District, Canada," *Bull. Geol. Soc. Amer.*, 1891, ii., 125-140; especially 135. *Oesterr. Zeitschr. f. Berg. u. Hütt.*, 1892, No. 17.

† A. E. Barlow, *Ottawa Naturalist*, June, 1891; *Ann. Rep. Dir. Min. Stat Geol. Surv. of Can.*, 1889-90, 128.

‡ H. B. v. Foullon, "Ueber Einige Nickelerzvorkommen," *Jahrb. d. k. k. Geol. Reichsanstalt*, xlii., 1892, pp. 276-302; especially 281.

§ D. H. Browne, "The Composition of Nickeliferous Pyrrhotite," *Eng. and Min. Jour.*, December 2, 1893, p. 565; especially p. 566. See also September 16, 1893.

‖ F. D. Adams, "On the Igneous Origin of Certain Ore-Deposits," *Proc. Gen. Mining Assoc. Prov. of Quebec*, January 12, 1894. Dr. Adams writes from personal observation, although this does not appear from the context.

¶ E. Renshaw Bush, "The Sudbury Nickel Region," *Eng. and Min. Jour.*, March 17, 1894.

** J. Garnier, "Mines de Nickel, Cuivre et Platine du District de Sudbury, Canada," *Mémoires de la Société des Ingenieurs Civils*, Paris, March, 1891. The writer is indebted to Dr. Raymond for the loan of this work, which was not otherwise accessible.

and nickeliferous sulphides are rapidly obtaining the same general recognition.

The Philips Mine, near Peekskill, N. Y.

The pyrrhotitè lens on Anthony's Nose, near Peekskill, is quite different in its geological relations from the ore-body just referred to. It is situated on the northern side of the mountain, some 700 feet above tide, and about 3 miles from Highlands station, on the New York Central and Hudson River railroad, 47 miles from New York. Anthony's Nose is the southern peak of the Archean Highlands, on the east bank of the Hudson. The general geology consists of the usual gneisses of these old formations. The general strike is northeast and the character massive. Several well-known iron-mines lie about 20 miles northeast (Mahopac, Croton, Tilly Foster). The ore-body was opened shortly after the war, and was known as the Philips mine. H. Credner, in the *Berg. u. Hütt. Zeitung* for 1866, p. 17, speaks of it as a strong bed, opened some 50 feet across, and affording copper and nickel. It was operated for ten or fifteen years after this, but for sulphur fumes, and not for its metallic contents, which proved too low for profit. My friend, Dr. Arthur Elliott, now of the College of Pharmacy, in New York, who was chemist to the company, informs me that the ore afforded

	Per cent.
Sulphur,	30.
Copper,	0.5
Nickel,	0 3

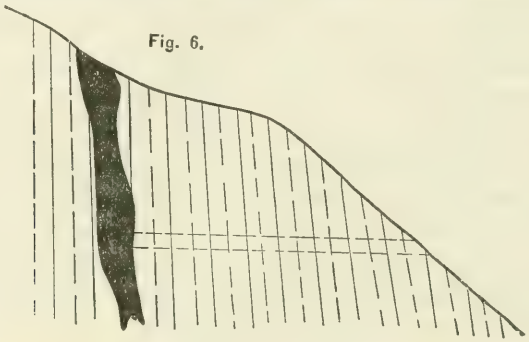
An acid-works, established on the river-bank to utilize the sulphur, still remains, although Sicilian sulphur has now, for some years, been employed in it. The fumes from the Philips ore were especially prized on account of their absolute freedom from arsenic, and a C. P. vitriol was made from them.

The ore was attacked, as shown in Fig. 6, by shafts and by a tunnel, which was run in from a point, perhaps a hundred paces or less down the slope. The ore is mixed with feldspar, pyroxene, hornblende, quartz, and various minor minerals, and appears to be lenticular in character, as the workings do not extend, so far as one can see on the surface, more than 100 feet on the strike,* while they are said to be 300 or 400 feet deep. The immediate wall rock is an acidic gneiss, of a light color, consisting of quartz and plagioclase in largest amount, with a few shreds of hornblende. About 100 yards west, at the mouth of the tunnel, a dark hornblendic gneiss

* Dr. Elliott has since informed me that, in depth, the workings extend several hundred feet on the strike.

is the rock, and consists of large hornblende and plagioclase crystals. It has no immediate associations with the ore; and while, as a possible basic intrusion before the general metamorphism and development of gneissoid structure, it may have been a stimulator of ore-deposition, there is only speculative ground for this suggestion. The deposit appears to be in all respects analogous to the familiar magnetite lenses; and it is significant that in the Croton mines,* which have always been sulphurous, magnetic pyrites, associated with pyrite, has lately been met in quantity. The last edition of Dana's *Mineralogy* also gives several analyses of pyrrhotite from Putnam county, without specifying the exact locality more definitely.

Fig. 6.



Cross-Section of Philips Mine.

The writer's attention was directed to these pyrrhotite mines while reflecting on the association of pyrrhotite and gabbro, and recalling the existence of the great area of gabbros and related rocks that make up the well-known Cortlandt† series just south of Peekskill. The thought occurred that possibly they might have associated with them, or with outlying masses, some show of nickel after the analogy of the Gap mines and Sudbury. The well-known Philips mine seemed a possible case, being only a few miles distant, but, as already observed, on investigation no such associations appeared; in fact, others totally in contrast were found. The Cortlandt series also appears across the Hudson, at Stony Point, and in some minor outliers further west.‡ As is known to those familiar with the mining of the region, nickel prospects have been opened up in this vicinity, and again the surmise was made that these might

* W. H. Hoffman, "The Late Discovery of Large Quantities of Magnetic and Non-Magnetic Pyrites in the Croton Magnetic Iron-Mines," *Trans.*, xxi., 513.

† H. Credner, *Zeitschr. d. d. geol. Gesellschaft*, xvii., 390, 1865. J. D. Dana, *Amer. Jour. Sci.*, September, 1880, p. 194. G. H. Williams, *Idem.*, January, 1886, p. 26, and several subsequent papers.

‡ J. F. Kemp, *Amer. Jour. Sci.*, October, 1888, 247.

Fig. I.

VIEW OF THE GAP MINE, FROM THE EAST, APRIL, 1894.

Fig. 2.

OPEN CUT - SOUTH SIDE OF GAP MINE, LOOKING EAST, APRIL, 1894.

Under the dump at the lower right hand corner is mica-schist. The wall at the left is amphibolite.

prove to be of the Gap type. A trip thither showed, however, that they are precisely like the Philips mine, but leaner and of less moment. A strip of gneiss, perhaps 25 feet wide, has more or less pyrrhotite disseminated through it, forming a fahlband. It has been opened up about 100 feet on the strike and 15 feet in depth. Some minor openings are also to be seen in the vicinity. These old workings are just north of St. John's Episcopal Church and school, and in the gneiss of the Highlands, about 8 miles west from Haverstraw.* Additional small prospects were blasted out under Dr. Elliott's direction, just west of Fort Montgomery—but they all proved too lean. They and the others indicate a belt of pyritous gneiss extending from the New Jersey line across Rockland and Putnam counties.

Openings for nickel in gneiss have also been made at Litchfield, Conn.; at Dracut, near Lowell, Mass.; and perhaps at other points. The geological relations, as indicated by hand-specimens, seem to be practically the same as those along the Hudson. A recent paper by Dr. F. D. Adams, in the *Annual Report of the Geological Survey of Canada*, vol. vi., p. 3 J, on the "Geology of a Portion of Central Ontario, Situated in Victoria, Peterborough, and Hastings Counties," cites the same form of ore-body in that region, viz., lean fahlbands of low-grade nickeliferous pyrrhotite in gneiss. Vogt mentions precisely similar cases in Norway and Sweden, where the name "fahlband" was first applied, and where, as he says, many hundreds of analyses show percentages of only 0.1 to 0.5 per cent. of nickel and cobalt,† and usually not over 0.25 per cent. This repeated and always identical experience in these various parts of the world gives good ground for believing that intrusions of basic igneous rock are the only geological associations thus far occurring with profitable deposits of nickeliferous pyrrhotite, and that light-colored, acidic gneisses are to be regarded as distinctly unfavorable walls, even though the amount of pyrrhotite may be large.‡

In conclusion, it may be added that, whether we agree with Prof. Vogt in his views of igneous origin or not, all will freely acknowledge the obligation he has laid upon the scientific world by his valuable and suggestive contributions.

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* Acknowledgments for assistance in reaching these mines are due to Mr. William A. Speck, of Haverstraw, whose acquaintance with the mineralogy of this region is very thorough, and was of great aid.

† Vogt as earlier cited, *Zeitschr. f. Praktische Geologie*, April, 1893, 130–131.

‡ The general bibliography regarding the igneous origin of ores, of nickel in particular, and of American localities, will be found in full in the writer's *Ore-Deposits of the United States*, 2d edition, 1894, not being reproduced here because hardly germane to the subject.

LEAD- AND ZINC-DEPOSITS OF MISSOURI.

BY ARTHUR WINSLOW, ST. LOUIS, MO.

(Bridgeport Meeting, October, 1894.)

Introductory.—The following paper is based upon a report on the lead- and zinc-deposits of Missouri, prepared for the State Geological Survey, which is now being printed. This report is the outcome of work prosecuted by the writer, assisted by Mr. J. D. Robertson, during the past five years. The studies in the State were supplemented by examinations of the similar ore deposits in Wisconsin and Arkansas. The report will contain in all about 800 pages, and the aim has been to make it complete and exhaustive, especially in descriptive matter. Naturally, only a brief outline can be presented here, and the reader is referred to the report itself for further details.

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All of the lead- and zinc-mining of Missouri is in the southern half of the State, no workable deposits having been discovered north of the Missouri river.

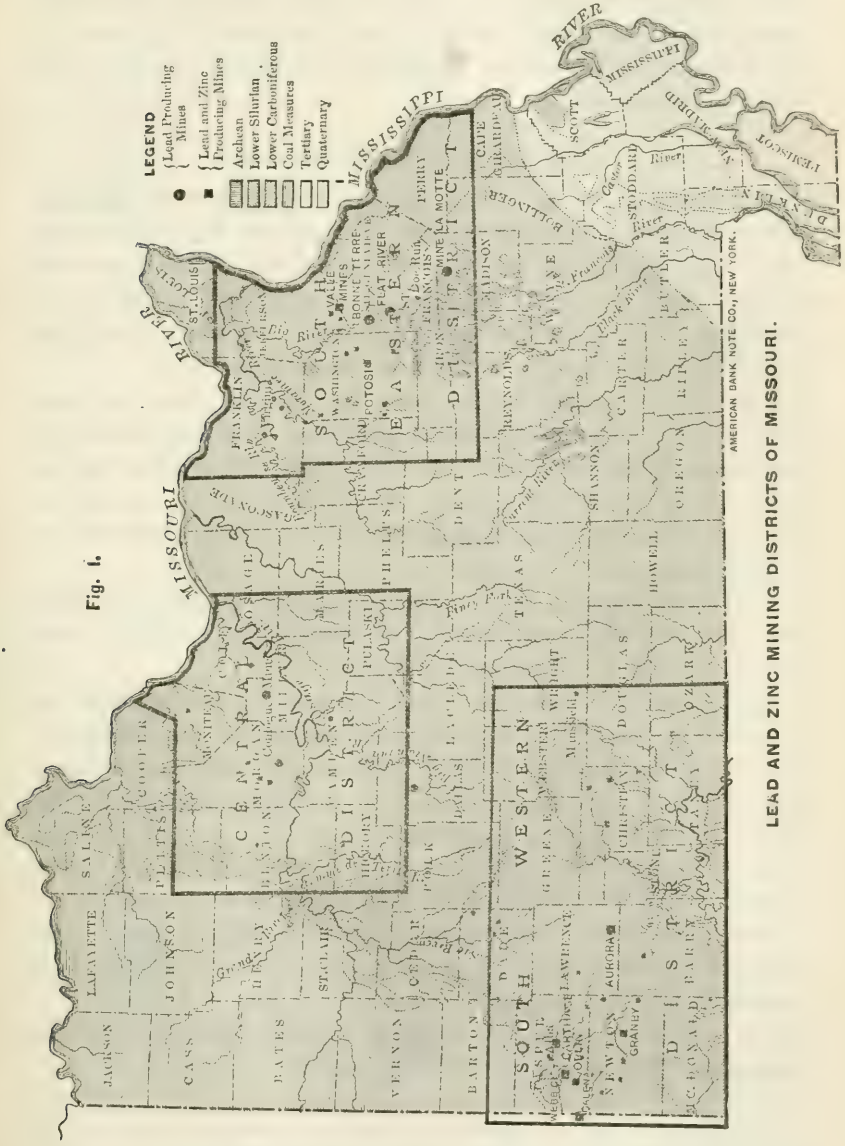


Fig. 1.

Three mining districts are recognized, known respectively as the southwestern, the southeastern and the central. The outlines of

these districts are defined on the map, Fig. 1, as are also the principal camps within each district. These limits include almost all of the mines. As can be seen on the map, they are separated by intervals. Many of the deposits in different districts are similar, yet the principal ones are dissimilar, and their distinctive features constitute a reason, other than that of geographical location, for their separation into districts.

I.—History and Statistics.

The first lead-mining in the Mississippi valley was in Missouri. The first recorded discovery of lead was made by Penicaut, a member of Lesueur's expedition, about the year 1700. According to one account, Mine La Motte was discovered about 1715, but this is doubtful. The first actual digging of the ore appears to have been done by Sieur du Lochon in 1719, on the Meramec river.

Lead-mining appears to have actually begun at Mine La Motte about 1720. From that time on, operations have been practically continuous in the southeastern district, though subject to fluctuations and lulls, up to the present time. The most productive period has been the last thirty years, and the output has increased steadily up to the present.

In the southwestern district, lead-mining hardly began before the year 1850, and work was more or less desultory up to 1870. Since then the growth has been rapid and great.

The utilization of zinc-ores began in Missouri as late as 1869, but expanded very rapidly. The ores had been mined previous to this, but were left on the dumps. This utilization of the abundant zinc-ores was one of the principal causes of the great growth of mining in the southwest after 1870. Since that time, nearly 94 per cent. of the zinc-ores of the State has come from the southwest, while 66 per cent. of the lead-ore has come from the southeastern district.

The central district is comparatively unimportant. Most of the mining there was during the seventies, though small amounts were produced over fifty years ago.

The growth of production in the State from the beginning to the present time is shown in the following table :*

* The figures of lead-ore productions of these tables are based in part upon returns of ore outputs and in part upon yields of metallic lead. They have all been reduced to ores by calculation, the assumed percentages varying from 50 to 70 per cent., according to the period. The zinc-ore returns were all in tons of ore.

TABLE I.

Total Production of Lead- and Zinc-Ores in Missouri by Periods.

Period.	Lead-Ore.	Zinc-Ore.
	Tons.	Tons.
1720–1799	36,000	.....
1800–1819	42,200	.....
1820–1829	30,800	.....
1830–1849	112,900	.....
1850–1859	73,100	.....
1860–1869	45,100	200
1870–1879	229,500	156,400
1880–1893	530,200	1,058,700
Total tons of ore	1,099,800	1,215,300
Total values of ores	$48,738,000	$22,614,000
" " " metals	73,881,000	49,823,000

The distribution of production to date is as follows:

TABLE II.

Total Production of Lead- and Zinc-Ores in Missouri by Counties, from the Beginning of Mining to the end of 1893.

No.	County.	Lead-Ore.	Zinc-Ore.	No.	County.	Lead-Ore.	Zinc-Ore.
		Tons.	Tons.			Tons.	Tons.
1	Barry	160	1,200	16	Madison	147,340	.....
2	Benton	400	10	17	Maries	100	10
3	Camden	700	.....	18	Miller	2,200	.....
4	Christian	5,500	500	19	Moniteau	2,500	500
5	Cole	3,900	.....	20	Morgan	3,550	60
6	Cooper	130	.....	21	Newton	77,285	183,000
7	Crawford	2,700	.....	22	Perry	575	.....
8	Dade	2,140	13,000	23	Saline	300	.....
9	Dallas	700	.....	24	St. Francois	353,811	26,712
10	Franklin	37,450	.....	25	St. Genevieve	1,250	.....
11	Greene	9,400	5,700	26	Texas	11	1
12	Hickory	475	50	27	Washington	160,930	14,000
13	Jasper	232,300	851,000	28	Webster	1,100	.....
14	Jefferson	26,663	34,561	29	Wright	330	1,300
15	Lawrence	25,867	83,711				
					Total	1,099,767	1,215,315

The totals of production during recent years for the whole State, as compiled by the State Mine Inspectors, are:

TABLE III.—*Annual Productions of Lead- and Zinc-Ores, Years Ending June* 30, 1890 *to* 1893.

	Lead-Ore. Tons.	Zinc-Ore. Tons.
1890,	42,100	100,200
1891,	45,450	123,800
1892,	49,600	131,500
1893,	40,300	108,600

The productivity of individual ore-deposits is, of course, very variable. There are, undoubtedly, many very small deposits of small yields, and there is much low-grade ore. On the other hand, there are also many large deposits and much high-grade ore, and some deposits have proved enormous, and have yielded splendidly.

In illustration of the last statements, in the southeast, Mine La Motte, which has been worked over 170 years, has produced lead to the value of about $10,000,000; the St. Joe mine, which has been worked for only thirty years, has produced lead to the value of nearly $14,000,000. The Vallé mines, which have been operated about seventy years, have produced $2,000,000 worth of lead and over $800,000 worth of zinc-ores; but this was from a number of openings.

In the southwestern district, the productions of individual mines have not been so great, and the work is more scattered, yet very large and rich deposits have been found. The following figures illustrate the magnitude of the output by camps:

	Lead-ore. Tons.	Zinc-ore. Tons.
Production of Joplin Camp for 1890 to 1892, inclusive,	12,000	67,000
Production of Webb City-Carterville, 1890 to 1892, inclusive,	12,000	154,000
Production of Aurora, 1886 to 1893, inclusive,	26,000	84,000

As instances of the yields of individual tracts, the Hatton lot of about 1 acre (200 feet square), on the land of the Center Creek Company, of Webb City, produced over $300,000 worth of lead- and zinc-ore. Half of lots Nos. 1 and 2 of the Victor Mining Company produced $275,000 worth of zinc-ore. The Paxton land in Joplin, covering several lots, produced from 1877 to 1893, inclusive, $630,000 worth of ore, mainly lead.

These are, of course, among the largest yields, but there are many others that approximate them; and a large number of properties that have yielded at the rate of $100,000 per acre.

II.—Topography of the Region.

Within the borders of the three districts defined are included the three types of topography recognized in the State. These we designate as: (1) the prairie country; (2) the plateau country; (3) the highland country. Each of these types has features which are represented in the others, and each has features which distinguish it. The combination of these has resulted in the production of regions of entirely different aspects.

The prairie country is represented principally in the southwestern and central mining districts; in the western and northern halves of the former, and in the northern half of the latter. Here the country assumes the character of a broad undulating plain, with a sparse growth or entire absence of timber over wide stretches. Adjacent to the larger streams it loses its characteristic features to a certain extent.

The plateau country occupies principally the southeastern portion of the southwestern district and the southern half of the central district; it may also be recognized, though not in so pronounced a form, over portions of the southeastern district. It differs from the prairie country chiefly in the greater relative altitudes and in the more vigorous sculpturing. The valleys are deeper and the slopes more abrupt, while between the depressions are comparatively flat plateaus and ridges.

The highland country belongs exclusively to the southeastern district, and is represented by the St. Francois mountains. It consists essentially of a series of knobs or domes of Archean rocks, irregularly distributed and covering a comparatively small area. It is distinguishable from the preceding types in the form, structure, and age of the features of relief.

III.—General Geology.

The formations of the three mining districts are the Archean, the Algonkian, the Silurian (with possibly some Cambrian), the Devonian, the Lower Carboniferous, and the Coal-Measures; above these, in places, are certain unconsolidated beds of probable Tertiary age. The distribution of the consolidated rocks is shown on the map, Fig. 1.

The Archean.—The Archean rocks are confined entirely to the southeastern district. They consist principally of the acidic eruptives, granites, and porphyries. The exposed areas are clearly shown

on the map. They occur in hills of the St. Francois mountains, surrounded by Silurian rocks in the valleys.

The Algonkian.—The Algonkian rocks are confined essentially to a limited patch at the top of Pilot Knob in the St. Francois mountains. This is a remnant of a former extensive mass. It includes the well-known bed of specular iron-ore.

The Silurian.—The Silurian rocks, which belong almost entirely to the Ozark stage of the Lower Silurian, surround the Archean outcrops, filling depressions in the old eroded surfaces of the latter formation. They are represented in each of the three mining districts, and occupy the surface over the largest portion of the southeastern district, and over almost the entire central district. In these last two areas they contain all of the important lead- and zinc-deposits. Their aggregate thickness is probably not far short of 2000 feet. A provisional classification of this stage of rocks is given in the following table:

The Ozark Stage in Missouri.

<table>
<tr><th>Southwestern District.</th><th colspan="2">Southeastern District.</th><th colspan="2">Central District.</th></tr>
<tr><td rowspan="8">White River Limestone, including several beds of sandstone.</td><td colspan="2">Joachim Limestone.</td><td colspan="2"></td></tr>
<tr><td colspan="2">Crystal City Sandstone.</td><td colspan="2">Roubidoux or Saccharoidal Sandstone.</td></tr>
<tr><td rowspan="2">St. Francis Limestone.</td><td>Potosi Limestone.</td><td rowspan="5">Gasconade Limestone.</td><td>Jefferson City Limestone.</td></tr>
<tr><td>St. Joseph Limestone.</td><td>Moreau Sandstone.</td></tr>
<tr><td colspan="2">La Motte Sandstone.</td><td>Osage Limestone.</td></tr>
<tr><td colspan="2">Iron Mountain Conglomerate.</td><td>Cole Camp Limestone.</td></tr>
<tr><td colspan="2"></td><td>Proctor Limestone.</td></tr>
</table>

By far the greater portion of the Silurian rocks are magnesian limestones; next in quantity are the sandstones and cherts; and last are the shales and conglomerates.

The Devonian.—The Devonian is very sparsely represented in southern Missouri, and is often entirely absent in the mining districts between the Lower Silurian and Carboniferous rocks. Its absence is evidence of a long erosion-period and consequent unconformity.

The Lower Carboniferous.—The Lower Carboniferous or Mississippian series of rocks is found in connection with the ore-deposits only in the southwestern district. Here, almost all the important ore-bodies are in that formation. It rests unconformably upon the magnesian limestones of Silurian age. Owing partly to a westward dip, but also to a deepening of the basin, the maximum thickness, of perhaps 500 feet, is in the western portion of Jasper county, while along the eastern margin the lowermost beds are exposed. The most abundant rocks are the Burlington limestones, but the underlying Kinderhook and the overlying Keokuk strata are also represented.

The rocks of this series are limestones, cherts, shales, and sandstones. The limestones are by far the most abundant, and are generally white or gray, coarsely crystalline beds, containing abundant crinoid stems and other fossils.

The Coal-Measures.—The Coal-Measures, like the Lower Carboniferous, occur principally in the southwestern district. The southeastern is destitute of these rocks, and in the central district only a few patches or pockets of shale and coal occur. They rest unconformably upon the Lower Carboniferous.

In the southwest, the margin of the Kansas and western Missouri coal-field is represented in Jasper county. Beyond this main area, however, in Jasper and Newton counties, are numerous outliers, too small to locate on the map. These are often of great thickness (frequently 50 feet and more) when compared with other dimensions. They are composed principally of drab shale, sometimes including sandstone and coal. They generally occupy what were originally depressions or "pockets" of approximately circular outline, which were probably great sink-holes, in the Lower Carboniferous limestones. The beds are sometimes horizontal and undisturbed, and sometimes tilted or broken.

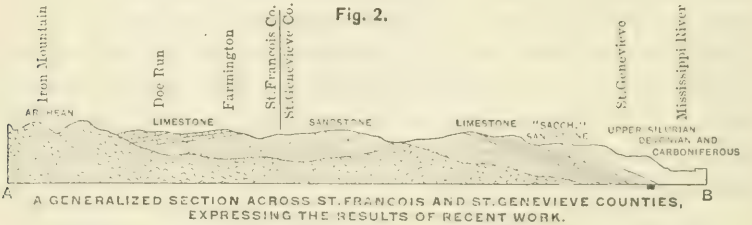
Farther east, in Lawrence, Greene, and Dade counties are other patches of Coal-Measure rocks, consisting of conglomerates, sandstones, and shales, and a little coal in places. These are of greater size than those of Jasper county. They occupy depressions like the latter, but also spread out over the surface and over the underlying Lower Carboniferous. The conglomerates are composed of pebbles of Lower Carboniferous chert. This and the facts of their distribution prove the unconformity between the two formations. A long period of vigorous erosion evidently intervened.

The Tertiary.—The Tertiary period is probably represented by certain deposits of gravel and clay, which are strewn over the hill-

tops in the extreme southwest, and which occur along the Osage and Meramec rivers, in the central and southeastern districts.

Structural Geology.—These formations, as shown by the geological map, Fig. 1, are arranged somewhat concentrically around the Archean of the southeast, from which they have a gentle radial, undulating dip. The two cross-sections, Figs. 2 and 3, illustrate the structure in southeastern Missouri.

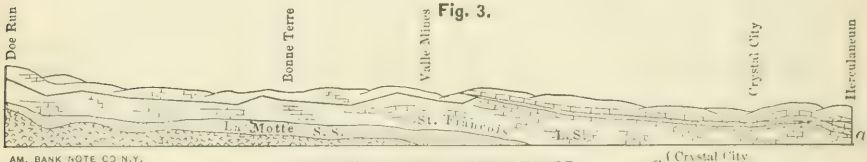
Fig. 2.



A GENERALIZED SECTION ACROSS ST. FRANCOIS AND ST. GENEVIEVE COUNTIES, EXPRESSING THE RESULTS OF RECENT WORK.

No pronounced system of flexures and no great regional faults are recognized in the area. In the radial or quaquaversal dips of the rocks away from the Archean center, we have one great master-flexure. Subordinate to this, certain minor flexures are recognized, but they are so gentle or so small that they are to be regarded as mere undulations or wrinkles. Similarly, there are some well-

Fig. 3.



SECTION THROUGH BONNE TERRE. FROM DOE RUN TO HERCULANEUM.

marked faults, a larger number of minor ones, and a great number of joints and crevices unaccompanied by movements. A certain parallelism between the trends of many of these flexures and fractures and the sides of the Ozark dome is observable, and such may be included in one class. Others are, however, at right angles to the central uplift.

In the southwestern district, a fault running N. 30° W. is found at Mansfield, in Wright county, associated with the ore deposits. The throw is probably not over 100 feet. Along White river, and also in the vicinity of Springfield, there are a number of faults, and local flexing of the rock is apparent. About the mines of Newton and Jasper counties, much local slipping and brecciation has taken

place. This is, however, not due to crustal movement, but rather to surface decay of the limestone country-rocks.*

In the southeastern district, one of the most marked and well-established faults traverses the Mine La Motte estate, about 3 miles north of the mine. The strike is N. E. to S. W., and the throw about 300 feet. Other parallel faults, of less extent, also traverse this estate.

In southern Jefferson county a line of disturbance can be traced, running southeastward for several miles. It consists of a series of breaks, combined with much flexing. Probably the throw does not amount to very much.

In Franklin county faults are exhibited at a number of the crevice-mines. The throws are not demonstrated, but they cannot be very great. The direction is nearly north and south, and thus at right angles to the general uplift.

Crevices, following probably joint-planes, are abundant. The ore-deposits of Christian and Greene counties are largely in such. In Franklin county most of the mines are along such openings. At Bonne Terre, Doe Run, Mine La Motte, and other southeastern mines many such crevices are found. In the early days of mining much block-mineral was taken from them, but they are found to dwindle and die out with depth.

IV.—The Ore-Deposits.

The geographic distribution of the ore-deposits is so clearly shown on the accompanying map, Fig. 1, that little further need be said. Of special interest, however, is the bunching of the deposits about certain centers and their comparative absence elsewhere; also, as already indicated, the preponderance of lead-ore in the southeastern and central districts, and of zinc-ore in the southwestern.

Geologic Distribution.

In the southwest, almost all of the deposits are in the Lower Carboniferous limestones and cherts. Some are in close proximity to

* In a recent paper ("Discussion of Lead- and Zinc-Deposits of the Mississippi Valley," *Trans.*, vol. xxii., p. 622), Prof. William P. Blake adduces the juxtaposition of Coal-Measures and coal with older rocks, on the Oswego tract near Joplin, as evidence of faulting. That he has been misled by the deposits known as coal-pockets, already referred to and abundantly described in the report, is plain. To one not familiar with these pockets, which are peculiar to Missouri, they are liable to be misleading in many ways.

patches and pockets of Coal-Measure shales and sandstones, but no considerable deposits are enclosed in the latter rocks. A few are in magnesian limestones of the Lower Silurian area, but these are comparatively unimportant. In the Lower Carboniferous the deposits apparently range from the lower Kinderhook beds to the Burlington and Keokuk, but in Jasper, Newton and Lawrence counties, which contain the most important, they are confined to the last two sub-stages.

In the southeastern district the deposits are in the Lower Silurian limestones (and Cambrian, if such exist). The larger lead-deposits are in the massive strata near the base, called the St. Joseph limestone. Other lead-deposits, and such zinc-ores as exist, occur higher in the sections, in the cherty Potosi limestone. The sandstones are noticeably deficient in ore. The great basal Mine La Motte sandstone marks, practically, the lower limit of the ore in St. Francois county, though galena is sometimes found a little below the contact. At Mine La Motte, and at the Avon mines, a little ore is obtained from the sandstone. Further, in Franklin county, veins traverse strata of sandstone, but the magnesian limestones are there also normally the ore-bearing rocks.

In the central district all of the deposits are in the Lower Silurian limestones, principally in what we term the Osage limestone, but also in the Jefferson City limestone. Sometimes they are associated with remnants of Lower Carboniferous rocks and Coal-Measure shales.

The Forms of the Ore-Bodies.

Ore-bodies of massive, lenticular, tabular and cylindrical forms exist in all three districts, but certain forms especially characterize each district.

Thus, in the southwest, especially in Jasper, Newton and Lawrence counties, the massive form prevails. Here we have great bodies hundreds of feet in diameter which, when removed, leave cavernous spaces. These may best be called cavern-deposits. Sometimes they are covered; sometimes they reach to the surface. Deposits of other forms also occur in these counties, but they are generally only modifications of the prevailing types. Sheets and pipes sometimes extend from the main ore-body into surrounding barren ground, but these are subordinate. In Christian and Greene counties, tabular or sheet-deposits filling vertical crevices, are more common, though the massive cavern-deposits occur here also.

In the southeast we have, as typical forms, the massive deposits of Madison and St. Francois counties. Here, however, instead of being cavern-fillings, they are impregnations of great masses of magnesian limestone. Occasional tabular deposits filling vertical crevices are associated with them. In Jefferson and Washington counties we have, as prevailing types, tabular and lenticular bodies in horizontal positions, also pipe- and cylindrical-deposits and stockworks associated with these. In Franklin county the tabular deposits in vertical crevices or veins are the common forms.

In the central district there are a number of instances of deposits of massive form, such as that of the High Point mine; tabular deposits in vertical crevices, however, are most common. Small lenticular bodies, almost nodular, and a few pipe- or chimney-deposits are encountered.

The Composition of the Ore-Bodies.

These, like all other ore-deposits, are made up of a mixture of gangue or vein-stuff and of minerals, metalliferous and others.

Among gangue-materials we recognize the following: 1, country-rocks, principally cherts and limestones, especially abundant in the southwestern deposits; 2, clays of several varieties, found in all three districts; 3, sands and shales; 4, secondary cherts; 5, secondary limestones; 6, dolomite; 7, barite.

Among minerals we distinguish metalliferous and non-metalliferous, which we will enumerate later.

Gangues.—The country-rock occurs as a constituent of ore-bodies in two forms: 1, in fragments; 2, massive. The fragmental form characterizes especially the southwestern district. Here, the ore-bodies are huge accumulations of breccia. The fragments are principally chert derived from the country-rock. Some limestone also occurs, but it is an unimportant constituent. Sometimes, blocks of Coal-Measure sandstone, shale, and even coal, occur in the breccia, as the result of a later secondary disturbance and rearrangement of materials. The chert occurs in large slabs or blocks, as in the original rock; or, it may be very much shattered and comminuted. The fragments are all angular and not water-worn. They are sometimes corroded and contain blende or galena in the crevices or cavities; but these minerals are never diffused through this rock. Another class of fragments occurs in the southeastern district. These are water-worn boulders and pebbles of granite buried in a limestone matrix which carries galena. They are abundant at the

Doe Run mines. Country-rock in the massive form constitutes an integral part of the typical ore-bodies of the southeastern district. Here the galena is disseminated through the rock. Similar conditions occur elsewhere also, as in the Pierson Creek mine in Greene county, where galena is diffused through a shale called "gumbo."

Clays of several varieties are found in almost all of the deposits, though they are least abundant in the massive disseminated bodies of the southeast. The color is generally red, but may be yellow, brown, black and even white. The clays are generally plastic, and are mostly quite pure, but sometimes loamy and sandy. They are abundant in Jasper, Newton and Lawrence counties, and in the less consolidated deposits they surround the fragments of the breccia and have the ore imbedded in them. They also occur in vertical crevices in Christian and Greene counties, and in the flats, pipes and crevices of Washington, Jefferson, Crawford and other counties of the southeastern and central districts. Doubtless they are, in most cases, derived from surface-decomposition of the country-rocks, and have been washed into their present positions by surface-waters. Their red colors show that oxidation has been active. Some of these clays, however, are of extreme homogeneity, and are doubtless chemically deposited. These are found filling cavities, often only the lower parts, and showing distinct stratification-marks. Such grade into what is known as tallow-clay, and are thus frequently mixtures of silicate of alumina and iron and of silicate of zinc, possessing particular interest by reason of their high zinc-contents. They are quite abundant in places, but the variability in the quantity of zinc, and the difficulty of separating the higher from the lower grades have prevented their utilization.

Under sands and shales we refer here to certain materials especially found in Jasper county, and also at Galena, Kansas. The sands are highly siliceous, and apparently result from the decomposition of cherts or quartzites. Sands composed of grains or crystals of dolomite are also found, but these we will refer to later. Among shales we include earthy sands and plastic and non-plastic clays. These are sometimes partly consolidated. They grade into sands on the one side and into clays on the other. They are very abundant in the southwestern mines, and, when black and moist, are known as black mud.

Secondary cherts constitute extremely interesting gangues, composed principally of amorphous, chalcedonic silica. They are confined almost exclusively to southwestern Missouri, and particularly

to Jasper county and adjacent parts of Newton county. In the mines of Galena, Kansas, they are very abundant. They vary in color, being black, brown, drab, and even white. In texture they may be glassy or granular, but they range in hardness from that of quartz to the softness of shale. They furnish indubitable evidence of great secondary silicification. Fragments of the original white chert are held so firmly by this secondary chert that when a block is broken the fracture passes through the enclosed fragments without even loosening them from the matrix. Often the whole ore-body becomes thus one compact hard rock, which is mined with great difficulty. Sometimes the solutions of silica have partially redissolved the original chert, and the two grade into each other. The silicification and consolidation of this matrix were evidently later than the formation of the blende, galena, and dolomite, as well-formed crystals of these minerals are held enclosed; often, perfect exterior casts of such crystals are left by their dissolving out from the matrix. In the case of dolomite, the crystals are frequently so abundant that their removal leaves a rock of tufaceous texture. Examples of this are shown in Plate I. The siliceous solutions permeated and cemented the clays and sands which were already present in the ore-bodies in greater or less quantities, which accounts for the differences in composition and color of these gangues. Where silicification was only partial, the material is less indurated and grades into a shale. At the Corry mine, in Dade county, a variety of secondary chert occurs which is called "mineral rock." It is light colored, porous, often vesicular, with minute quartz crystals lining the cavities.

Limestones are found in some deposits, particularly in Jasper county, where galena and blende crystals appear to be held in that rock. This is a secondary rock, formed in the ore-bodies, and cements the chert and other fragments. Crystals of blende frequently fall out of this matrix, leaving perfect casts. Such rock occurs at the Linzee and other shafts about Carthage; also in the Kohinoor and in the Gray mines near Joplin, and at Belleville.

Dolomites (other than the beds of magnesian limestones of the Ozark stage, and other than crystals, which are only of mineralogic interest) are abundant in southwestern Missouri, particularly in Jasper and Newton counties, as irregular bodies immediately associated with the ore-deposits. They are composed of a dense mass of white or drab dolomite crystals. These sometimes make a compact rock, but sometimes it is incoherent and soft, like sand, and can be dug with a shovel. This dolomite frequently occurs in small patches,

scattered through the other gangue-materials or even diffused in crystals; sometimes it is in large bodies, through which drifts are driven for hundreds of feet. It is generally contiguous to the limestone wall-rocks, and appears to grade into them. Blocks of limestone are often found covered with a shell of such dolomite, evidently formed by the action of solutions containing magnesia upon the limestone.

Barite occurs as a gangue-material almost exclusively in the southeastern and central districts. As it will be fully described under the head of associated minerals, we omit further reference to it here.

Minerals.—The minerals of the ore-deposits we divide into: (1) lead- and zinc-compounds, and (2) accessory minerals.

The principal zinc-compound is the sulphide, sphalerite. Important also are the decomposition-products, the hydro-silicate (calamine), and the carbonate (smithsonite); these two are classed indiscriminately as "silicate" in the trade. Of rare occurrence is the hydrated carbonate (hydrozincite), and no occurrence of the anhydrous silicate (willemite) has been noted.

Galenite is at present almost the only commercially important lead-ore. In earlier days more of the carbonate (cerussite, called "dry bone") was found and utilized, but the quantity now mined is insignificant. The sulphate (anglesite) and the phosphate (pyromorphite) occur, but they are comparatively rare and not commercially important.

The principal accessory minerals are calcite (abundant in the southwest), barite (abundant in the southeast), dolomite, pyrite, marcasite, chalcopyrite, quartz, bitumen, limonite, malachite, and azurite.

Though all of these minerals are found in the southeastern and central districts, they are most abundant and occur in most varied association in the southwestern district. This is a rich field for the mineralogist, and contains a wealth of material of crystallographic interest.

Sphalerite or blende, locally known as "jack," occurs in great abundance and purity in Jasper and Newton counties. It is also found in Lawrence and Dade, and to small extent in Jefferson, Washington, and Morgan counties; in the latter associated with barite. It occurs in the common forms and of various colors, due to impurities. An amorphous, white sulphide of zinc is found at Galena, Kansas, and about Joplin, Missouri, evidently deposited from solu-

tion along with tallow-clays. Rarely it is found in the form of stalactites. Sphalerite occurs in crystals distributed through the gangue of the ore-bodies. It also incrusts cavities, and crystals are attached to chert slabs. It is found deposited on dolomite; crystals of red and other colors are seen on galenite; calcite and pyrite or marcasite are deposited on sphalerite. It sometimes replaces fossils. Many crystals have a greenish-yellow coating of the cadmium sulphide (greenockite).

Calamine is locally known as "silicate," and is not distinguished from the carbonate (smithsonite) in the trade. It is especially abundant at Granby in Newton county; also occurs in quantity at Aurora, and is found at other points in the southwest. It is comparatively rare in the southeast. It is found in the usual forms and of the common colors. It generally incrusts sphalerite and plainly results from its decomposition. It is often found coating calcite and pseudomorphous after that mineral (see Plate II.).

Smithsonite is the principal ore of zinc in the southeastern district, and is also found in quantity at Granby and Aurora in the southwest. It occurs of the usual colors and forms. Like calamine, it frequently coats sphalerite and often replaces calcite. Stalactites of both of these compounds are often found.

Hydrozincite is found very rarely in Missouri deposits.

Galenite, commonly known as "mineral," is the only abundant ore of lead. In the southwest it is found almost invariably at the higher levels in the mines. It occurs massive, granular and in crystals, the latter sometimes as much as 6 inches in diameter, often in large aggregates. The crystals are almost always cubes, with octahedral faces developed at alternate corners. In the rock, galenite sometimes occurs in solid sheets or veins, and sometimes cements fragments of chert. The granular galena of the disseminated ores is composed largely of imperfectly formed crystals. When imbedded in calcite, barite, secondary chert, dolomite or other gangue, the crystals are well developed, and were evidently formed prior to the solidification of the enclosing matrix. At Bonne Terre, however, galenite is found deposited on calcite, which is of special interest as indicating the recent formation of the lead-mineral. Pyrite, marcasite and calcite are deposited on galenite. It appears generally to have been formed later than sphalerite, though crystals of the latter are sometimes on galenite. All of the galenites of the State contain small quantities of silver, but none outside of those of the Einstein mine, which is in Archean granite, contain enough to be classed as argentiferous.

Assays of the limestone-ores generally range up to about 4 ounces of silver to the ton, and in one instance, over 10 ounces was obtained. The disseminated ores of the southeast, as at present mined, carry from $\frac{1}{2}$ to 2 ounces, and allowance for all over $\frac{1}{2}$ an ounce, is made by some purchasers and smelters.

Cerussite is known as "dry bone" and "ash mineral." It is found in all districts, mostly near the surface. It is nowhere abundant now, but was formerly mined in quantity. It is often seen as an earthy, white coating on galenite, and crystals frequently stud the surfaces and line cavities.

Anglesite and pyromorphite are comparatively rare and of no commercial importance. Specimens of leadhillite have recently been discovered in ores from Granby.

Calcite is known as "tiff" or "glass tiff." It is especially abundant in the southwest. The crystals are generally scalenohedra, sometimes of great size, as much as two feet in length. Small scalenohedra, with peculiarly curved faces, and of great crystallographic interest, have been found abundantly about Joplin. Calcites generally occur imbedded in the clay of the ore-deposits. They also line cavities. They are later formed than the other minerals, excepting pyrite or marcasite, which is frequently found on calcite.

Barite is called variously, "tiff," "heavy tiff" and "ball tiff." It is confined almost exclusively to the southeastern and central districts, though small quantities have been found in the southwest. It replaces limestone, and is also pseudomorphous after calcite. It occurs frequently massive, filling veins, and as the gangue of galena-deposits. It seems generally to be formed later than galena or blende, but this is doubtful in some cases.

Dolomite is commonly known as "spar." It is abundant in the southwest but not in the southeast. It occurs in masses consisting of a granular or dense aggregate of minute crystals. This is sometimes soft and incoherent and is called "sand." It was undoubtedly an early-formed mineral in the deposits of the southwest, preceding both blende and galena.

Pyrite and marcasite are commonly known as "mundic." Pyrite in isometric crystals is comparatively rare. Marcasite is comparatively abundant, especially in the southwest. This is a late-deposited mineral, and is probably still forming. Chalcopyrite is also quite common in small tetrahedra.

Quartz is rare in crystallized form. It also seems to be of late deposition. It has been found coating galenite and cerussite. In

the form of drusy quartz and chert it is quite abundant in the southeast, and is there known as "mineral blossom."

Bitumen is found principally in the breccia of the ore-bodies of the southwestern district. It also occurs there in cavities of the country-rock. Sometimes the rock or gangue is completely saturated with it.

Malachite, azurite and limonite are also found in small quantities and are minerals of subordinate interest.

Paragenesis of Minerals.—This subject was quite fully discussed by Schmidt in his report of 1873.* As to the originally formed minerals of the southwest, he notes the following succession: Galena, blende, dolomite, calcite, pyrite. In the southeast the succession was blende, galena, barite, pyrite, calcite. So far as the writer's observations have gone, the succession seems to be, generally, dolomite, blende, galena, barite, calcite, pyrite. This order is not invariable, but is the rule. Dolomite appears, however, to be constantly of prior formation. Decomposition-products are naturally of any age subsequent to the formation of the original mineral.

The Structure of the Ore-Bodies.

The prevailing structures are the brecciated and the granular or crystallized; the dense and the banded are, however, also found.

The brecciated structure characterizes the southwestern deposits, as has already been said. Details of many such ore-bodies are given in the descriptions of mines about Joplin. They consist of blocks of chert and other rock imbedded in a matrix of clay, shale and secondary chert, through which the metalliferous and other minerals are diffused. Not all of such a breccia of any one ore-body is metalliferous, and many such masses contain no ore at all. Strictly speaking, perhaps only those can be classed as ore-bodies that contain ore. Sometimes the brecciated condition passes by gradations into unaltered country-rock; sometimes the line of separation is sharp. Breccias filling vertical crevices very often contain comparatively few fragments, and the filling is mostly clay matrix. Such are common in Jefferson and Washington counties of the southeast. Other vertical crevices are compactly filled with a solid breccia. In the central district, the circular or chimney-deposits have a brecciated structure. In the southeast, the brecciated structure sometimes passes into a true stockwork (see Plates III. *a* and III. *b*).

* *Report Mo. Geol. Sur.*, 1873.

The granular or crystallized structure characterizes the great deposits of disseminated ores in the southeastern district. These are made up of an aggregate of grains, and often of well-defined crystals of galena and dolomite. Some crevice- or vein-deposits, such as those at Mansfield, in Wright county, have also a granular structure, passing into the brecciated. The Pierson creek ore of Greene county may also be classed as granular. Portions of the brecciated masses of the southwest are also sometimes granular, especially those composed largely of dolomite.

The dense structure is, strictly speaking, applicable only to those vein-fillings of the southeastern and central districts, which are composed entirely of one material, generally galena. They are usually very narrow or thin. It is applicable also to those composed of massive barite. The class is not an important one.

The banded structure is rare. True ribboned veins are not found. Barite and galena occur in the flat and interbedded deposits of the southeast sometimes in layers, but these are about the only instances.

V.—Descriptions of Types.

Having considered the ore-deposits in a general way, we will introduce as illustrations descriptions of a few types from different districts, in order that the details may be better understood.

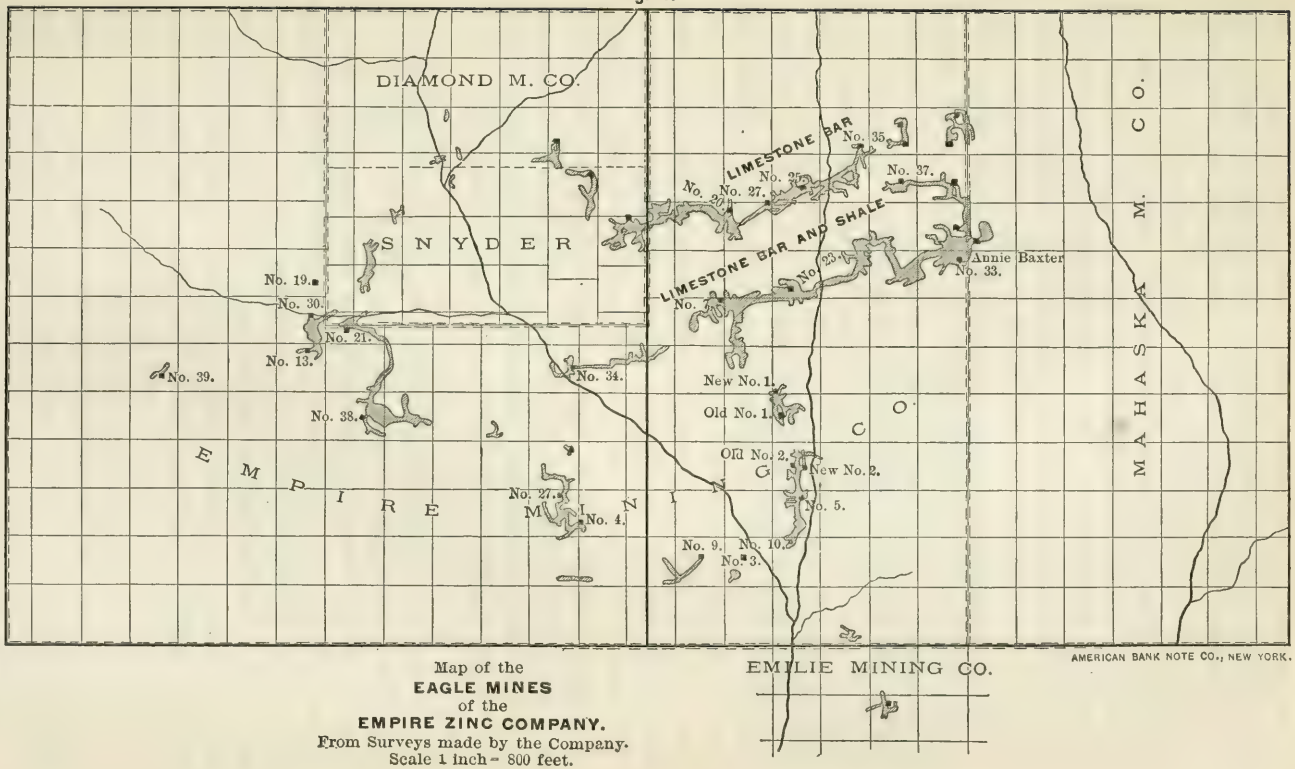
The Eagle Mines.

In the southwestern corner of the Joplin camp, in the S. E. quarter of section 16 and the S. W. quarter of section 15, township 27 N., 33 W., is a group of diggings of which the Eagle mines of the Empire Zinc Company are the center. Adjoining these to the east, and in similar ground, are the mines of the Mahaska Mining Company, and to the west those of the Snyder Brothers. Immediately across the county line to the south are the mines of the Emilie Zinc Company.

In Fig. 4, reduced from the company's mine maps, the distribution and shape of the underground workings are shown. Though parts of these excavations were in barren ground, their outlines are quite closely those of the ore-bodies, and hence the illustration is a very instructive one.

Shaft No. 23 of the Eagle mine, at the time of our visit, was operating two drifts or levels at depths of 55 and 75 feet respectively. The workings of the upper level followed generally an E. to W. direction, while in the lower level they lay more nearly N. to S.

Fig. 4.



Map of the
EAGLE MINES
of the
EMPIRE ZINC COMPANY.
From Surveys made by the Company.
Scale 1 inch = 800 feet.

The ore occurred in horizontal "runs," which are ill-defined oblong bodies of irregular sections, sometimes expanding in cross-sections to form chambers, the contents of which merge into the surrounding barren ground. This barren ground was in composition often the same as the gangue of the ore-bodies proper, and consisted largely of chert. The chert occurred generally in slabs, and sometimes in roughly spherical masses. The slabs were evidently remnants of lenticular layers or thin strata, and sometimes extended across or over an entire ore-body, so as to form the roofs of large chambers; elsewhere they dipped strongly, the strike being frequently parallel to the run of ore.* This chert was of the white or bluish variety of the country rock, extremely fragile, a sharp blow of the hammer shattering a large mass into numerous fragments.

In and around these slabs and blocks of chert was a matrix of granular dolomite, more or less decomposed, associated with red tallow clay, sometimes occurring in large masses. Some portions of this matrix were partially silicified, forming a somewhat soft and impure, dark, secondary chert. In one part of the mine, barite was found in a crystallized condition in sufficient quantities to interfere with the milling of the blende; this is, however, an exceptional occurrence, barite being rare in the mines of this district. Through this gangue the mineralization appeared to follow certain axes, though spreading varying distances from them. The ore-deposit proper is, thus, not a distinct body as if introduced into a pre-existing cavity, but it is, more exactly speaking, an impregnated portion of a brecciated and dolomized mass.

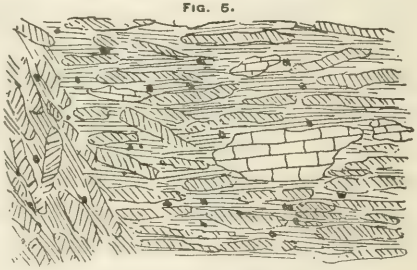
The metalliferous constituents of the ore were principally blende, but a good deal of galena was also found. The crystals of these minerals were disseminated through the decomposed dolomite or clay and through the dark secondary chert, or they were attached to the surfaces of the fragments of the original chert. The mineralized portion was frequently a mere string, while elsewhere it spread out to a large body. In such cases, if the gangue was soft, the whole ore-body could be shoveled out, leaving large chambers, sometimes as much as 150 feet across. The zinc-contents of these ores, as taken from the mines, ranged from $10\frac{1}{2}$ to $12\frac{1}{2}$ per cent., the moisture from 17.3 to 22 per cent., the balance being gangue. From the relative outputs of lead- and zinc-ores, the lead-contents is prob-

* Mr. Pope Yeatman, the mining engineer of the company, expressed the opinion that where such dips converge, as in a shallow syncline produced probably through settling, ore is liable to be found.

ably below 1 per cent. The dressed zinc-ore contained from 60 to 62 per cent. of metallic zinc, and at times reached as much as 66 per cent.

All the phenomena of these deposits yield evidence of great decomposition, alteration, and local disturbance of the country rock. The traces of original stratification are usually well-nigh obliterated, only a few limestone blocks being found in the ore-body, and these disconnected and enclosed in disturbed and shattered chert. Fig. 5, produced from a sketch made in the mine by the writer, illustrates the structure. Between the different runs of ore, concealed ridges of undecomposed limestone, known as "bars," are, however, found.

Fig. 5.



SKETCH SHOWING DISTURBED CONDITION OF ORE BODY AT EAGLE MINES.

The ore-bodies of the other Eagle shafts, and of the Snyder and Emilie mines, located on the map, are similar to the last, with the exception that, in the latter, part of the gangue was silicified and very hard.

Reviewing this group of mines, an inspection of the map, Fig. 4, shows that the runs of ore at the different shafts follow certain lines or courses. One series is plainly in an E. N. E. to W. S. W. direction, while another is approximately N. W. to S. E., nearly at right angles to the last. Further, among the peculiarities of the Eagle mines is the fact that they are located on a divide between two hollows, though in a large sense they are in a depression. The ore-bodies are characterized generally by being much disturbed and open, are associated with much shattered chert with no limestone roof and with few limestone bars; the ground is also usually soft and easily mined. The preponderance of blende over galena is also very great. Oxidized ores are remarkably scarce, considering the open nature of the ground; pyrite is not generally diffused, though it is found at a few places.

The Western Zinc Company or Oswego Lands.

These are on the southeastern edge of Joplin. The ground is less open than that of the Eagle mines, and illustrates a different type.

The Gobar and Tomlinson shaft and the Barker shaft were situated in the N. E. quarter of the S. W. quarter of section 11. They

were about 50 feet deep, and were sunk through solid limestone and chert near the surface, reaching ore-bearing and more open ground at the bottom. The decomposition of the country rock is not very great, and the strata in places appeared to be undisturbed, though at times they dip west fully as much as 30°. In the drift at the bottom of the shaft, the limestone was seen to be highly dolomized, and in this rock were cavities or chambers frequently large enough to admit several men. These cavities were lined with beautiful and large crystals of calcite and galenite, and more rarely with sphalerite. Ore seemed to be confined to the dolomized portions of the limestone. The gangue consisted of this dolomite, of a gray secondary chert or partly silicified sandy shale, the latter containing crystals of blende disseminated through it, and of bitumen diffused in specks. The ore did not follow a crevice, nor did it occur in a run, but was found in bunches in the dolomized rock, and also following decomposed and altered layers between the limestone strata. The blende was of a brilliant ruby red and cinnamon color; no "steel" blende was noticed. As the statistics show, galena is present in large quantities, but is not so abundant as blende. No silicates or carbonates were observed; in some shafts marcasite was found.

The presence of limestone only partially decomposed seems to characterize much of the ground of this tract, as does also the great dolomization and the abundance of calcite crystals. These mines are located about on the crest of the divide between Shoal creek and Turkey creek.

The Alba Company's Mine.

We have at this mine, which is about 7 miles northwest of Carthage, an illustration of an occurrence of ore similar to that of the well-known Circle mines at Oronogo. This is illustrated in the next figure, prepared by the writer while on the ground. To give a thorough understanding of the conditions, we will add the following detailed description of the different openings represented on this map.

At A is a shaft 42 feet deep, in open, chert ground.

At B is a shaft 82 feet deep, in similar ground, with some blende at 70 feet.

At C is a shaft 70 feet deep, going through fossiliferous limestone, with some white chert; only small specks of blende were found.

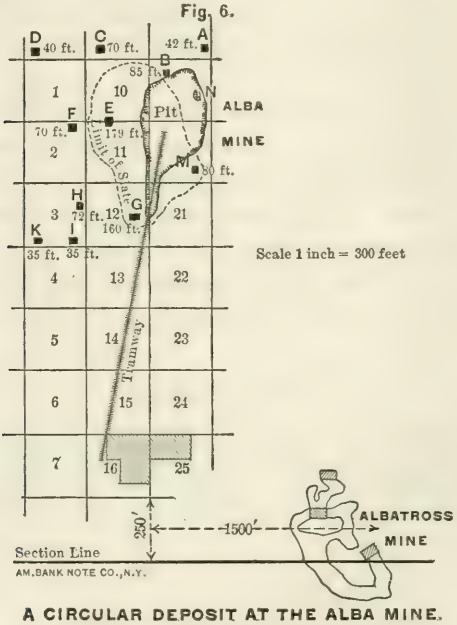
At D is a shaft 40 feet deep, in solid limestone.

At E the shaft is 179 feet deep, and passed through the following materials:

1. Clay and loose chert, ...	15 feet.
2. Clay, yellow, plastic, like tallow, ...	8 feet.

3. Slate and shale, dark drab, with blocks of coal lying at all angles in the upper part, lower half undisturbed; in the shale crystals of blende were found, and calcite penetrating the coal, ... 107 feet.
4. Blocks of dolomite and secondary limestone, with blende between blocks, 24 feet.
5. Ore-body consisting of a breccia of white chert fragments, surrounded by black mud and harder, indurated, impure secondary chert; also red, drab, and black tallow clay, all indiscriminately mixed; softer portions termed mud openings; some dolomite in grains and crystals and in softened blocks, through this, blende is distributed in crystals and fine grains of light-reddish color, . 26 feet.

Fig. 6.



A CIRCULAR DEPOSIT AT THE ALBA MINE.

At F is a shaft 70 feet deep, through open ground, with dolomite, but no slate; at about 60 feet, coarsely crystallized blende is disseminated through the dolomite.

At G is Shaft No. 12, 160 feet deep. Only about 15 feet of slate was passed through here, covered by the surface clay and chert and underlain by limestone and dolomite blocks; the ore-body was struck at about 50 feet, containing some galena and much blende; this continued down to 70 feet. Between 140 and 160 feet another run of ore was found.

At H is a shaft 72 feet deep, in which chert and yellow clay were found containing a little lead.

At I and K are shafts in which solid limestone was struck.

At M is a shaft 80 feet deep, which passed through broken ground and about 15 feet of shale. The upper run of ore, containing principally blende, was struck at 55 feet.

At N is an undisturbed outcrop of soft porous chert, weathering red on exposure. Just west of this, between it and the margin of the slate, smithsonite was struck at about 40 feet.

In addition to these openings, numerous shallow shafts were put down in past years beyond the western edge of the open pit shown on the map, and on the eastern side also; these worked the upper run of ore at depths between 50 and 70 feet, which contained lumps of galena, much blende, and some carbonate of zinc, associated with clay and chert.

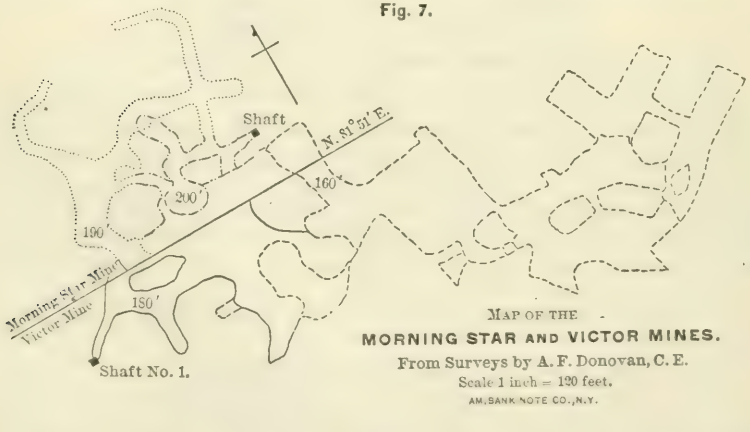
During the year 1892 an attempt was made to work this upper run of ore by an open cut or pit, which was sunk at great expense to a depth of 60 feet. Its outlines are defined on the map. Among other materials, some 35 feet of coal shale was taken out within the limits shown.

A consideration of all these facts plainly shows that we have here an instance of a coal-pocket, around the margin of which the ores occur in a breccia of residuary materials derived from the Lower Carboniferous rocks. This zone of brecciated material, though irregular, appears to be in the neighborhood of 100 feet wide, and beyond this the solid limestone is encountered. A few hundred yards south of the mine undisturbed limestone is seen in the bluffs along the Spring river.

The Victor Mine.

This mine operates on what is known as the Thomas Conner land, south of Webb City. It is in the northeastern corner of the S. E. quarter of the S. W. quarter of section 20. It immediately adjoins

Fig. 7.



the Morning Star mines, with which the workings are connected underground. The adjoining map shows the extent of these workings. This mine has been one of the largest producers of the group,

the product being entirely zinc-ore. At the time of inspection the shafts were about 200 feet deep. Shaft No. 3 passed through about 80 feet of limestone overlying the ore-body. The latter consisted of chert-breccia, the matrix or filling between the chert fragments being clay and a soft, earthy secondary chert. Masses of dark bituminous shale were also found, showing planes of stratification and containing nodules of chert; the layers of shale were seen, however, to dip strongly, showing that it was not in its original position. Drifts were opened in this ore-body to a height of 70 feet. The chert in the body was very brittle and much shattered, often breaking into long splinters, whence it is called "butcher's-knife flint." No black shale or hard, secondary chert were seen. The ore, which is almost entirely blende, occurs in massive crystalline masses between the slabs of chert, and also disseminated through the gangue. Very little pyrite or dolomite was observed.

The fact that shaft No. 3 penetrated a thick stratum of limestone does not necessarily mean that the whole body of ore is covered by such a bed; and the same remark applies to many other mines in this camp which are operated under a limestone roof. On the contrary, at the Victor mine numerous instances of sink-holes or cavings of old workings were seen, which showed that the cap-rock of Lower Carboniferous limestone was penetrated up to the surface in some places by pipe and sink-hole openings, thus furnishing channels for the ready access of surface materials to underground cavities.

The principal distinguishing features of these Webb City and Carterville deposits are, 1, the presence of a stratum of limestone of variable thickness, overlying the main ore-body; 2, the frequent presence of bodies of Coal-Measure shale, and sometimes of coal beds above this limestone roof, these shales often reaching as much as 50 feet in thickness;* 3, the great size of the ore-bodies and their complete brecciation, no evidence remaining in the large mines examined that the materials were ever stratified; 4, the inclusion in the ore-breccia of large masses of shale, distinctly bedded and dipping at various angles, indicative of disturbance; 5, the preponderance of zinc minerals over lead minerals, as is suggested by the statistics of production given on page 638, a very small percentage of the output being lead-ore, probably less than 1 per cent. if measured by tons.

* At some points the coal-bed is of sufficient thickness to warrant mining; and such a pocket northwest of Webb City has yielded several thousand tons of coal.

The Bonne Terre Mines.

As an illustration of the disseminated ores of the southeastern district, we select for description the Bonne Terre mine of the St. Joseph Lead Company in St. Francois county. This is a very large mine, employing hundreds of men, and equipped with complete operating and smelting plants. The value of the lead produced during the past twenty-eight years has been over $14,000,000. The magnitude of this and other deposits of southeastern Missouri does not seem to be appreciated by the outside world. They are apt to be classed with the shallow crevices and surface diggings of the upper Mississippi region and elsewhere in Missouri, from which they are entirely distinct.

General Geology of Bonne Terre.—The country-rocks about the mines are principally magnesian limestones, though some argillaceous shale, called slate, is also found; sandstone occurs at depths under the limestone, but is nowhere exposed at the surface, near the mine. No chert occurs in the limestone, nor is it found either in drusy or nodular forms on the surface, excepting over the hills to the southwest. The entire thickness of the limestone series must be in the neighborhood of 700 feet, reckoning from the hill-tops to the bottom of the drill-holes. The upper 100 feet or so of this contains more or less shale, and the magnesian limestone is in part thinly bedded and flaggy; under this are some 300 feet of thicker bedded, massive limestone in which the ore occurs; below this again there is argillaceous shale for about 30 feet, followed by 100 feet or so of magnesian limestone, which is sandy to a greater or less extent, and contains also some thin layers of shale. This is followed by the basal La Motte sandstone of as yet undetermined thickness. The strata dip 1 or 2 degrees northward.

The magnesian limestones are generally of a gray color when unweathered; are dense and crystalline, or granular or pitted, with numerous small cavities; they are sometimes of a bright-green color, from contained chlorite, and are often arenaceous, passing into sandstone, especially in the lower horizons.

The shale is of a greenish or bluish color, argillaceous, frequently intercalated with thin layers of magnesian limestone; it becomes very fissile and soft on exposure to the weather.

The sandstone of the lower members of the limestone series is highly calcareous, of a gray color, and it is often impossible to say where the dividing line should be drawn between it and the associated magnesian limestone. Of the basal limestone, only one set

of specimens was seen by the writer, namely, that from drill-hole, No. 50 C. These cores were of soft friable, brown sandstone, the grains being well-rounded and not enlarged by crystallization, and held in a matrix of very fine sand with little or no lime.

The Ore-Bodies.—The ore at the Bonne Terre mines consists of galena disseminated through magnesian limestone. No special kind of limestone seems specially favorable for the ore, though almost all of the galena mined is found in the open and coarse-textured rock, and little or none in the compact and fine-grained varieties. Much of the rock in the mines is of dark color, probably through a discoloration of the calcite and dolomite by iron or manganese salts and

FIG. 8.

DISSEMINATED GALENA IN MAGNESIAN LIMESTONE.

some organic matter. Neither do we find the impregnations of the limestone confined to a limited horizon; all portions of the rock above the lower shale-beds have yielded ore. Numerous drill-hole records examined show a preponderance of galena between the depths of 150 and 250 feet, and most of the mining is now conducted within these limits. In the early days, however, disseminated ore was mined from higher levels, and east of the Hoadly shaft disseminated ore in sheets was quarried out at the surface in large quantities. The galena occurs diffused or concentrated along stratification planes, in either case intimately mixed with the grains of calcite or dolomite, so as to form an integral part of the rock. These conditions are

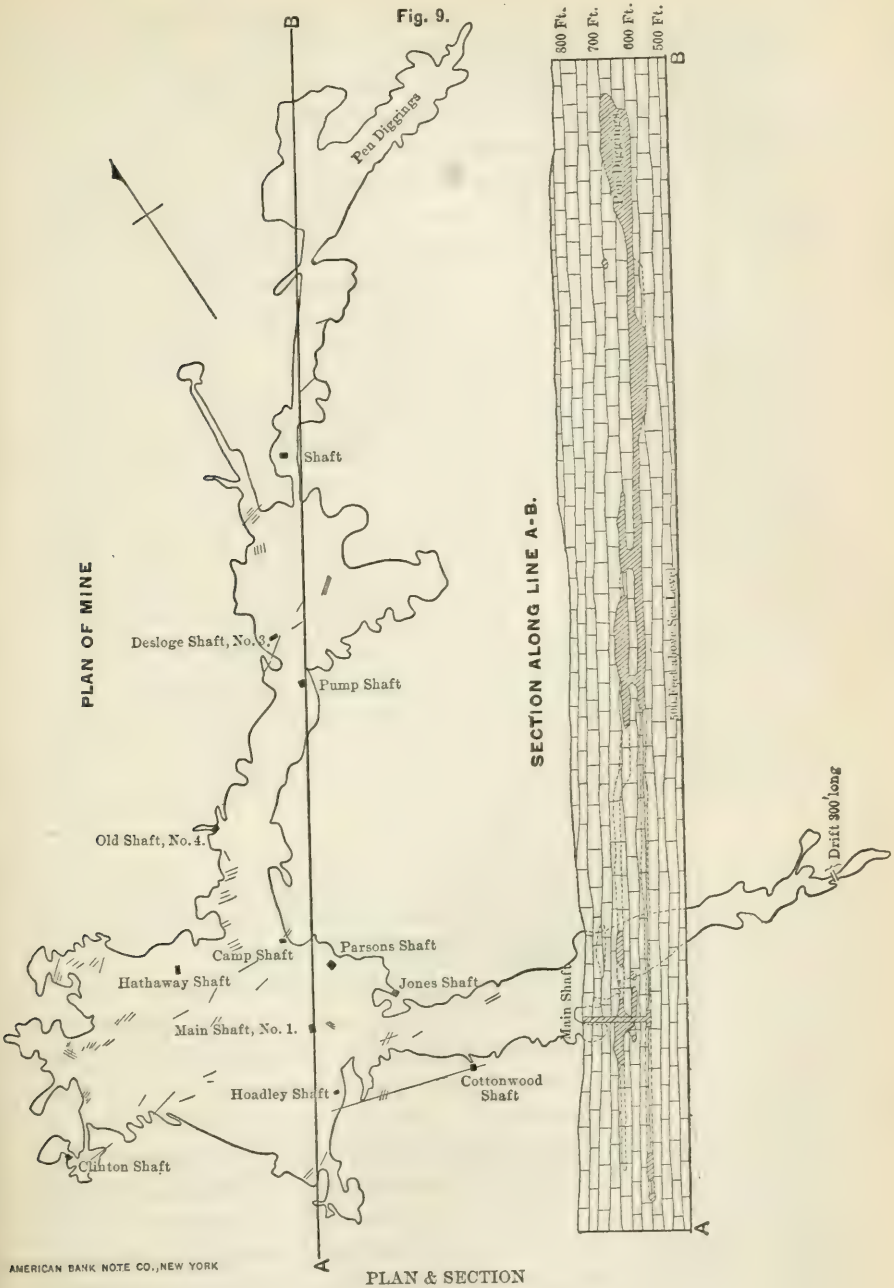
illustrated in Fig. 8. It is also found lining and filling minute joint-crevices. The ore-body at any one point may occupy one or more beds of limestone of limited thickness, while elsewhere the impregnation may expand, and magnificent faces of ore nearly 100 feet high have been opened in the workings. No generally established lines or planes limit these ore-bodies in any direction, but the galena-contents usually fade out into the surrounding country-rock. In some instances, however, stratification-planes or crevices mark the limits of the ore. The average contents of the rock, as taken out of the mines, is about 5 per cent. of metallic lead.

Near the surface, concentrated deposits of galena were found in crevices, and in the days of early mining, these were the ores principally worked. Crevices are also encountered in the deep mines, but there they are not ore-bearing. Mr. C. B. Parsons, superintendent of the company, informed the writer that a channel, located about 50 feet south of the Hoadly shaft, and running thence through the Cottonwood shaft, was plainly displayed in past years of mining, and supplied large amounts of massive galena. This crevice was from 2 to 4 feet wide; its position is indicated on the accompanying mine-map (Fig. 9). Several such crevices were developed and worked here, but they were all found to grow narrower and to become unproductive with depth.

The ore of these deposits is now entirely galena, though cerussite was found at the surface in early mining. Absolutely no zinc occurs here, not even enough to be detected by analysis. Pyrite is always present, though it does not always contain copper. Chalcopyrite and pyrrhotite, carrying about 4 per cent. of nickel and cobalt, characterized the upper workings down to a depth of about 100 feet. Latterly it has been encountered in the deeper Pen diggings, and the ore as mined now, according to Mr. Setz, the assistant superintendent, contains from 3 to 4 per cent. of copper. From one to two ounces of silver per ton are known to exist in the galena. These impurities class the product as "mineral" lead. Calcite has been frequently found in this mine, but no barite was observed. Interesting specimens were obtained here, showing pyrite crystallized on galena and also *galena on calcite*, which latter fact suggests that a deposition of galena may still be in progress. The galena is crystallized in both octahedral and cubical forms.

Fig. 9 is reduced from the company's mine-map. The accompanying cross-section is ideal in part. This map shows that the mine extends nearly three-fourths of a mile underground. The

Fig. 9.



PLAN & SECTION
of the
BONNE TERRE MINE
of the
ST. JOSEPH LEAD COMPANY.
Scale, 600 Feet = 1 Inch.

drifts or galleries or rooms are large, generally presenting faces about 20 feet high and sometimes as much as 100 feet. The shafts are well equipped with cages, and the underground haulage is effected with mules. Compressed-air drills and other improved appliances are used in excavating the ore.

On this map special pains have been taken to show the distribution and courses of the many crevices which are to be observed in the mines. The prevailing course is plainly seen to be between E. to W. and N. E. to S. W. In many cases the crevices were non-persistent, and died out in the roof or wall-rock; at other times they were continuous so far as exposed, and may be represented further on by other crevices. No one series of openings is, however, prominent on account of size or associations, and there is nothing to lead one to conclude that any master-crevice traverses the ore-body. For this reason no attempt has been made to represent on the map anything but facts regarding these openings. Any one who will study this map can formulate about as good an idea as to their possible connection as he could do were he in the mine. No faulting or displacement was noticed along them; they are not generally open, and those that are so usually connect with the surface and are filled with red mud or clay. More often they are tight, with lens-like expansions or openings at intervals. Water drips from most of them, which is highly charged with lime, and coats the surfaces of the rock and cements the loose materials which lie on the floor.

The Vallé Mines.

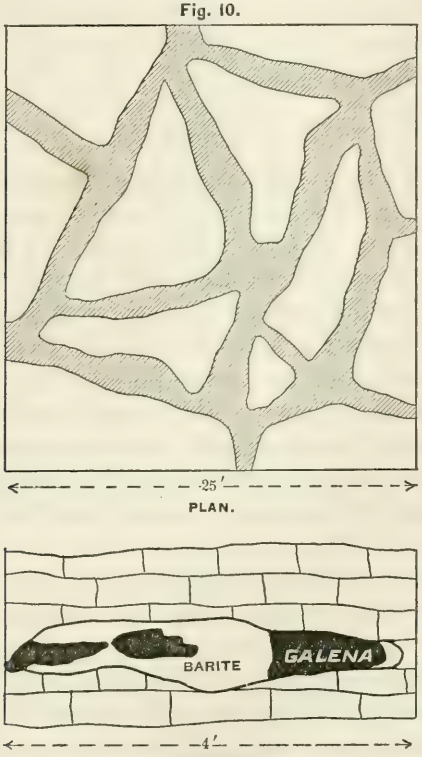
As examples of the interbedded and pipe and crevice deposits of the southeastern district, we introduce the following descriptions of openings at the Vallé mines, in Jefferson county:

The Garatee Mines.—These mines are located about a mile and a half northwest of the railway-tunnel at the Vallé mines, somewhere near the southeastern corner of section 1, township 33 N., 4 E. Several shafts have been sunk here. The workings of No. 6 were examined by us. This was about 135 feet deep, and three runs of ore were encountered at depths of 65, 95, and 135 feet respectively. The shaft was sunk in a chimney or opening, and no rock was encountered down to the upper run of ore. This consisted of a network of channels, lying in an approximately horizontal position, similar to the occurrences of ore at the Palmer mine, Washington county, and like those at the Corn Stalk diggings, described below. These channels contained a large amount of red clay. The ore con-

sisted principally of galena enclosed in barite. Some smithsonite was also found. With these occurred calcite in large crystals, pyrite, and some limonite. The ore in the other runs occurred in similar networks of channels, and these were connected at times by vertical chimneys. The channels were frequently not over 1 foot thick and a few feet wide, but they often expanded to cave-like dimensions.

The Corn Stalk Diggings.—These were shallow pits at the time of

Fig. 10.



DEPOSIT AT CORNSTALK DIGGINGS.

inspection, and the deposits had been quite recently opened. They are located about half a mile northwest of the Vallé Mines post-office, and, consequently, about in the N. W. quarter of section 6, immediately adjacent to the railway-track. The galena was first encountered in the surface-clay in the railway-cut, lying directly on the limestone country-rock; thence eastward, with the rise of the hill, it extended under the rock also. Shafts to the depth of 30 feet

had been sunk at the time of our visit. The ore was galena, in large masses, associated with and imbedded in barite, as is shown in Fig. 10. Red clay was also found with the ore. These occurred in a network of horizontal runs, as illustrated. They were confined to one plane of stratification, so far as developed at that time. The channels were of flattened, lenticular section, being normally 12 to 18 inches thick and about 3 feet wide. Some pyrite and some calcite were found, and a good deal of limonite adhered to the galena. The rock enclosing the ore is normally a very light-colored, crystalline, magnesian limestone, but near the contact with the channels it is softened and sandy. About fourteen openings had been made in an area of less than 15 acres.

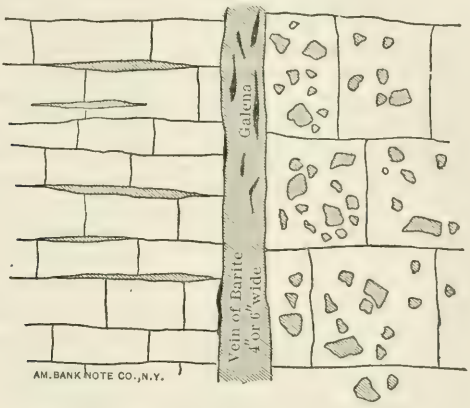
The Virginia Mine.

This Franklin county mine is, perhaps, the best illustration of a crevice or vein deposit in the State. It was discovered in 1854, and has been the scene of large operations since. To date, over 13,000 tons of lead-ore have been produced from it. The maximum depth is 480 feet. What is known as the Inge shaft is close to the old furnace, and is reported to have been 325 feet deep. From this the pits extend in both directions along a course about N. 20° W. (magnetic), and show the direction of the fissure, though this is probably somewhat sinuous. In an old shaft, about a quarter of a mile south, the vein as exposed is 18 inches to 2 feet thick, and consists entirely of barite. About 100 feet farther south, in another pit, the crevice is filled with a vein of barite 4 to 6 inches thick, containing patches of galena. The barite is of a dark-purplish color, resembling some varieties of chert, and exhibits slickensided surfaces in places. The east wall of the vein consists of massive magnesian limestone with chert, which latter does not appear in layers, but in numerous fragments, as if brecciated. The west wall consists of magnesian limestone thinly bedded, with layers of chert alternating with the limestone. These conditions are unmistakable evidence of faulting. They are illustrated in Fig. 11. The amount of throw could not be determined with the opportunities afforded for examination. The great depth to which the vein has been followed is suggestive of considerable movement, yet the comparatively undisturbed condition of the wall-rocks immediately adjacent is opposed to this inference.

This mine was described at some length by Litton in 1854. As seen by him then, the deposit occurred in a vertical fissure running nearly due N. and S., which had been traced and worked for more

than a mile. The width of the fissure varied from 1 to 15 feet, and it traversed magnesian limestone. It was filled with clay and barite, the latter both crystalline and amorphous, in which the galena was imbedded. Specimens of blue crystals of barite have been found in the dumps by the writer. At one point, between what were known

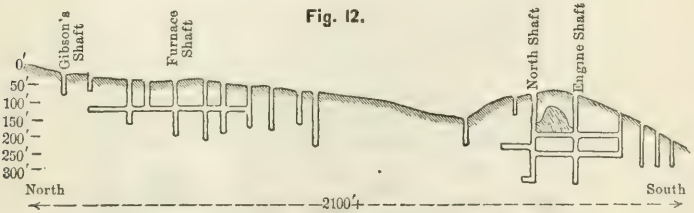
Fig. 11.



THE VEIN OF THE VIRGINIA MINE.

as the Engine and North shafts, Dr. King, who also examined this mine, describes a "vast cavern" extending from the first level almost to the surface, with an average breadth of nearly 5 feet, and from 50 to 100 feet in height; this, he states, was nearly filled with pure galena. Fig. 12 illustrates the conditions of development at that time.

Fig. 12.



LONGITUDINAL SECTION THROUGH THE VIRGINIA MINE, IN THE PLANE OF THE VEIN.

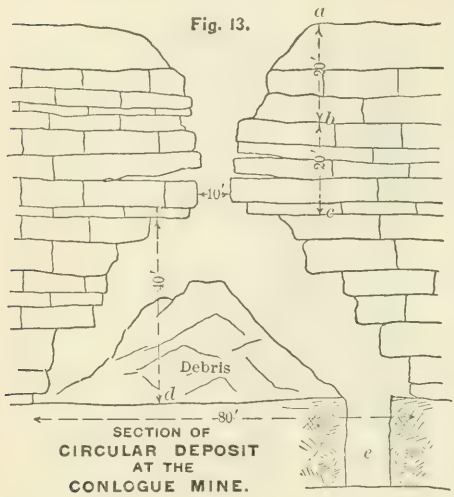
The Conlogue Mine.

This mine is situated in Miller county. Though the yield has not been very large, it is an excellent example of the so-called "circle" deposits of the central district. It is situated in the N. W. quarter of section 5, township 41 N., 14 W. More work has been done and more ore obtained here than at any other mine in Miller

county. A large amount of shallow surface-ore was found, as is attested by the innumerable pits which cover the hill-sides.

Chief interest attaches to the large open pit or conical chimney at the Pioneer shaft. This is illustrated in Fig. 13, based on actual

Fig. 13.

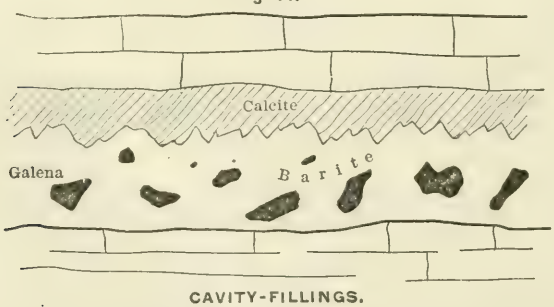


SECTION OF CIRCULAR DEPOSIT AT THE CONLOGUE MINE.

Between levels *a* and *b*, the opening was originally filled with surface clay, decomposed limestone, etc., and perhaps some galena. The narrow part, *b* to *c*, in the magnesian limestone, was filled with broken rock, clay, sand, barite and galena. Barite, and ore in quantity, are first struck here. The cavity from *c* to *d* was completely filled with ore-breccia—blocks of limestone, surrounded by clay, sand, etc., in which were galena and barite throughout, but mostly in the outer part. The sump, *e*, is said to be in clay and broken rock to the bottom.

measurements by writer. The galena, as seen now in undisturbed pillars, is firmly imbedded in calcite or barite, which line cavities; in the breccia, however, which originally filled the cavity, it is said to have been loose, and the whole mass was excavated with pick and

Fig. 14.



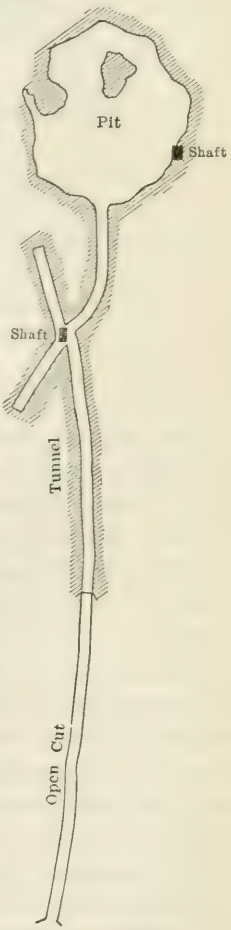
CAVITY-FILLINGS.

shovel. Sheets of barite were seen attached to the walls of the chamber, and galena is said to have been found in similar sheets, both attached to such surfaces and penetrating crevices in the wall and floor. The minerals filling cavities are disposed somewhat, as

is shown in Fig. 14. Most of the barite occurs in crystalline masses, clear and glassy, like calcite, with rhombohedral cleavage and apparently pseudomorphous after the latter mineral.

This deposit was examined by the Geological Survey in 1873, when it was being worked, and is described by Schmidt as of circular shape, 30 to 40 feet in diameter, and increasing in width with depth. The upper part was filled with red tallow-clay, and the lower part with masses of broken limestone altered and softened, though angular, with broken sheets of galena and barite between. Seams of galena extended into the wall-rock in places, and some such were followed as much as 40 feet. Seams also extended below the bottom of the shaft, but the enclosing rock became harder. Copper-pyrites and malachite were found with the barite. Galena was always deposited on the limestone while the barite filled the remaining space, sometimes in fine crystals. The whole mixture contained from 5 to 10 per cent. of galena. About 500 tons were estimated to have been mined here up to May, 1874.

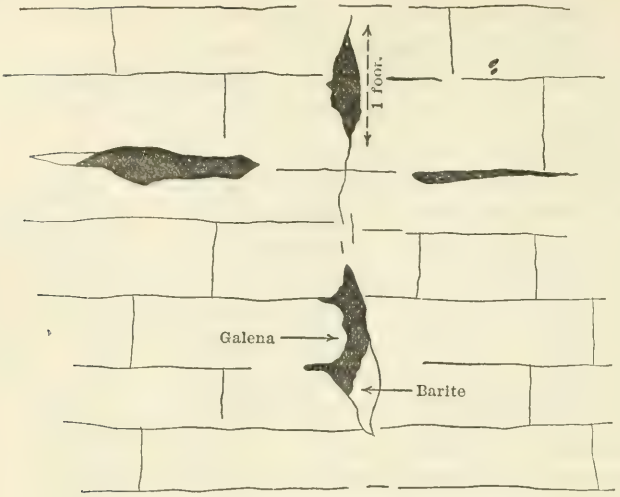
Fig. 15.



OPEN CUT AND TUNNEL OF CONLOGUE MINE.

Recently, a tunnel has been driven into the hill-side through the magnesian limestone country-rock to the circle or central pit with the idea of draining the latter and of reaching a large body of ore which was commonly believed still to remain. A plan of this tunnel is shown in Fig. 15. It followed an ill-defined vertical crevice with lens-like expansions containing galena and barite. These lenses were, in cross-section, a foot or so long and about 6 inches wide; they were also found between the horizontal beds of the wall-rock. Their disposition is illustrated by Fig. 16, from a sketch of a face in the tunnel. No faulting-movement has taken place along the vertical crevice. The galena was also found in thin sheets. In all such cases, however, it is in the solid undecomposed rock, with no clay, though barite and calcite generally accompany it. The magnesian limestone is of coarse open texture. Chert is imbedded, in nodules and lenticular layers.

Fig. 16.



ORE LENSES IN MAGNESIAN LIMESTONE.

VI.—The Mode of Formation of the Ore-Bodies.

For the purpose of this discussion we shall consider the deposits in the following order:

1. Deposits filling crevices, chambers or caverns, such as occur in all three districts.

2. Brecciated deposits of the southwestern and central districts.

3. Deposits impregnating the country-rock of the southeastern district.

The consideration of the first class resolves itself into (*a*) How were the variously shaped cavities formed? (*b*) How was the filling effected? These questions answered, the mode of formation of the breccia is the principal problem which presents itself in connection with the second class. With the third, it then remains alone to be inquired: How were the metalliferous minerals introduced?

The Formation of the Cavities.—The crevices containing ores are principally of two kinds: 1, those transverse to the strata; 2, those between the strata, and horizontal. The formation of both of these is well understood, and can be explained in a few words.

From what has already been said concerning the geologic history and structure of southern Missouri, it is plain that this has been an area of frequent crustal movements; several uplifts and depressions have succeeded each other. These movements produced strains of various kinds, resulting in fracturing, faulting and some flexing.

Faults necessarily produced vertical openings in the rock, but in addition the rocks have been fractured without faulting along many lines, forming joint-planes. Further, they are massive sediments, which, covered since their original deposition, have probably shrunk and suffered various chemical changes, resulting in similar fractures and openings.

In the intervals between different submergences, and especially since the last great Carboniferous uplift, the rocks have been exposed at the surface, and have been subjected to prolonged action of atmospheric waters and other surface-agents of decay. These waters, sinking into the ground, naturally chose the openings along faults and joints for their lines of flow. Charged with carbonic acid, and probably with organic acids also, they exerted a corroding action on the limestone wall-rocks, and soon widened the space between the planes of fracture. Where the rocks were most soluble, or the agents most aggressive, the action was quickest, and hence arose such irregularities as chambers or caverns. In some cases, irregularities of fault-plane surfaces, or brecciation caused by movements, doubtless supplemented this effect. Without proceeding to further detail regarding such well-known processes, it will be readily understood how the crevices and associated caverns have originated. The fact, well illustrated at Bonne Terre, that crevices diminish in width and die out with depth, is proof of their formation largely through surface agencies. The character of the filling of many is further evidence, as will appear later in this paper.

As regards horizontal or interbedded cavities, whether these be mere barren crevices or high and wide chambers, their origin in limestone is exactly the same as that of the vertical openings, except as to the localizing causes. Here, movements of the rocks have had probably less influence, though a horizontal slipping of the strata may have occurred at places, as a result of shrinkage or plication, producing brecciation and crushing of the rocks. Generally, however, a difference in composition has been the localizing cause. The downward flow of surface-waters along vertical crevices may be arrested by a comparatively impervious stratum, or by some obstruction just at such a stratum; the waters would then seek their way along the top of the bed, dissolving the rocks above it; or, because of differences in composition, texture or structure, one bed or portion of a bed may be more pervious or soluble than another, causing waters to flow along it and to corrode adjacent walls. This is the familiar process by which intricate passages and chambers or caves

in limestone are formed. The same has produced the cavities in which the flat or horizontal deposits of southeastern Missouri occur.

The Filling of the Cavities.—We have seen that the cavities thus formed are filled with different materials. These we have divided into gangues and minerals. The gangues are principally clays and fragments or blocks of country-rock. The minerals we have divided into lead- and zinc-compounds and accessory minerals.

The fragments of the country-rock are readily seen to be derived from the walls of the cavities. Generally they are such as are found in the immediately adjacent walls. Sometimes they may have been transported from more or less remote portions of the cavity. They are never foreign to the country. In the process of solution and decay of walls not entirely homogeneous, it is natural that more resistant portions should remain in place or fall to different positions in the cavity. Brecciation along planes of fracture and movement doubtless often supplements this action.

The clays, which sometimes fill these cavities almost completely, cannot be explained in precisely the same way. The wall-rocks contain such materials, it is true, but not in anything like a sufficient quantity for the filling to be merely residuary clay in place. The great bulk of these clays must have been transported. These we refer largely to surface-origin, and the transporting agent was water flowing in from the surface.

The decay of the rocks at the surface has been very great, not only during recent times, but also in past geologic epochs of emergence, and especially since the deposition of the Lower Carboniferous limestone. The corroded and pinnacled exposures attest this. The great deposits of clay at present found we can but regard as residuary from such decay. Surface-waters would naturally transport the finer products of this decay into the crevices and caverns of the rocks, and often, when in volume, would carry coarse material and even rock-fragments with them. The clogging of a cavity at any point would result in rapid filling. The animal remains found in the Wisconsin deposits were probably introduced in this way. During past epochs of emergence, great quantities of such surface-residuum must have been washed in. In some caverns the assorting action of water is shown by a distinct stratification of the clays. They also often fill the spaces completely from top to bottom, reaching into all the irregularities, such as pockets and crevices of the corroded roof. Only through introduction in a nearly liquid condition could such filling be effected.

In some cases, chemical deposition doubtless supplemented this process. The very pure, plastic, tallow-like clays have doubtless originated in this way. Gangues of crystalline limestone, completely filling crevices, have also been deposited from solution, as have also the cherty or quartzose matrices, which sometimes, though rarely, occur in deposits of this class.

The substances in these deposits, which we have classed as "minerals," have all been deposited from solution, generally by chemical reaction, sometimes, possibly, by concentration and evaporation. These solutions have traversed the crevices, and, on meeting with suitable reagents or suitable physical conditions, the various minerals have been deposited in familiar ways. The deposition of a mineral, once begun at any point, continues, by reason of the law of affinity or segregation governing the separation of minerals from solution. The question of the source of these solutions or of the contained minerals, we will postpone to the final section of this paper.

The Formation of the Breccia-Deposits.—The breccia-deposits of the southwest are the products of the action, on a larger scale, of the same agents as have formed the deposits filling crevices and caverns. The limestones have been dissolved by surface-waters, and the breccias are but great accumulations of residual materials.

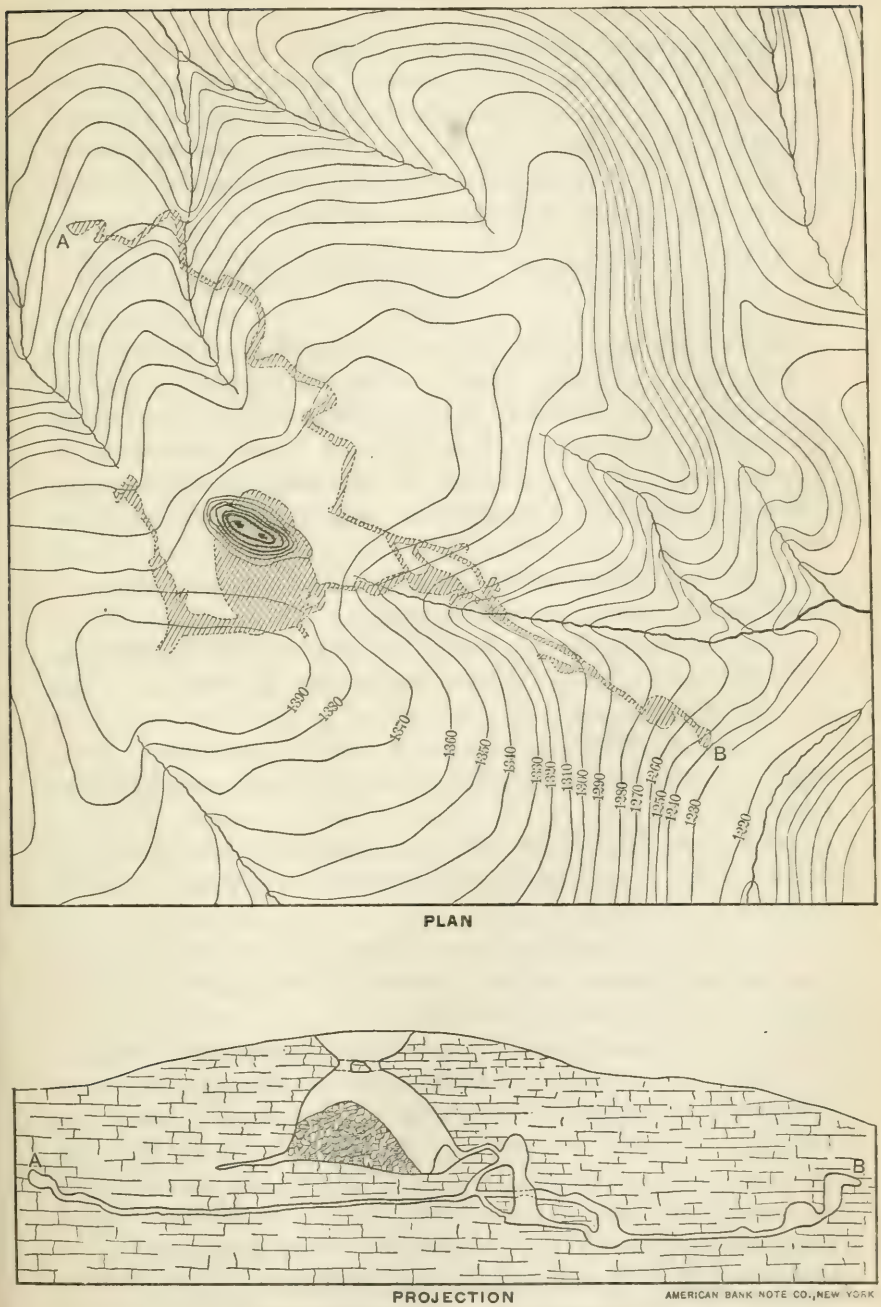
As is shown by numerous sections of the rocks, we have, in the southwest, to thicknesses of hundreds of feet, alternate layers of chert and limestone. These rocks have been affected by crustal movements producing planes of jointing and fracture, like those found in other parts of the State. The limestones here are probably more soluble and the solvents have been stronger, especially during the early part of the Coal-Measure epoch, when the district was on the margin of the swamps, and the waters were highly charged with carbonic and organic acids. The frequent alternation of beds of limestone with impervious chert beds intensified the action along certain planes. As a result, the limestones were dissolved out through great thicknesses and over large areas, and their removal caused a settling of the ground and a fracturing of the chert beds. The depressions thus produced caused a converging and increased flow of water, inducing further dissolution and removal of the limestone, and further settling and fracturing of the indestructible chert, until it was ultimately reduced to the condition of a breccia. Into these depressions, and thence to the subterranean cavities, surface-waters naturally carried the finer residues of decay, such as clays and sands, and doubtless often dragged coarser materials, thus filling all spaces.

A submergence of this deeply corroded area during the Coal-Measure epoch would bring more fine material into the interstices of the breccia, and would also give rise to the formation of overlying later deposits in the surface-depressions. Thus have been formed the shales and coal-pockets of this country, under and around which many of the so-called "circle deposits" of ore have been worked. Later subterranean solution would result in further settling under such pockets, causing the beds to assume steep dips, or even brecciating them and transporting the fragments to greater depths.

The minerals of this breccia have been introduced as have been those of crevices and caverns; the nature of the mineral varying according to the physical or the chemical conditions.

The Formation of Circle-Deposits.—To be included under the head of breccia are, furthermore, the fillings of those peculiar deposits of the central district known as "circles." These are well illustrated by the High Point mine in Moniteau county, Smith's mine in Cole county, the Conlogue mine in Miller county (see p. 667), and the Rambo mine in Dallas county. They are essentially deposits filling wide circular cavities or chimneys 50 to 100 feet in diameter, which generally widen out laterally with depth, down to certain limits, so that the space occupied within the surrounding walls has the shape of a cone. The most natural explanation of these deposits is, that they are fillings of caves which had vertical openings leading to the surface. In Fig. 17 is given, as evidence, a plan and cross-section of the well-known Marble cave in Stone county, made from actual surveys by Dr. E. O. Hovey and Mr. J. D. Robertson. The cross-section of the cavern shows that its shape is closely that of the described circular deposits. We have the same conical shape, with widening of the walls downward. In the center is a conical mass of debris, consisting of rock derived from the roof and surface. Around this is an annular space. If, now, the outlet downward from this cave were closed, and waters carrying surface materials were introduced at the top, the whole space would become filled, as are the circle-deposits. The annular space about the central cone of debris would be, subsequently, the most ready course for solutions carrying metalliferous and other minerals. Thence they would penetrate inwards into the core of the breccia and outwards into the crevices of the wall-rock. Hence we should expect the greatest concentration of these minerals in the annular space, as is the case with the circle-deposits. At the bottom of the cavity is the floor of the cave, beyond which the ore would not extend, excepting along crevices. Such a floor is also

Fig. 17.



PLAN

PROJECTION

AMERICAN BANK NOTE CO., NEW YORK

MARBLE CAVE, STONE COUNTY.

Horizontal and Vertical Scale, 1 Inch = 300 Ft.

found in the circle-deposits when exploitation has proceeded far enough.

The Impregnated Deposits of the Southeast.—Concerning these deposits, we will only say that the minerals have been introduced into massive beds of magnesian limestone by solutions, and have replaced particles or grains of the rock by the process of metasomatic interchange. The reasons for this conclusion and the origin of the solutions we will consider under the next heading.

VII.—The Origin of the Metalliferous and Other Minerals.

In what precedes, we have taken for granted the introduction of the minerals in solution. This is so generally accepted by authorities, regarding not only these but the majority of other ore-deposits also, that the assumption is warranted; and we will pass at once to the final and important question, Whence came the solutions and contained minerals? The principal hypotheses advanced are as follows:

1. Original or simultaneous deposition with the rocks, in a concentrated condition, from oceanic waters.

2. Derivation from great depths in solutions ascending through profound fissures, from which the minerals were deposited in cavities or impregnated the rocks.

3. Original diffusion through the country-rocks, and subsequent concentration by percolating waters, by the process known as secretion.

4. Original diffusion through the country-rocks and subsequent concentration through surface-decomposition of the latter, supplemented by percolating waters.

The last is the hypothesis here advocated. Before entering upon argument in its support we will, however, briefly present our objections to the acceptance of the others.

1. *The Theory of Original Deposition in a Concentrated Condition.*—This mode of formation is evidently out of the question with those deposits filling cavities or filling interstices of breccia formed subsequently to the rocks in ways already described. It is, therefore, only applicable to such deposits as occur in beds either in the diffused or disseminated form or in massive sheets. In Missouri, galena alone occurs in those forms. Now, though it may be chemically possible for galena and such rocks as are found in the southeast to be deposited together, it does not seem possible under natural condi-

tions. It would require a greater quantity or greater concentration of metalliferous minerals in the sea-water than exists or probably ever existed. Animals could not have lived in such solutions. If the ores were so derived, we should expect the deposits to be of wider and more generally uniform distribution than they are, especially where the lithological characters are similar and the evidence of diffusion of organic matter is about equal. Finally, if other forms of deposits in the State, with some of which these disseminated or flat sheet bodies are closely associated, cannot be explained on this hypothesis, the adoption of a different explanation of origin merely because of difference of form is not philosophical. The closest approximation to this hypothesis is Whitney's explanation of the Wisconsin ores.* He, however, regarded subsequent segregation as necessary. Broadhead† has suggested that the galena in the state of solution replaced "portions of the limestone beds which had previously been softened by acids." He states that the limestones were probably first formed, but whether the solutions referred to were oceanic or traversed the rocks in crevices is not stated.

2. *The Theory of Derivation from Great Depths, etc.*—This hypothesis claims consideration because, with many ore-deposits, it is doubtless a true one. Its strength lies in the probable preponderance of metals at great depths beneath the surface, and in the considerations that subterranean waters are of high temperature and under great pressure, and are consequently powerful solvents; that the relief of pressure and the diminution of temperature accompanying the ascent of such solutions would naturally cause the deposition of minerals. The obstacles to the acceptance of the hypothesis in the present case lie in local conditions. It was originally advanced for the upper Mississippi valley deposits by Owen‡ and Percival.§ It has since been argued, with modifications, by others. The most recent advocacy has been in papers, published within a year, by Prof. F. Posepny|| and Mr. W. P. Jenney.¶ The conclusions of these papers have already been considered by the writer in the discussions, and also in an article in the *Journal of Geology*.**

* *Report Wis. Geol. Sur.*, 1862.

† *Trans.*, v., 104.

‡ *Rept. Geol. Explor. of a part of Iowa, Wis., and Ill.*, 1839.

§ *Ann. Repts. Wis. Geol. Surv.*, 1855 and 1856.

|| "The Genesis of Ore-Deposits," *Trans.*, xxiii., 197.

¶ "The Lead- and Zinc-Deposits of the Miss. Valley," *Trans.*, xxii., 171.

** "Notes on the Lead- and Zinc-Deposits of the Miss. Valley, and the Origin of the Ores," *Jour. of Geol.*, vol. i., p. 612.

We will not repeat the discussion here, but refer the reader to the originals. We consider the hypothesis inadequate and incompatible with the phenomena and geologic history of the region. To the arguments already advanced may be added that many, if not all, of the crevices contract and diminish in ore-contents with depth, and are often essentially surface-phenomena. Whitney long since called attention to this fact.* The Wisconsin ores, which are found at different horizons from the Lower Magnesian to the Galena limestones, do not simply extend through the upper strata down to the lower, but characterize the exposed portions of these formations, and this only to certain limited depths.

3. *The Theory of Original Diffusion Supplemented by Lateral Secretion.*—The application of this theory to the explanation of ore-deposits in general has been quite prevalent during the past twenty years. It was stimulated principally through Sandberger's investigations and publications upon the presence of metals in rock-forming minerals. In this country, Emmons's monograph on the Leadville deposits has done more than any one other work to bring the theory into prominence. Sandberger's and Emmons's investigations were prompted and sustained by results relating to the presence of metals in rocks reached by Forchhammer,† Bischoff, Dieulafait,‡ Richardson,§ and others, who found minute quantities of lead or zinc and other metals in rocks of all kinds. Lead and zinc occur, moreover, in a large number of minerals of which they are not essential constituents; they are also detectable in sea-water, in deep-sea deposits, in many mineral waters, and even in plants and animals, all of which may have been drawn upon to supply deposits.

During recent years, however, strong opponents to the lateral-secretion theory have appeared, and something of a reactionary movement has obtained; objections and obstacles have been raised, and in some cases, doubtless, the inapplicability of the theory has been shown. The sufficiency of the metalliferous contents of the country-rocks has been questioned. Prof. A. Stelzner‖ investigated the methods of analysis, and concluded that Sandberger had not determined decisively whether the metals in the minerals were original

* *Rept. Wis. Geol. Sur.*, 1862.

† *Pogg. Ann.*, xcv., p. 60.

‡ *Ann. de Ch. et de Ph.*, xxi., p. 256, 1880.

§ Fremy, *Ency. Chimique*, art. "Zinc," p. 3.

‖ Prof. A. Stelzner, "Die Lateralsecretions-Theorie," *Jahrbuch der k. k. Bergakademien zu Leoben und Przibram*, 1889, xxxvii., p. 1.

constituents or secondary impregnations derived from the ore-deposits. This is, of course, a very difficult thing to prove positively, yet the presence of the metals diffused through rocks at localities remote from deposits, as will be instanced later, is strong presumptive evidence that they are original constituents.

In a recent paper on the origin of lead- and zinc-deposits in soluble rocks,* which has come to the writer through the kindness of Professor Posepny, the latter lays stress upon certain objections to the lateral-secretion theory, which seem to him insuperable. Briefly stated, these are: 1, that sulphides are not formed in the presence of air, and, therefore, cannot have been deposited in the surface or "*vadose*" region, and must have been formed at depths (pp. 6 and 54); 2, that lateral-secretion is only conceivable in the *vadose* regions where the solutions can flow into cavities, and there only oxidized compounds could be deposited (p. 54); 3, that in none of the "thousands of analyses" of limestones remote from ore-deposits has the presence of foreign metals been shown (p. 44); 4, that it does not seem possible that the whole array of minerals which are found in ore-deposits in such rocks, including Pb, Zn, Ag, Au, Cd, Fe, Mn, Co, Ni, etc., could have come from different sources and directions than the metals of other ore-deposits.

Regarding the first objection, we can readily conceive air to be excluded from the interstices and even cavities of rocks near the surface, and oxidation to be *nil*. Though it is probably true that oxidation is exclusively a *vadose* phenomenon, the reverse does not follow *a priori*, nor does experience teach us that it does. Posepny himself cites (p. 54) the formation of sulphides through the influence of organic matter; and, where the latter is abundant at the surface and in the proper condition, deoxidation and the deposition of sulphides may prevail.

As to the second objection, deposits formed by lateral-secretion are not necessarily cavity-fillings; segregation and replacement of rock by metalliferous solutions is, at least, as possible with this as with other processes.

Professor Posepny's statement concerning the absence of foreign metals in limestones is difficult to reconcile with the results of Forchhammer and others previously referred to. Just what analyses he refers to, we do not know; of course, many thousands have been made of limestones in which no thought was given to the detection

* *Ueber die Entstehung der Blei und Zinklagerstätten in auflöslichen Gesteinen.* By F. Posepny, Ber. Allgem. Bergmannstag zu Klagenfurt, 1893.

of these metals, and which, moreover, were not sufficiently refined to reveal the presence of very minute quantities. Mr. Robertson's results, quoted later, suggest that this may be the explanation. The statement certainly does not hold true for the limestones of the Mississippi valley.

Concerning the final objection, the mere presence of a long array of metals in deposits of the class under consideration seems to the writer to have little or nothing to do with the questions of their immediate origin and mode of accumulation. Their relative quantities have, perhaps, more direct bearing. On any theory deserving recognition, all the metals of our ore-deposits are constituents of the earth's crust. It is natural to infer that they would be diffused through its rocks as well as segregated in deposits, and that the amounts so diffused would be somewhat proportional to the solubilities of their more common compounds.

Some opponents and some advocates of the theory of lateral-secretion have, apparently, a narrow conception of it. They would restrict the source of the ores to the rocks immediately contiguous to the ore-body and allow only lateral flow of solutions. A broader conception of the theory permits the minerals to have been derived from any or all of the country-rocks, and recognizes that solutions may traverse these rocks in all directions, according to local conditions. Thus, the solutions may, in one case, rise, and, in one sense, come from below; in another case they may descend, and thus come from above; again, they may flow into a cavity from the sides. The distribution of the minerals in any body, whether attached to the roof, sides or bottom of a cavity, is, hence, immaterial. The theory may thus grade at times into that of the derivation by solutions from depths. A main point of difference is, that the flow of the solutions is generally controlled by ordinary hydrostatic conditions rather than by volcanic agents.

Some opponents of the theory decry it upon the grounds that it is a convenient one, and can be argued from the laboratory and office without the trouble of field-work and mine studies. We see no justification for this accusation. Knowledge of the actual conditions is as necessary in the one case as in the other.

To explain the Upper Mississippi valley ores, lateral-secretion has been used to some extent by Whitney, but principally by Chamberlin.* Even with the latter, however, it was secondary, the original

* *Report Wis. Geol. Sur.*, 1877, vol. iv.

concentration being attributed to oceanic currents. That it is a factor in the formation of these deposits we also recognize. But that it is *per se* sufficient to supply the great bodies of ore of many of the Missouri deposits we do not believe, even though such local concentrations exist as Chamberlin's hypothesis would provide for. The country-rocks surrounding the large ore-bodies do not present the leached appearance which would seem a necessary result.

A modification of this theory has been proposed by F. L. Clerc.* He advocates the view that the ores were derived from the patches of Coal-Measure shales which are so abundant in southwestern Missouri. As instrumental in effecting this result, he requires a Quarternary submergence, during which the ores diffused through these shales were leached out and deposited in the underlying breccias. An objection to this proposition is, that such Coal-Measure pockets are by no means always associated with the deposits. Moreover, they are not readily traversed by waters, being, on the contrary, dense and impervious. Finally, if derived from these rocks, we should expect to find bodies of ore in the interior of the Coal-Measures.

Mr. Carl Henrich, in a recent description of the Webb City deposits,† refers to the bitumen of the limestones as instrumental in the reduction and deposition of the sulphides.

Mr. Nason, from his observations in Missouri, is opposed to the idea that the lead- and zinc-ores were derived from the depths.‡ He states that there is no evidence of deep-seated thermal springs, and that all the evidence is in favor of a downward circulation of the waters and of a transmission of metalliferous compounds from above to the lower levels.

4. *Concentration through Surface-Decomposition.*—This hypothesis, which we now formally advance, starts with the proposition that the metalliferous minerals originally existed in the Archean rocks, either in a diffused condition or in veins. With the decay of these early-formed rocks the minerals became diffused through later sediments, this diffusion being quite uniform over contiguous areas. The successive decay of successively-formed rocks simply resulted in a transfer of these minerals.

This hypothesis so far agrees with Chamberlin's, in that it recognizes the presence of minerals in the country-rocks and the deriva-

* *Lead- and Zinc-Ores of S. W. Mo.*

† "Zinc Blende Mines and Mining, near Webb City, Mo," *Trans.*, xxi., 3.

‡ Discussion, *Trans.*, xxii., 636.

tion of the deposits from them. It differs, however, in maintaining a condition of general diffusion rather than one of concentration over certain favored areas. Chamberlin's hypothesis of oceanic currents has always seemed to the writer inadequate and too theoretical. It does not meet the facts of the differences in the ages of the rocks in which the deposits are found. His diagram exhibits the currents of Silurian times, while the deposits of southwestern Missouri are in Lower Carboniferous rocks.

According to our theory, the concentration is entirely subsequent to the formation of the rocks. It is primarily the result of great and long-continued surface-decay of the rocks, and secondarily the result of locally favorable physical and chemical conditions, important examples of which we will enumerate later.

Recent studies of Mr. W. P. Blake in the Wisconsin area have caused him to express views in a recent paper* which, as the writer interprets them, must lead to similar conclusions. He recognizes the presence of faults there, but does not seem to see that they indicate a deep source for the ore. On the contrary, he regards the evidence, all things considered, as favoring lateral secretion combined with a *descent* by solution. He favors an original general dissemination of the ores in the masses of the strata, "from which during decomposition they are drawn or leached and finally concentrated in the fissures or crevices." The method of doing this he expresses in the following words (p. 628):

"The evidence is strongly in favor of the view of the long-continued decomposition, downward flow and re-composition of not only the ores of zinc, but of lead and of the pyrite from the upper formations to the lower, as the general water-level of the region subsided, and as the upper formations by long-continued exposure through geologic ages were gradually decomposed in places. By such a process the present zinc-deposits would seem to have accumulated and to represent the originally diffused ores in many formations, possibly as high in the geologic scale as those of Missouri or the Lower Carboniferous. This is, however, improbable, owing to the dense and impervious nature of the intervening Hudson River shales."

Faulting, Mr. Blake considers an indirect cause in localizing the ores. The original deposition of the sulphides from the sea he attributes to the decomposition of organic matter. He suggests that an incursion of hydrocarbon, perhaps, caused by dislocations, may have also produced this result. The presence of the bituminous shale, known as "oil rock," at the base of the Trenton, he empha-

* Discussion, *Trans.*, xxii., 621.

sizes as significant in this connection. The ore-bodies often terminate with this and spread out along the upper surface of it. If it did not determine the original deposition of the ore, he concludes that it certainly had a strong secondary influence.

With reference to the surface-decay of the rocks, he makes the following statement, of special interest in this connection:

"It is a significant fact that the lead-producing region has not been glaciated, while the glaciers extended over the adjoining regions. This evidence is in favor of the view that the deposits have been formed by leaching downward, the unglaciated area having been long exposed to atmospheric agencies and decomposition, without removal, while the rocks were decomposed *in situ*. Similar conditions prevailed in Missouri, where, according to Dr. Jenney, the land has been above water from the beginning of the Carboniferous period to the present time."

To place the hypothesis on a firm foundation, it is necessary, first, to present evidence of the truth of the fundamental proposition that the metals are diffused through the country-rocks.

In Chapter II. of the *Survey Report* we have made certain statements and have quoted certain results indicating the general diffusion of minute quantities of lead and zinc in minerals, rocks, and ground-waters. These we have briefly referred to on page 678 of this paper. To bring the evidence nearer home, a series of examinations of Missouri rocks was undertaken. These were conducted by Mr. Robertson with great care and after much preliminary experimentation.* Duplicate and sometimes triplicate analyses were made in order to confirm the results. Working with such bulky samples as were necessary, a great amount of time and labor was involved. The samples were mostly collected by the writer in person, and the analytical work was done in the survey laboratory. Some were collected at considerable distances from known bodies of ore, and some are from points outside of the mining districts entirely. An abstract of the results of this investigation is given in the table on the following page.

Similar small quantities of copper, manganese, and barium were found in all of the specimens. Full details of the results of analysis and descriptions of the specimens will be given in the *Report of the Geological Survey*.

This investigation is a convincing demonstration that the important elements of the ore-bodies in question are present in the rocks.

* A description of the methods of analysis pursued is given in Appendix B of the *Report of the Geological Survey*.

Metalliferous Contents of Missouri Rocks.

(J. D. Robertson, Anal.)

	Lead.	Zinc.
Archean Rocks: Range of 8 Analyses of 4 Specimens...	Per cent. 0.00197 to 0.00680	Per cent. 0.00139 to 0.01760
Silurian Magnesian Limestones: Range of 12 Analyses of 6 Specimens...	Trace to 0.00156	Trace to 0.01538
Lower Carboniferous Limestones: Range of 15 Analyses of 7 Specimens...	Trace to 0.00346	Trace to 0.00256

The amounts of metallic lead vary from about .0.0004 to 0.007 per cent., of metallic zinc from about 0.0002 to 0.018 per cent., and of copper, manganese, and barite there are correspondingly small amounts. The largest percentages of the metals appear to be in the crystalline rocks. The number of analyses is not sufficient to establish this finally; but, if true, this relation is in harmony with the idea of the derivation of the metalliferous contents of the limestone from the pre-existing crystallines. Were the metalliferous contents due to secondary impregnations from fissures, we should expect to find a smaller proportion in the comparatively impermeable crystalline rocks. The presence of both lead and zinc in a specimen of limestone from Louisiana, which is north of the Missouri river and remote from the mining districts, supports the belief that these metals are generally diffused. Moreover, the amounts contained are greater than in some of the limestones from the very center of lead- and zinc-mining.

Averaging the contents of the limestones, we obtain the following results:

.001009 per cent. metallic lead = .00117 per cent. galena.

.00239 per cent. metallic zinc = .00357 per cent. blende.

This is equivalent to:

0.00198 pounds galena to 1 cubic foot of rock.

0.00603 pounds of blende to 1 cubic foot of rock.

From this the contents of different areas may be calculated as follows:

87 pounds galena per acre, 1 foot thick.

27.8 tons galena per square mile, 1 foot thick.

13,900 tons galena per square mile, 500 feet thick.

261 pounds blende per acre, 1 foot thick.

83.6 tons blende per square mile, 1 foot thick.

41,500 tons blende per square mile, 500 feet thick.

From these figures we see that the metalliferous contents of 500 feet of limestone, covering about 3 square miles, is equal to our annual production of lead- and zinc-ores; while the contents of about 70 square miles equals the total production of lead-ore, and the contents of about 30 square miles the total production of zinc-ore to the present date.

It thus appears that, on this hypothesis, which does not require that the ores of the deposits should come from the immediately adjacent walls, the metalliferous contents of the country-rocks are ample to supply our ore-bodies.

The fact that the relative amounts of blende and galena in the rocks are proportional to the present annual production of these ores, must be regarded as a coincidence. Were the relation maintained in the case of the total productions, and were lead- and zinc-ores both equally abundant in all of the mining districts, the fact would be a strong confirmation of the hypothesis.

The combinations in which the metals and other minerals exist in the rocks could not be determined with the means at hand. It is probable that these are carbonates, sulphates, sulphides, and silicates. Whether one or all of these, it may be assumed that, with the general decay of the country-rock, these minerals would be affected also and would pass into solution, at least in part. A portion of such solutions would flow off in surface-waters and streams, and would be transported long distances to be emptied into some lake or lagoon or to pass into the ocean. Portions would percolate downward and enter the rocks. Where crevices existed, or where depressions were formed, the waters would converge and the flow would be more abundant. Where physical and chemical conditions were favorable, deposition of minerals would take place, the amounts being proportional to the strength and volume of the solutions. Favorable physical conditions are open cavities, inter-fragmental spaces of breccias, pores in massive rocks, etc. Favorable chemical conditions are, principally, the presence of reducing agents, such as organic matter, which was doubtless the main agent in the precipitation of the ores.

These facts determined, and conceding that the above outlined processes are not only possible but reasonable, what evidence do we find in the geologic history and conditions of the deposits that such has been their mode of formation?

In answer to this question, we direct attention again to the fact that this region has been one of great and long-continued subaerial decay during different geologic periods. From the beginning to the present time, many hundreds of feet of rock must have been removed. That the Wisconsin area has had a similar history, is confirmation of the belief that this cause has been influential. The great length of the erosion-period is referred to by Blake, and was noticed by Whitney, Strong, and Chamberlin; but its full bearing upon the ore-deposits does not seem to have been appreciated. The derivation of the deposits from the whole mass of the country-rocks, and not simply from those immediately adjoining the deposits, answers an objection raised by Dr. Jenney to the theory of lateral secretion; and the source of the magnesia of the dolomites of the southwestern Missouri ore breccias is thus also readily explained. Moreover, the theory accounts for the essentially superficial nature of most of the ore-bodies. This condition exists in Wisconsin, and is described by Whitney, Chamberlin, and Blake. It is also, undoubtedly, a characteristic of the Missouri deposits, though the disseminated ores of the southeast seem exceptions. Through the derivation of the minerals from above, we can explain the filling of those crevices and other cavities found in Missouri, which diminish in size and ultimately terminate downward. Finally, this theory offers an explanation for the association of bulky deposits, like those of lead and zinc, with the comparatively soluble limestones, a fact observed and commented upon by all students of ore-deposits. The decay of these rocks in large volumes furnishes correspondingly large deposits.

These are the general conditions which are in harmony with the hypothesis. Next to be considered are the local and special conditions. Do they yield additional support? Or, putting the question in another form, are they readily explicable in this way? We think so, in great part at least. The occurrences being local and special, however, are attributable to special conditions prevailing at certain times and places.

Taking, first, the southwestern Missouri district, we have there great bodies of breccia impregnated with ore. These breccias fill large spaces or "caverns" in the country-rock which have resulted from surface-decay. The breccias are composed of residuary materials, associated with metalliferous and other minerals. They are in Lower Carboniferous limestone, and must hence have been formed later. We have already emphasized the fact that, immediately after

this epoch, before or during the early Coal-Measures, there was a time of vigorous erosion. This doubtless affected not only the marginal Lower Carboniferous rocks, but the Silurian magnesian limestones of the interior also. With the emergence, after the deposition of the Lower Carboniferous limestones, the rainfall was probably great and drainage abundant. The geological map shows a tongue of Lower Carboniferous extending from southwestern Missouri eastward into the Ozark area. This probably marks the site of a former estuary towards which drainage converged. Thus, a great volume of water probably flowed into this basin, bringing with it the products of decomposition of the magnesian limestones and other rocks to the east. To this source we attribute the magnesia which dolomized the Carboniferous limestones in the earlier stages of the erosion of the Jasper county area. After this, as the Coal-Measure epoch developed, vegetation became dense, swamps were formed, vegetable matter began to decompose, and coal-beds to accumulate. Here, then, were furnished almost perfect conditions for the reduction of the ores in solution. Already great quantities of residuary materials had accumulated from the decay of the Lower Carboniferous rocks *in situ*. In addition, the drainage was constantly supplying new solutions and matter in suspension from a distance; and upon all of these the organic matter acted.* Ideal physical conditions were furnished by sink-holes, caverns, etc., filled with breccia, as have been described. Under these circumstances, the deposition of the ore must have been rapid and abundant. The silicification which gave rise to the secondary cherts of the ore-bodies must have taken place at the same time or shortly after. To the post-Lower Carboniferous or early Coal-Measure epoch, we therefore assign the formation of the principal deposits of southwestern Missouri. Others were doubtless formed at other times, and later additions may have been made to these, but that was the time of greatest activity. These were the special conditions of time and place which supplemented the general conditions of surface-decay in producing the deposits of the southwestern district.

The origin of those deposits of the southwestern, and also of the

* As bearing upon the chemistry of the process, we quote the following from Bischoff's *Chemical Geology:*

"Water saturated with carbonic acid dissolves $\frac{1}{1108}$ of its weight of artificial carbonate of zinc. Sulphuret of potassium precipitates from this solution white sulphuret of zinc. If, therefore, water contains alkaline or earthy sulphate, organic matter, and carbonate of zinc, the conditions for the formation of zinc-blende are complete."

central and southeastern districts, which occur in vertical crevices or cavities of other shapes and attitudes, we ascribe to the convergent flow of waters which had leached surface-residues and had passed through decomposing rocks. The cavities furnished the requisite physical conditions, and the chemical condition necessary was the presence of organic matter. This probably existed in all epochs, but was most abundant during the Coal-Measures; and this was doubtless the time of maximum enrichment of these deposits also. The finding of galena and blende within the very coal-beds of outlying marginal coal-pockets shows the influence of carbon in their deposition.*

The distribution and character of the disseminated ores of the southeast are more difficult to explain. Though they are often mined at considerable depths, they are also found near the surface. One of the principal determining causes, we think, was the original open structure of part of the rock. This is often observable now, and especially characterizes ore-bearing strata. Second, a prevalence of organic matter in certain strata or along certain horizons, as indicated by a darker color now often seen, had doubtless some influence. Again, the various shale-beds probably limited and guided the flow of the solutions, as referred to in describing the Bonne Terre deposits above.

The vertical and other crevices undoubtedly acted as channels for the flow of the solutions. The sheets of galena frequently found in these crevices prove that the solutions followed them. These were sufficient to supply the ore of higher-lying disseminated bodies. The contraction and disappearance of the crevices with depth makes them inadequate for explaining the deep deposits, such as prevail along Flat river. For these ores we suggest the underlying sandstone, which is in close proximity, as the solution-carrier. This is saturated with water, much of which flows directly from decomposing crystalline rocks. The sandstone itself contains particles and fragments of these rocks, which must hold more or less of the metals.

* The question may suggest itself here, why, if the Coal-Measure epoch was so favorable for the deposition of ores, such are not found in bodies within the coal-area proper. Our answer to this is, that as the metalliferous contents and products of decomposition of the rocks were reduced and precipitated in the interior of the Ozark uplift or along the margin, little remained to be carried far from the shore-line and that little became diffused in the Coal-Measure seas. Instances of the occurrence of both lead- and zinc-compounds in small quantities are, however, common in the Coal-Measures; those of Miami and Linn counties in Kansas are the most noteworthy.

PLATE I.—SECONDARY CHERT GANGUES.

Fig. 1.—Vesicular chert from Corry Mine, Dade County.
" 2 and 3.—Breccia of fragments of white, original chert, held in dark secondary chert; from Galena, Kansas.
" 4.—Dark secondary chert from which dolomite crystals have been dissolved out; from Snyder Bro's. Mine, Joplin.
" 5.—Breccia, showing partial solution of orginal white chert.

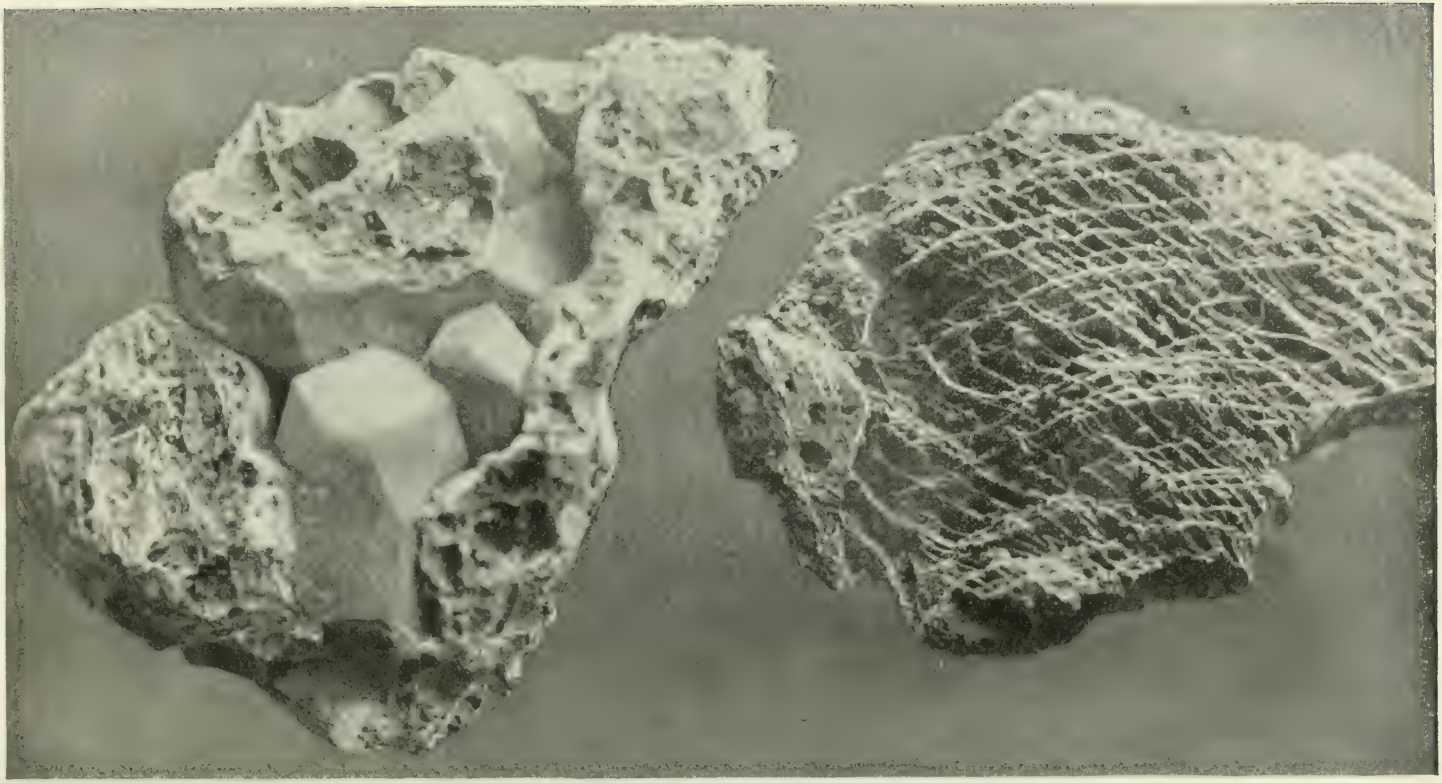


PLATE II.—PSEUDOMORPH OF SMITHSONITE AFTER CALCITE

(Specimens about four inches in diameter.

PLATE III A.—(SEE TITLE OF PLATE III B.)

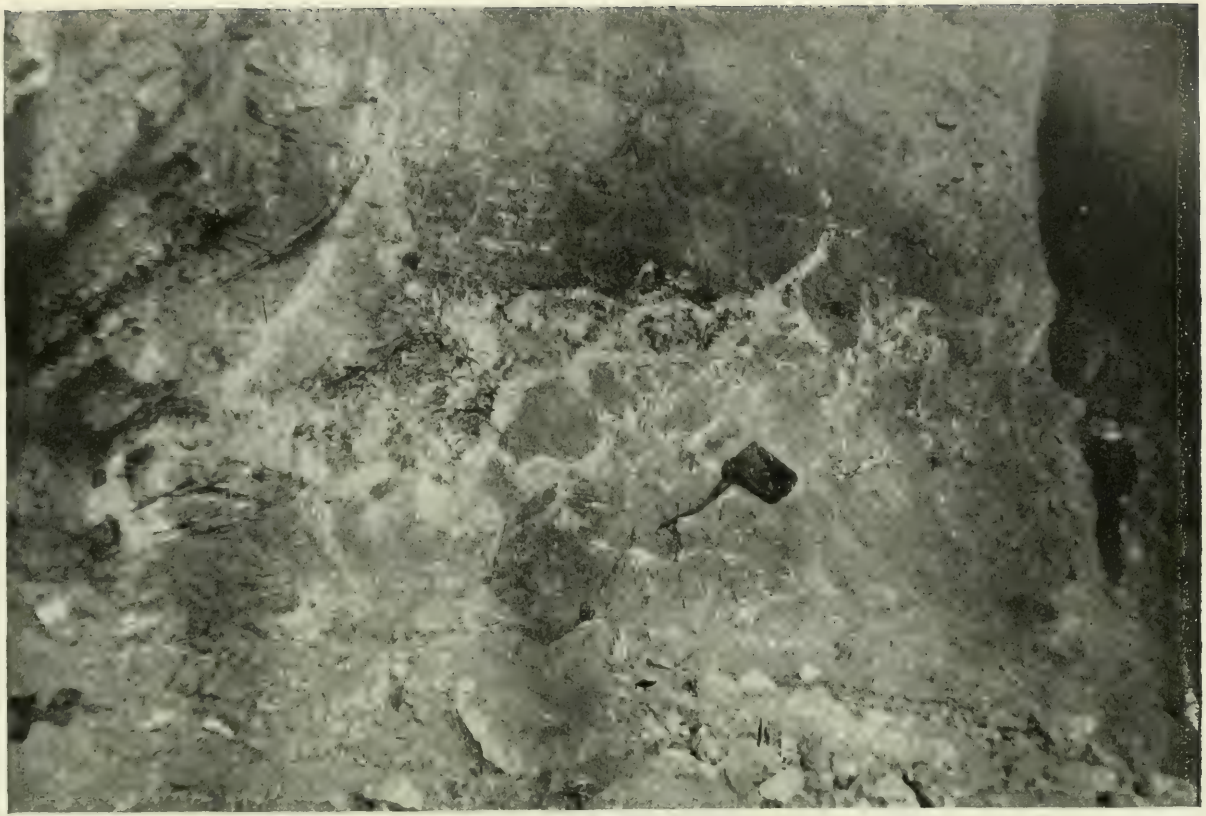


PLATE III B. (SEE ALSO PLATE III A.)—BRECCIA AND STOCKWERK AT THE FRUMET MINE IN JEFFERSON COUNTY.
The rock is magnesian limestones; the vein filling, baryte, inclosing galena.

A downward flow of water towards Flat river and Bonne Terre is induced by reason of the slope of the Archean floor, and also probably by the Farmington anticline to the east, which we have illustrated in Fig. 2. The water is thus under pressure sufficient for it to rise up through the limestone and, where suitable physical and chemical conditions are reached, the deposition of the ore can take place.

We are inclined to think that the formation of the ore is still in progress; the finding of galena on calcite crystals already referred to (p. 662) is evidence of this. In the southeast, as elsewhere, however, the Coal-Measure epoch furnished probably the most favorable conditions. Decomposing organic matter was doubtless introduced into the rocks through solutions. The proximity of the Illinois coal-field had probably also a localizing influence.

We have thus given in some fulness a statement of the causes and processes involved in the formation of these deposits. We think the conclusions rest upon a good foundation of facts. Still, there is much which it is difficult, if not impossible, to explain with our present knowledge. Much must be left for future investigation; but we feel that these results will be serviceable in directing it. The causes controlling the localization of the deposits should be inquired into. Additional analyses should be made which will permit generalization as to the distribution and relative quantities of the metals in the different rocks, and as to the combinations and conditions in which they exist. Careful examination of residuary clays will furnish valuable data. Observations should be continued to detect whether ores are at present in process of formation, especially galena and blende. If so, why do we find the observed paragenesis of the minerals? The chemistry of the processes of solution and deposition of the ores needs further investigation. These are some of the problems to be attacked; others will suggest themselves to the careful reader and observer.

THE MINES OF THE CHALANCHES, FRANCE.

BY T. A. RICKARD, DENVER, COLORADO.

(Bridgeport Meeting, October, 1894.)

In southeastern France, among the magnificent alpine masses of the Dauphiné, there is a group of celebrated mines of silver-, nickel- and cobalt-ores, the deposits of which present many features

of interest. In 1891 I directed the work done at the Chalanches, and at that time made the notes upon which the present paper is based.

The workings consist of a complex series of adit-levels entering the heart of a mountain, the summit of which is one of the lower peaks of the Belledonne, in the commune of Allemont, and overlooking the valley of the Romanche and its tributary, the Eau d'Olle. The lower adits are 3700 feet above the village of Allemont, or 7250 feet above sea-level.

Historical.

The discovery of these, as of many other notable mines, was accidental. In 1767, Marie Payen, a shepherdess (bergère) of Allemont, found an outcrop of silver-ore, and brought away, in ignorant curiosity, a lump of heavy stone, which she handed to the village smith. When tested on his forge, the molten silver trickled from it. The shepherdess received 600 francs upon her wedding-day as a reward for the discovery.* Thereupon some of the peasants of the commune began to dig and to smelt the silver-chloride ores, the croppings of which they traced down the mountain-side. The exploitation thus inaugurated continued with but little interruption for more than a century.

The first mining done was digging.† A loss of life through a crush in the excavations caused the authorities at Grenoble to take official cognizance of these operations; and M. Lemonnier, a member of the Academy of Sciences, was sent by order of the king to investigate the discoveries of silver reported to have been made by the peasants.

In 1769, systematic work was commenced under the direction of a Piedmontese engineer, M. Binelli.‡

On the 10th of June, 1776, the king granted a concession of these mines, as well as those of Allemont and La Gardette,§ to his brother,

* There is an entry on the pay-sheet for September, 1768, made in the handwriting of M. Schreiber: "Paid to *bergère* Marie Payen, at her marriage with Jean Roux, master charcoal-burner at the *fonderie* d'Allemont, the sum of 600 livres as recompense for discovery of the mine."

† "Gophering" or "coyoting."

‡ For many of these data I am indebted to an *Extrait des Mémoires de la Société des Ingénieurs Civils* by M. Alfred Caillaux.

§ See "La Gardette: The History of a French Gold-Mine," by the writer. *Trans.*, vol. xxi., p. 79.

the Comte de Provence, afterwards Louis XVIII. Smelting-furnaces were erected at the base of the mountain near Allemont.

In 1781, Binelli gave place to Schreiber, who assumed the direction of the smelting establishment and of the various mines. Schreiber's name is interwoven with the history of most of the mines in this part of Europe. He was a Saxon engineer of much ability, and was the father of the *École des Mines*, which was first established at Moutiers in Savoy.

On the 2d of August, 1792, as a consequence of the Revolution, the mines became national, and passed into the hands of the new government.

The years intervening between 1776 and 1791 cover the most prosperous period in the history of the mines. In 1791, the amount of development-work was decreased, and the profits dwindled away under bad administration. The silver produced was sent away, but the amount of money necessary to pay for the work was not forwarded to the mines. Accounts were liquidated with *billets* or promissory notes instead of cash. Matters went slowly from bad to worse until 1807, when the State abandoned operations and made a concession of the mines to a public company.

From the first discovery up to 1801, a period of thirty-two years, the mines produced 9453 kilos, or 303,914 Troy ounces of silver. According to Schreiber, the receipts were 2,098,421 francs, or 65,577 francs per annum. The expenses were 1,890,096 francs, or 59,090 francs per annum. During this period there were times when the lodes became very much impoverished, and the profits were further diminished by the erection of reduction-works and buildings and by other outlays. There remained, nevertheless, a net balance of 207,585 francs. The best years were 1784 and 1785, when the profits amounted to 55,000 and 54,000 francs respectively.

In 1808, Schreiber became director of the *École des Mines* at Moutiers,* and ceased to manage the Chalanches. In 1809, a new company obtained control of the mines through an imperial decree. It was short-lived, and was succeeded by a series of other companies, which did more or less work in an unsystematic manner up to 1873. In 1889, application was made by Pierre Manin for a concession of the then abandoned mines. A company, *La Société*

* Sometimes spoken of as the "*École des Mines de Pesey.*" The mines of Pesey and Macot, in the hills of old Savoy, were the cause of the choice of Moutiers, the nearest town, as the locality for the first school of mines.

Savoysienne,* was formed to operate mines in Savoy and in the Dauphiné, including those of the Chalanches. This ambitious enterprise exhausted its energy in making large promises, and, after a brief existence, was sold out for the benefit of its creditors. The Countess de Grailly became proprietress of the Chalanches, and of Grand Clos, the mines of which have always been worked in conjunction with the former.

In 1890 the mines passed into the hands of an English Company, the "French Mines, Ltd.," and vigorous work was commenced at the Chalanches, as well as at Les Arabes, Villaret and Grand Clos. Owing to the fact that the other mines of this extensive group offered better inducements for the investment of capital in their development, the Chalanches was operated to a limited extent only. The exploratory work led to the discovery of several rich pockets of silver-ore and small patches of nickel- and cobalt-ores. On the whole, however, the results were not such as to encourage further work, and in September, 1891, operations ceased. These interesting old mines are therefore again abandoned.

Resumé of the History of the Mines.—The record of the Chalanches presents a story similar to that which is told of mines in more modern mining districts. The inaccessibility of the mines in winter, the richness of the ore, its great fusibility and the consequent systematic robbery of the silver are local commonplaces. Circumstances all worked together to make the Chalanches mines the prey of the most barefaced plunder. With the aid of a common forge-fire, even without the intervention of a crucible, and with little knowledge or skill, lumps of silver could be produced from the very rich chlorides, ruby silver and black sulphides which constituted in the main the soft earthy ores or *terres* found in the crevices of the outcrop. Aged inhabitants still talk sportively of the theft like old smugglers, and point out nooks in the woods which the remaining ruins of the little furnaces dug out by the miners, show to have been the scenes of former illicit silver-ore smelting. In these furnaces, no larger than an ordinary fire-place, dug in the earth and smeared with clay, with charcoal, or, failing that, clods of dung for fuel, and two or three little urchins to blow, like cherubs on the old maps, out trickled the white metal. Clergy and people joined cheerfully in these moonlighting operations without in any degree shocking

* That Gallic imagination by no means falls short of the Anglo-Saxon faculty when it is applied to prospectus-making, is proved by the flamboyant style in which the promoters of the *Société Savoysienne* addressed the public.

local ethics. The priest at Allemont, who lately restored the parish church, says that the old church had a room adjoining the sacristry, in which a former reverend father used to melt down the silver-ore brought to him by the faithful. The slags were concealed in an excavation under the floor, where a large accumulation of them was found when the church was restored.

During the earliest period of mining at the Chalanches, some bodies of extremely rich ore were found near the surface. It is said* that two shots produced sufficient silver to pay for the two buildings known as the pavilions at Allemont, with their various ornamentations, including the *fleurs-de-lis* which still adorn the roof. As 200 to 300 kilos of silver would at that time be worth from $10,000 to $15,000, this statement does not seem incredible.†

A "pockety" mine is notoriously apt to be loosely and extravagantly managed. The uncertainty of the work is prejudicial to the maintenance of system. The various companies that operated these mines from 1808 to 1873, did so at a loss, due largely to inexperienced engineering and loose financial management.

M. Geymard, an engineer of repute, says of this interval of sixty-five years:

"Explored, exploited, abandoned and resumed by new companies, the mine paid dividends or levied assessments according to the ability of the men sent to take charge of the works and according to the amount of the capital placed at their disposal. To sum up, the mining was restricted to the workings opened up by Schreiber; for all his successors confined themselves to a few meters of development and exploration among the old drifts."

M. Caillaux, summarizing the history of this period, adds:

"The mines of the Chalanches were never worked on a proper scale during the present century and, as has been well expressed by M. Gruner, the want of success marking the various attempts made during this lapse of time does not in any way prove the sterility of the ore-deposit, or the impossibility of its being operated to advantage."

The latter part of this statement is open to discussion.

It is not a little remarkable that although the silver is always

* For this information, and many notes, I am indebted to my father, Mr. Thomas Rickard.

† An idea of the value of the ore, outside the patches of extremely rich matetial, may be gathered from a statement made by M. Schreiber, in a report sent to the *Académie* and afterwards reproduced in the *Annales des Mines*, that the average richness of the Chalanches ore treated in the furnaces at Allemont, up to that time, was 750 grammes per 100 kilos, equivalent to about 219 Troy ounces per ton of 2000 pounds avoirdupois.

associated in the lodes with rich nickel- and cobalt-ores, often with bunches of stibnite, and more rarely and erratically with gold, the government engineers took no note of any metal other than silver. None of the valuable metals mentioned figure in the old accounts. The speiss containing nickel and cobalt was rejected with the slags, and went to fill the swamps and to form the road-beds which, in later times, were furrowed and turned over to recover their valuable contents.*

The possibility of utilizing three metals instead of one seems to have dawned upon the later engineers quite as a discovery; and this fact stimulated the repeated spasmodic attempts to rehabilitate the old mine The arsenides of nickel and cobalt were sold in England and in Germany.† More recently, a German chemist was

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* It has not been found possible to ascertain the value of the old mattes with which roads were made and marshes filled, but the following notes will assist: Herr O. F. Köttig, writing from Oberschlema, August 7, 1889, with regard to the speiss, picked up in the old smelter-dump and shipped by Pierre Manin, says that it contained 3.6 per cent. metallic cobalt, 8.9 per cent. metallic nickel, and 0.2 per cent. silver. Another lot gave 3 Co, 11.8 Ni, and 0.3 Ag. This speiss was neglected during Schreiber's time.

In August, 1863, the Viscount de Talon sent 7 barrels of ore to Vivian & Sons, Swansea. The ore in these barrels ranged in value (net) from £3 to £31 per ton, not including the silver. The average was about £16. The nickel was valued at 1*s.* 9*d.* per pound, the cobalt at 7*s.* per pound. The smelting charge was £1.10.0 per ton. The contents of the barrels were:

No.	Cwt.	Qr.	Lbs.	Nickel. Per cent.	Cobalt. Per cent.	Silver. Oz. per ton.
1, ...	16	0	9	.6	.07	11
2, ...	2	0	6	2.6	1.30	47
2, ...	3	0	6	1.1	0.60	19
3, ...	6	0	10	3.2	2.00	81
4, ...	17	3	26	3.0	1.60	201
5, ...	1	0	8	2.3	1.6	86
5, ...	3	0	7	3.8	1.9	192
6, ...	23	3	9	3.2	1.9	27
7, ...	1	1	8	4.3	3.3	39

† The Chalanches mines were always worked in conjunction with those of Grand Clos, near La Grave. The lead-ores of the latter were brought about 20 miles to Allemont, to be smelted with the products of the Chalanches. The smelting was simple. The lead-sulphides and the arsenides of nickel and cobalt were submitted to stall-roasting, and then passed through a low blast-furnace (*four à manche*), giving as products: (*a*) work-lead, containing the silver, and, of course, the gold, if present; (*b*) speiss, containing the nickel and cobalt; (*c*) slags. The work-lead went in succession to *Pattinsonage* and cupellation for the extraction of the silver. The speiss and slag went over the dump. It was only in later years that the nickel and cobalt were taken account of.

employed at Allemont in an experiment to manufacture cobalt pigments for the arts. He was not successful, and the attempt was abandoned.

In 1891 the gold-value was first recognized.* Its importance proved greater from a scientific than from a commercial point of view. The old mine-workings, aggregating 20 kilometers in length, showed that a great deal of unsuccessful exploration had been carried out. Search among these galleries, particularly near the surface, resulted in the finding of certain rich bunches of ore, which were soon exhausted. An attempt to introduce the tribute- or lease-system was made, with partial success. The necessity for concentrating the operations of the company led to more active work at Grand Clos and Les Arabes, and, at the same time, made it advisable to abandon the Chalanches.

The Ore-Deposits.

The geological formation is simple. A network of veins traverses crystalline schists of very variable character. The country forms a part of the great crystalline formation usually referred to as the Archaic schists of the Alps, though in point of fact they probably include rocks from the granite up to the Carboniferous. Lithologically, certain sections suggest the Huronian and Laurentian. These schists lie immediately upon the granite; they are extremely variable in character, so that at different places they can be described as gneissose, granitoid, talcose, micaceous, graphitic, or amphibolic.† At the base of the slope leading to the mines there are superb blocks of rock, containing crystalline epidote.

Chalanches, like the mines of the Alps generally, in France, in Savoy, or in Switzerland, is far up toward the summit of the complicated schist-region. Looking across the valley of the lovely Romanche, one can see La Gardette, the historic gold-mine of France, perched upon the cliffs overlooking Bourg d'Oisans. Across

* Small samples of the earthy ore gave occasionally as much as 1 to 1½ ounces per ton. At Les Arabes, a mine just above the village of Allemont, there occurred narrow streaks of maroon-colored earthy stuff, which assayed from 2 to as high as 7 ounces of gold per ton.

† On the government map the whole mass of the Chalanches, as also the Corneillion, on the opposite side of the river, is marked "amphibolites." Above and beyond Chalanches, there is shown a peak of "euphotide amphibolique," flanked by serpentine. There is no doubt that these rocks, on either side of the Romanche, are in places highly amphibolic, but it is an error to describe the entire mass of the mountain as such.

the winding Eau d'Olle rises the imposing mass of mountains known as "les Grandes Rousses."* One can distinguish their structure, which more immediate examination proves to be that of crystalline schist overlain patch-wise by the dolomite of the Trias, in turn succeeded by the shales and slates of the Lias.

Immediately above the Chalanches mines and in rocks of Carboniferous age there is a deposit of anthracite. To the anthracite succeeds, in fragmentary deposits, the dolomite limestone, which, though less constant than the Carboniferous sandstone and shale, almost always accompanies the anthracite basins, and in the Hautes Alpes† is associated with what appears to be Permian sandstone and the *grés bigarré*.‡ The latter fact gives rise to the suggestion that the dolomite belongs rather to the Permian than to the Trias.

The country which more immediately holds the lode-channel of the Chalanches mines, and which I may term the encasing rock, has the character of gneiss. In actual contact with the veins it is amphibolic and contains a notable amount of pyrite. This last characteristic causes it to emit sparks when struck with steel and indicates the origin of its local name, *la roche martiale.*

The oxidation of the pyrite is the reason of the red bands which seam the steep bluff crowning the upper precipitous summit of the mountain. These bands of pyritic schist dip into the hill just as the main series of veins does, a fact which led M. Graff, a distinguished engineer, to conclude that they played the part attributed to the fahlbands of Kongsberg, in Norway.

The maps of the mine exhibit a wonderful network of galleries, spreading like a cobweb over an area of about 600 by 300 meters.

It is computed that the workings aggregate in length not less than 12 miles, an extent in remarkable contrast to the relatively small quantity of ore produced.

The principal veins are six in number, four of which, the Cobalt, Prince, Simeon and Hercule, lying about 15 meters apart, dip rather flatly into the mountain, in conformity with the bedding of the enclosing rock-formation, while the two others, the Directoire, a nearly vertical vein, and the St. Louis, which cuts it diagonally, may be

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* *I.e.*, the great "roughs," ruffians, or rugged ones.

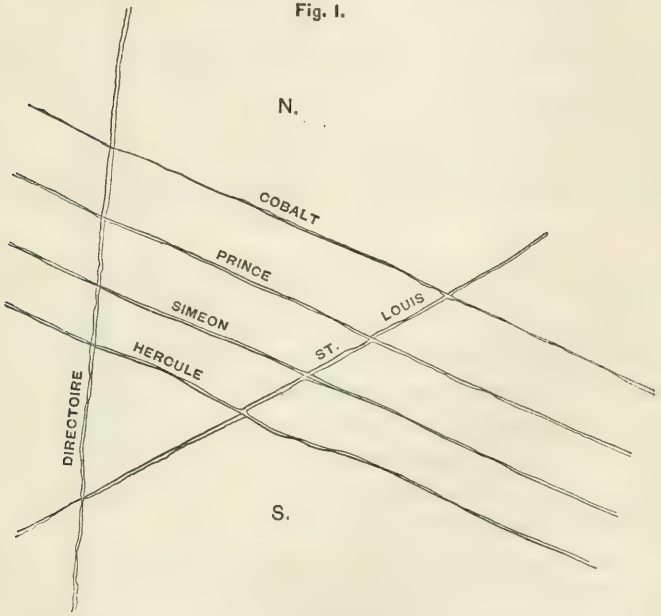
† At Argentière and Valgaudemar.

‡ The *grés bigarré* is the Bunter sandstone of the Vosges, where it rests conformably on the red Permian sandstone. (See Geikie's *Textbook of Geology*, 3d ed., p. 870.)

considered as cross-veins or counter-lodes to the main-series. The accompanying cross-section (Fig. 1) will explain these relations.

In addition to the six lodes mentioned there are several minor veins which may be considered as branches or feeders of the main series, the whole forming a complicated web of ore-bearing fissures. Schreiber is quoted as having said that "the veins have as many directions as there are points of the compass." Gruner speaks of Chalanches as "a mountain radiated and fractured in every direction, the largest fissures being filled with fragments of the encasing rock and the smaller fissures with metalliferous ores of very varied character." Geymard considered the lode-structure to be a *stockwerk;*

Fig. 1.



Cross-Section, Showing the Veins of the Chalanches.

but Graff distinguished two principal groups of veins, namely (1) those which have a north-south strike and dip either east or west, apparently parallel to the lamination of the encasing schist, and (2) those which strike east-west and dip uniformly to the north. According to the same authority, the veins north-south were the most regular and continuous. Both series, however, were stated to throw off numerous branches and thus gave rise to an apparently inextricable confusion.

Of the engineers quoted, Graff was the most trustworthy observer.

His description of the veins is good. Others, even Schreiber, seem to have entirely failed in mastering the difficulties of the deposit. In the more fissured ground near the surface the lode-formation may bear the description of a *stockwerk*, or, more properly, little separated patches of *stockwerk;* but in depth this character is not apparent. Observation at every point at which the numerous veins occur along the southeastern flanks of the Belledonne *massif* goes to prove that by far the greater number of the veins which have been mined conform to the bedding and, therefore, dip into the hill, as the Chalanches lodes do. They follow the flexures and folds of the schist, and naturally present all kinds of variations of direction when exposed in the mine workings. This is also observable at Les Arabes, Villaret and Oulles. The cross-lodes appear to be relatively unproductive, the St. Louis and Directoire being exceptions. It may be said, speaking generally, that these bedded veins in the Dauphiné* are weak, inconstant and difficult to follow.† They are frequently altogether obliterated, leaving to the miner the bedding of the country as his only guide. Hence the apparent extravagance of drifts.

Towards the center of the mine workings of the Chalanches there are three dikes of diabase, respectively 23, 3 and 30 meters in thickness. Furthermore, all those who have at various periods directed the mines take note of large barren fissures which traverse the mountain and dislocate the ore-bearing veins. They are filled with fragments of country and with clay, both sandy and micaceous. Schreiber considered them as barren lodes and termed them "*filons sauvages privés de substances métalliques.*" A Cornishman would call them "flookan."

It has been thought by several observers that the lodes were more numerous near the surface than in the interior of the mine. This is due to the fact that any single fissure, in approaching the surface, spreads itself out into a number of subordinate fractures. It has also appeared that the lodes gained in regularity as they penetrated the mountain. Caillaux therefore adds that this fact seems to indicate the probable occurrence in depth of only a small number of lodes, but that those surviving will have a regularity greater than those which have been hitherto exploited. Regularity of structure would be a poor compensation to the miner for the fact that the en-

* The more modern departments of the Isère and Hautes Alpes.

† These bedded veins in the schists remind one often of the lodes in the quartzose schists of Otago. (See *Trans.*, xxi., 411 *et seq.*)

closing rock is much harder, and the thickness of ore smaller, than in the ground nearer to daylight.

The veins vary in width from a knife-blade to 80 centimeters (31.5 inches); their usual thickness lies between 3 and 30 centimeters (0.1 to 1 foot). The following data from my note-book will indicate, in a general way, the size and nature of the veins:

July 14, 1891.

Première Hercule.—12 cm. in two parts: the upper, quartz and fine-grained galena; the lower, efflorescence of nickel. (annabergite) in earthy ore.

Troisième Hercule.—30 cm. Antimonial ore (stibnite and oxide) accompanied by cobalt-bloom (erythrite).

Cinquième Hercule.—Vein crossed by cross-course. Broken; barren.

St. Nicholas.—35 cm. Brown earthy ore, containing arsenates of nickel and cobalt, with native silver.

Galerie d' Argent.—5 to 7 cm. Wire silver, with black sulphides of silver, in an earthy gangue.

August 11, 1891.

Première Hercule.—10 cm. Chiefly calcite, a few spots of galena.

Troisième Hercule.—35 cm. Calcite, with threads of galena.

Prince.—20 cm. Red and black earthy ore (found afterwards to contain 35 ozs. Ag) stained with copper.

Cinquième Hercule.—7 to 10 cm. Calcspar, with splashes of stibnite.

St. Nicholas.—12 cm. Black earthy ore, with stones of fahlerz and kupfernickel.

Galerie d'Argent.—5 cm. Black earthy ore, full of native silver.

General Conclusions Regarding the Ore-Occurrence.—Some of the conclusions of several accomplished French engineers have been quoted. They differ according to the condition of the mine at various periods, depending upon whether the development-work was being vigorously pushed ahead, new ground opened up and rich ore extracted, or whether the work of exploration was restricted and the operations confined to the search for the pockets of silver which were found from time to time irregularly distributed amid the complex of veins. At the time when I directed the work, attention was mainly confined to the exploration of blocks of ground not previously intersected by drifts or cross-cuts. No work was done in the ends of

galleries farthest advanced into the mountain. My experience of the mine and its ore-deposits led me to the following conclusions:

The formation of crystalline schists has been subjected to fissuring at more than one epoch, an earlier one being marked by the fractures filled with ore and now forming the lodes, and a later one characterized by the formation of cross-fractures which broke across the previously formed lodes, and are themselves non-metal-bearing.

Near the surface the lodes are soft, especially where they are richest in silver. Though they agree in the main as to their mineral impregnation, the daily advance of drifts will exhibit an extraordinary variety of vein-filling. They all contain nickel, cobalt, silver, and in places also antimony and gold, as their commercially valuable elements. With these are associated the numerous mineral species for which the mines are famous, notably acicular crystals of epidote. The minerals of the Chalanches are to be found in most of the important collections of Europe.*

Examination of the old workings proves clearly that with increasing distance from the surface the country gets harder, the veinstuff loses its soft character, the veins become fewer in number, more regular, less wide and less ore-bearing. Approaching the surface, on the contrary, the schists are fractured in a multiplicity of directions, the veins become larger, their filling is generally earthy, and they throw off branches, at the intersections of which ore-bodies are found. In general, mineralization becomes more pronounced with approach to daylight; this being due, not merely to the oxidation of the sulphides, but to an actual relative increase of "orey" matter.

The outer portion of the mountain is jointed and otherwise fractured to a very remarkable degree. The seams of ore which follow such joints and fractures are often composed of earths rich in nickel and cobalt, or of wire-silver mud. An illustration of such an occurrence is afforded by the pocket of native silver found within a few feet of the surface, at the intersection of the St. Louis and Cobalt veins, in July, 1891. The accompanying sketches, Figs. 2 and 3, show the change in the lode-structure as the working progressed, the time between the two drawings being four days. In Fig. 2 the native silver, enclosed in a black mud, and accompanied by ochreous earth, extends from one vein, across the point of intersection, into the other. In Fig. 3 the silver is confined to the upper part of the

* Its contribution to the Museum of Natural History at Grenoble would itself repay a voyage across the Atlantic. All the mines of Leadville, for instance, would hardly furnish a richer collection of minerals than the Chalanches alone.

Cobalt vein. Red iron-earth formed the filling of the St. Louis. The structure of the enclosing country is merely suggested in the drawings.

The observations made from day to day led me to conclude that

Fig. 2.

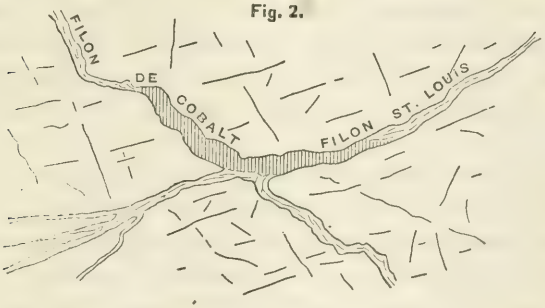
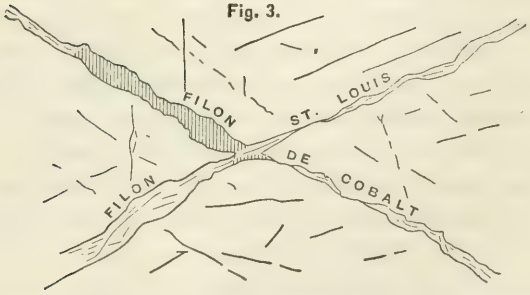
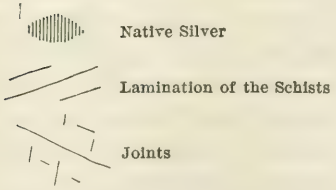


Fig. 3.





Sections Showing Pocket of Native Silver.

the richest part of the mine was that which was within the influence of oxidation, and that both chemical agencies and structural conditions favored an enrichment of ore near the surface. This statement is particularly applicable to the silver contents. It also holds true of the gold, but it is less accurate with respect to the nickel and co-

balt. The richness in silver of the oxidized ores suggests secondary precipitation. This is confirmed by the fact that the silver appears to be thrown down upon the nickel and cobalt arsenides, and often envelopes them in such a way as to impart to them the rudiments of a nodular structure. The hard, undecomposed arsenides contain only small amounts of silver. The gold, only occasionally present, is associated invariably with soft, maroon-colored, earthy, iron-bearing vein-stuff. The nickel and cobalt minerals appear to be primary ores, and are more persistent than those of silver and gold.

The dependence of the occurrence of large amounts of rich ore upon the broken and fractured character of the country would seem to me to indicate that that particular concentration of metallic minerals which renders the deposit economically important is of comparatively recent geological date; for, only near the surface (the surface of any given time, not necessarily only that of to-day) were there the conditions favoring such a concentration. A study of the vein-structure of the surrounding region shows that the ore-bearing veins are younger than the Jurassic age. This, of course, applies only to the fissures in which the ore is now found. The actual deposition of ore could not have commenced before the fracture took place, but it has probably been going on ever since. At the Chalanches, the change in the nature of the ore-deposits, both in structure and in mineral contents, is measured, not from any imaginary nearly horizontal surface of a former unknown epoch, but from the steep slope of the hillside of to-day.

The occurrence of deposits of nickel-ores in close association with basic eruptives, and more particularly magnesian rocks, has been frequently noted. The country-rock of the Chalanches lodes consists of the crystalline magnesian schistose rocks, which have been already mentioned. They are overlain by dolomitic limestone, and are intruded upon by a mass of altered gabbro or euphotide, which is, in turn, flanked by serpentine. The serpentine may have been derived by the metamorphism of amphibolic schists, euphotide, or limestone.* That the origin of nickel-deposits is traceable to the leaching of basic eruptives; that the metal and its ores occur in a finely disseminated condition in such rocks, and have been by

* That serpentine can be derived by metamorphism from magnesian silicate rocks, or from limestone, has often been pointed out—quite recently by Mr. S. F. Emmons, in "Geological Distribution of the Useful Metals," *Trans.*, xxii., 71.

them brought within the reach of circulating waters, is, at present, a strongly favored theory.*

As bearing on this part of the subject, the following additional facts are pertinent. At the Chalanches, in addition to the magnesian silicate schists, forming the encasing rock of the lodes, there are, at least, three dikes of diabase. Above Bourg d'Oisans – six miles distant—there is an amygdaloidal melaphyre (the *spilite* of the French geologists), which carries nodules of calcite accompanied by sulphide of nickel.† Between Allemont and Vizille there are, according to the government map, several outcrops of *spilite*. They are usually associated with the upper Trias, and occasionally appear to belong to the Jurassic. Certain of the ore-bearing veins of the district penetrate from the crystalline schists into the anthracite beds, and even into the Lias, the last being the youngest formation of the lode-mining portion of the region.

The *roche martiale*, or pyrites-bearing bands of schist, which immediately contain the most productive veins, illustrate that association of nickel and iron pyrites which has been often remarked by geologists.

The origin of the metals which enrich the veins of the Chalanches is a matter which, owing to the limited data bearing upon it, cannot be discussed at great length with profit. The silver and gold may be supposed to have been derived, as elsewhere, from ascending solutions which, in approaching the surface deposited their precious contents according as the structural conditions of the rock or the chemical composition of the casing of the fissures may have regulated that deposition. The nickel and cobalt will be considered, by many, to have had a more definite and immediate origin in the magnesian silicates of the diabase and schists, out of which they will be supposed to have been leached. These explanations of the origin of the four metals mentioned would, in that case, be a compromise between the contending views of the two sides in the controversy between the extremes of lateral secretion and ascension.

It will be claimed, however, that, if the nickel and cobalt were obtained from out of the encasing (the wall-rock), or the enclos-

* It is advocated, for instance, by Mr. P. Argall, in a contribution to the Colorado Scientific Society, entitled, "Nickel: the Occurrence, Geological Distribution, and Genesis of its Ore-Deposits."

† A similar occurrence is that of millerite, or sulphide of nickel, recently noted by me in certain hornblende-schists at the Gipsey Queen Mine, 3½ miles east of Salida, Colo.

ing (the remoter country) rock, then, there should be some difference in the schists penetrated by those parts of the lodes carrying a notable amount of nickel and cobalt, as compared to the schists in which the lodes are barren. It cannot be said that this is the case. The bands of pyritic schist are parallel to the lodes, and enclose both their rich and their barren portions. That the lode-channel is marked by the presence of schist rich in pyrite is true; but the fact points not to the pyrite as the source of the metal, but simply to a probable identity of source, and contemporaneity of deposition. Within the reach of oxidizing agencies, both the nickel and cobalt arsenides and the iron pyrite are found decomposed in some places, unaltered in others. The interior workings of the mine show veins carrying hard unoxidized ores encased in harder schists, the pyrite of which is unaltered. While the rock which has been most affected by oxidation contains veins richer in silver and gold than that which does not show the action of such agencies, there is no noteworthy difference in the nickel- and cobalt-contents.

In these mines, as in others in widely separated regions, I have observed that it often happens that a very narrow but very rich streak of ore may occur encased in hard undecomposed country, while, on the contrary, a large width of poor veinstuff may be enclosed by highly altered and mineralized rock. This, which is, I believe, a common observation to those who spend much time underground, is a fact forever opposed to the narrower* views of any lateral-secretion theory. In such cases it is evident that the encasing rock has been mineralized and enriched through the agency of solutions which travelled in the lode-channel; that the mineralization took place from the lodes to the country and not from the outside country toward the interior of the lode-channel.†

If we accept the current theory that the nickel and cobalt came from the leaching of magnesian silicates (and facts are numerous pointing that way), then, we must conclude that the origin of the nickel and cobalt of the Chalanches was not the immediately enclosing country, but rocks similar to it, which underlie it at a greater depth. The silver and gold, it may be suggested, were precipitated from other solutions, and at a period other than that which saw the deposition of the nickel and cobalt. The precious metals were prob-

* As distinguished from the wider interpretation given to that theory, not by Prof. Sandberger, but by Mr. Emmons and others in this country.

† This view was advocated by Mr. Pearce thirty years ago. (See *Trans.* xxii., 740.)

ably derived from a deeper-seated source; and may have been leached from the granite which underlies the schists and is penetrated by the basic eruptives. In both cases, the various metals must have come from a depth where leaching action was powerful, and from which ascending currents brought the metallic constituents, the subsequent precipitation of which produced valuable ore-deposits.

FURTHER EXPERIMENTS ON AMORPHOUS GOLD.

BY HENRY LOUIS, LONDON, ENGLAND.

(Bridgeport Meeting, October, 1894.)

THE present paper comprises two parts, which have but little connection with each other. The first is practically a continuation of the investigation on the specific gravity of gold liberated by means of acids from its alloys, on which subject I had the honor of presenting a brief note to the Institute in 1893 (*Trans.*, xxii., 117); the second describes a series of experiments on the behavior of amorphous gold under the action of heat.

By the kindness of Prof. W. C. Roberts-Austen, C.B., F.R.S., chemist to the Royal Mint, I have been permitted to conduct these experiments in the mint assay-office, all the splendid resources of which institution were freely placed at my disposal. I desire at the outset to express my obligations to Prof. Roberts-Austen and to all the staff of the mint assay-office for their kind assistance at all stages of my experiments.

I.—THE SPECIFIC GRAVITY OF GOLD IN ALLOYS.

In the note already referred to, I published my discovery that the gold left on dissolving out the silver from an alloy of gold and silver (which I shall here term "residual amorphous gold") has a higher specific gravity than ordinary molten gold. I also pointed out that I had worked on very small quantities of material and with balances and weights on the accuracy of which I could not implicitly rely, and that in so far my numerical data were inconclusive, although I had but little doubt of the correctness of the broad fact. The experiments now recorded were made under the most advantageous circumstances and with various refinements. Particularly, I found that the ordinary form of specific-gravity bottle was responsible for several small sources of error; and I accordingly designed an improved form (*Journal of the Society of Chemical Industry*, April 30, 1894, vol. xiii., p. 322), which I used for this work. The

weighings were always made in air and no correction for the weight of air displaced has been made; this would, however, be a very trifling amount when working with so dense a body as gold.

A. *Residual Amorphous Gold from Gold-Silver Alloys.*—A batch of cornets was kindly prepared for me by Mr. F. W. Baily of the Royal Mint assay-office. These were prepared in the manner invariably used for making assays of gold bullion, and were perfectly normal cornets, the mode of procedure being as follows: $\frac{1}{2}$ gramme of gold is alloyed with $1\frac{1}{4}$ grammes of silver and a little copper, by cupellation in 4 grammes of pure lead-foil. The resulting button is rolled into a fillet, coiled up and then parted in platinum cups in platinum boilers, by means of two boilings in dilute nitric acid of specific gravities 1.26 and 1.32 respectively. The gold thus obtained is 999.7 parts per mil. fine. The cornets prepared for me in this way were repeatedly washed with hot distilled water, boiled in water and allowed to cool *in vacuo*. Two batches of 28 and 26 cornets respectively were operated on, and the specific gravity of each batch was determined by means of the specific-gravity bottle and by weighing in a light glass cup suspended by means of a fine silver wire in a beaker of water. Each lot was then dried and annealed in the muffle in the ordinary way, and its specific gravity was again determined.

Finally these annealed cornets were fused under a layer of borax-glass, and, after careful washing and cleaning, the specific gravity of the molten gold was determined.

The following were the results obtained:

	I.	II.	Mean.
Specific gravity before annealing, . .	19.517	19.505	19.511
" after " . .	18.729	18.728	18.7285
" after fusion, . .	19.200	19.173	19.1865

These results, the accuracy of which I have no reason to doubt, show that the residual amorphous gold has, in spite of its very much greater bulk, a higher density than gold which has been fused, whilst the annealed cornet is markedly lower in density than the same gold. For this latter phenomenon I am unable to suggest any explanation, unless it be that the gold sponge on contracting may completely enclose some cavities, and have therefore a cellular structure.

B. *Alloys of Gold with Other Metals.—a.* Two alloys of gold and copper were prepared in approximately the proportions of 1 to 3 and 1 to 7 respectively. These were rolled into strips, cut into small pieces, annealed and parted in nitric acid. The gold in the richer alloy re-

tained fairly the shape of the original pieces; in the poorer alloy it broke up, and occupied a bulk fully double that of the original alloy.

b. Three alloys of gold with lead were prepared in the approximate proportions of 1 to 3, 1 to 4 and 1 to 6. All these alloys were gray, granular, and very brittle. They were pounded to a coarse powder and parted by means of nitric acid. In each case the gold formed a loose, spongy mass, very bulky indeed, occupying at least six or eight times the volume of the original alloy.

c. Two alloys of gold with zinc in the approximate proportions of 1 to 3 and 1 to 7 were prepared. These alloys were difficult to pour, but proved to be fairly malleable, so that they could be rolled into strips and then cut into pieces. These were hard, grayish-white, with a granular fracture. They were not annealed. Parting was commenced with dilute sulphuric acid, but it was not satisfactory, owing probably to the presence of small amounts of lead in the zinc. The acid was therefore poured off and the residual metal well washed, first with water and then with ammonic acetate, to dissolve any sulphate of lead that might have been formed. After washing again, the parting was completed with dilute nitric acid, the action of which was almost explosive in its violence. The pieces of residual gold retained in each case the shape of the original pieces of alloy, and did not break up at all.

The specific gravity of the residual amorphous gold was determined in each case with all due precautions, and the results obtained are tabulated below:

Alloying Metal.	Composition per 100.		Specific Gravity.	Fineness of Residual Gold.
	Alloy.	Gold.		
a. Copper	75.09	24.91	19.666	.....
"	87.46	12.54	19.567	.....
b Lead	75.36	24.64	19.786	999.3
"	80.02	19.98	19.122	999.0
"	85.78	14.22	19.751	999.25
c. Zinc	73.66	26.34	18.854	990.9
"	87.85	12.15	18.919	996.4

The fineness of the residual amorphous gold was determined for me by Mr. F. W. Baily, the results given being the mean, in each case, of two determinations. It will be observed that the richness or poorness of the original alloy does not seem to affect the specific gravity of the residual amorphous gold, whilst, on the other hand, this fig-

ure seems to be higher, the more completely the alloying metals cause the liberation of the residual gold in the pulverulent form. The average of the specific gravity found was higher than that of ordinary gold, except in the case of the zinc-alloy, in which the parting was troublesome and unsatisfactory, and the resulting gold impure.

II.—Effect of Heat on Amorphous Gold.

A series of experiments was made upon unannealed gold, with the object of discovering whether this metal, in passing from one state to the other, showed any points of "recalescence" similar to those obtained in heating and cooling iron. In the case of gold, there was no object in investigating the phenomena during cooling, seeing that the change in the condition of the gold is produced during heating. The arrangement of the apparatus used is shown in Fig. 1, in which A is a rectangular box, made of cast-iron, about $\frac{3}{8}$-inch

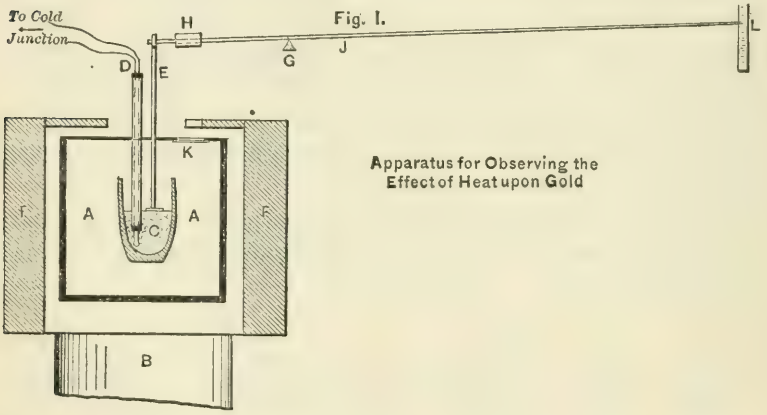


Fig. I.

Apparatus for Observing the Effect of Heat upon Gold

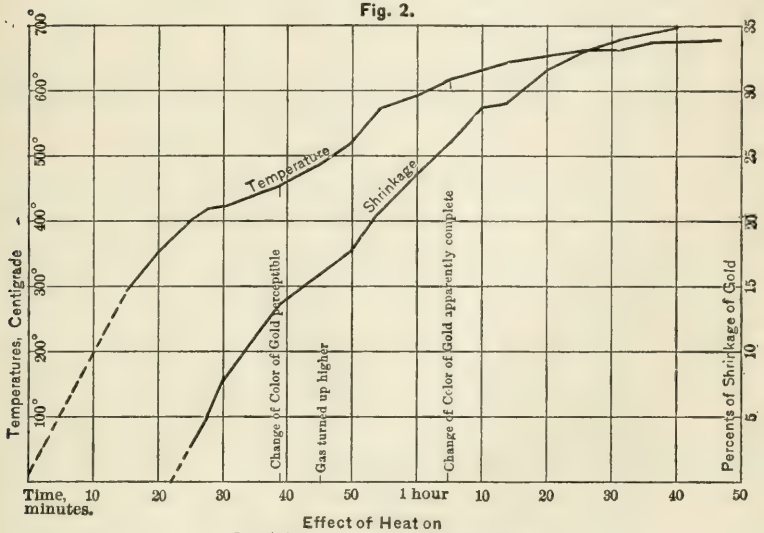
thick. It is surrounded by the furnace, F, and heated by the powerful burner, B, the gas-supply to which can be regulated to a very fair degree of uniformity. The box has an iron cover, with a mica window, K, through which the interior can be examined. In the box is suspended a crucible, C, containing the gold to be operated upon. The terminal wires, D, of a Le Chatelier pyrometer of the usual type are inserted in the mass of gold, the wires passing to a cold junction, the temperature of which was taken at intervals during the experiment, and thence to a reflecting galvanometer, the deflections of which were recorded photographically by Prof. Roberts-Austen's method.

In carrying out my experiments, I had the advantage of the help

of Mr. R. Stansfield, Prof. Roberts-Austen's assistant, who has had much experience with this pyrometer. On top of the gold in the crucible was placed a light rod, E, on the upper end of which rested the short arm of the lever, J, having its fulcrum at G. H is a small sliding counterpoise, by means of which the short end of the lever was kept steadily pressing upon the top of E. The extreme point of this lever, L, passed over a graduated scale, by means of which the contraction of the gold in the crucible could be measured.

Precipitated gold was first experimented on, the gold used being

Fig. 2.



Effect of Heat on
Precipitated Gold, March 80th, 1894.
Weight of Gold used, 13,51 grammes.
Depth of Column of Gold, 0:87 in.

some that had been prepared for use as check-pieces in the mint assay-office. It had been precipitated by means of sulphurous acid gas from a rather strong solution of auric chloride, and was in moderately fine powder, which showed, however, a strong tendency to cohere into lumps. Under the microscope it appeared to be somewhat crystalline, although no distinct structure could be made out, and it had a well-marked metallic luster.

The effect of heat on this gold is shown graphically in Fig. 2, in which the curves both of rise in temperature and of shrinkage of volume are plotted. The abscissæ show time in minutes, the ordinates showing degrees centigrade and percentages of contraction of depth of column (linear contraction) respectively. It will be seen

that there is no indication of recalescence, the temperature-curve being, on the whole, fairly regular. The first indication of shrinkage was obtained at about 400° C., although it must have commenced some 20° lower, and it appeared to continue up to about 680° C. The change in bulk after that temperature being scarcely

Fig. 3.

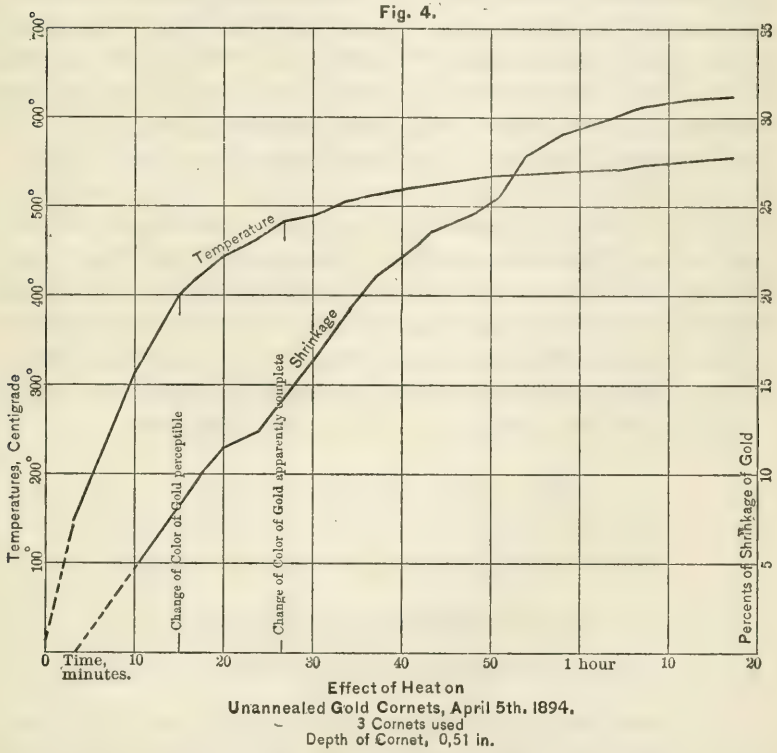
Effect of Heat on a
Mass of Gold Cornets, April 19th, 1894.
18 Cornets used = 9 grammes.
Depth of Column of Gold, 0.623 in.

noticeable, the experiment was accordingly stopped. The points at which the first indication of change in color and luster were perceived, and at which the change appeared complete to the eye, are indicated on the diagram; but it must be remembered that this change is very gradual, and that the points of its commencement and completion could not be definitely fixed.

The experiment was repeated, with the modification of placing two crucibles in the heating-chamber—one containing unannealed gold, and the other, gold already annealed, in order to see whether there was any difference in their respective rates of heating. No such difference could be noticed; and it seems certain that there is no recalescence with gold thus precipitated.

Figs. 3 and 4 exhibit the results of similar experiments upon gold

Fig. 4.



Effect of Heat on
Unannealed Gold Cornets, April 5th. 1894.
3 Cornets used
Depth of Cornet, 0,51 in.

cornets, thoroughly air-dried but not annealed. In the former case (Fig. 3), the cornets were broken into small fragments, and then transferred to the crucible; in the latter case, unbroken cornets were operated on. As will be seen, the results are somewhat similar to those obtained with the precipitated gold. Shrinkage was first distinctly noticed at 360° C. and 320° C., respectively, and appeared to be comparatively complete at 620° C. and 550° C., respectively. The latter experiment is especially interesting, because the decrease in volume which a cornet undergoes during annealing was directly measured thereby; the diminution in the depth of the cornet, that

is to say, in the width of the fillet, was measured, and it was found that this dimension, after heating, was two-thirds of that of the unannealed cornet. On the assumption that the linear shrinkage of all three dimensions is equal, the volume of the annealed would be 0.3 of that of the unannealed cornet.

Differential experiments upon unannealed and annealed cornet-gold, heated side by side for the purpose of comparison, were also made; and none of these showed the least indication of recalescence. It seems certain, therefore, that the change from dull-brown amorphous gold to the ordinary yellow lustrous variety is not accompanied by any absorption or evolution of heat; and, in so far, these experiments do not support my views on the allotropism of gold. It must, however, be admitted, that they do not oppose those views.

It is evident, that whatever the nature of the change may be, it is a very gradual one, unmarked by any abrupt transition. At the same time, it is possible that different results might be obtained by working upon the very finely divided gold precipitated by a dilute solution of ferrous sulphate from a dilute solution of auric chloride in the cold; and this experiment remains yet to be tried. Meanwhile, the above experiments show conclusively within what ranges of temperature the annealing of gold cornets takes place.

THE NEW MINING LAW OF NEW YORK.

BY R. W. RAYMOND, NEW YORK CITY.

(Bridgeport Meeting, October, 1894.)

In a former paper (*Trans.*, xvi., 770) I gave the text of the archaic mining law of the State of New York, together with some comments upon its curious provisions. In that connection I pointed out two facts: first, that the law seemed to have originated in the schemes of private parties, who were satisfied with those parts of it which concerned them particularly; and secondly, that the remaining provisions had been hitherto quite inoperative, for the reason that mines of the character to which they referred had not been discovered in the State.

Since 1888, when that paper was written, the New York Legislature has passed several acts relating to mines as follows:

1. Chapter 394, Laws of 1890. This provides for an inspection

of mines to secure the safety of workmen. As it is purely an exercise of the "police authority" of the State, and does not contemplate any regulation of mining titles or the economy of mining operations, it does not fall within the scope of the present paper, and will not be further mentioned.

2. Chapter 411, Laws of 1890. This provides for the working of mines upon the State lands, and also protects private land-owners against unauthorized entry upon their lands. As its provisions have been substantially incorporated, with additions, into a later act (Chapter 317, Laws of 1894), they will not be separately discussed here.

3. Chapter 667, Laws of 1892. This concerns only the safety of workmen, and, for the reason given above, will be omitted from the present discussion.

4. Chapters 317 and 745, Laws of 1894. The consideration of these acts will be the subject of the present paper. For convenience of reference, I will designate as A, the law discussed by me in 1888; as B, Chapter 317 of 1894; and as C, Chapter 745 of 1894. For my comments upon A, I must refer the reader to my paper, already cited; but it seems necessary to reproduce here, for ready comparison, the text of that statute.

A. The Law in Force in 1888.

This will be found in Title XI., Chapter IX., Part One, of the Revised Statutes as follows:

Section 1. The following mines are, and shall be, the property of the people of this State in their right of sovereignty:

(1.) All mines of gold and silver discovered, or hereafter to be discovered, within this State.

(2.) All mines of other metals discovered, or hereafter to be discovered, upon any lands owned by persons not being citizens of any of the United States.

(3.) All mines of other metals discovered, or hereafter to be discovered, upon lands owned by a citizen of any of the United States, the ore of which, upon an average, shall contain less than two equal third parts in value of copper, tin, iron, and lead, or any of those metals.

Sec. 2. All mines and all minerals and fossils discovered, or hereafter to be discovered, upon any lands belonging to the people of this State, are, and shall be, the property of the people, subject to the provisions hereinafter made to encourage the discovery thereof.

Sec. 3. All mines of whatever description, other than mines of gold and silver, discovered, or hereafter to be discovered, upon any lands owned by a citizen of any of the United States, the ore of which, upon an average, shall contain two equal third parts or more, in value, of copper, tin, iron, and lead, or any of those metals, shall belong to the owner of such land.

SEC. 4. Every person who shall make a discovery of any mine of gold or silver within this State, and the executors, administrators, or assigns of such persons, shall be exempted from paying to the people of this State any part of the ore, produce or profit of such mine for the term of twenty-one years, to be computed from the time of giving notice of such discovery, in the manner hereinafter directed.

SEC. 5. No person discovering a mine of gold or silver within this State shall work the same until he gives notice thereof, by information in writing to the Secretary of this State, describing particularly therein the nature and situation of the mine. Such notice shall be registered in a book to be kept by the Secretary for that purpose.

SEC. 6. After the expiration of the term above specified, the discoverer of the mine, or his representatives, shall be preferred in any contract for the working of such mine, made with the legislature or under its authority.

SEC. 7. Nothing contained in this title shall affect any grants heretofore made by the legislature to persons having discovered mines, nor be construed to give any person a right to enter on or break up the lands of any other person, or of the people of this State, or to work any mine in such lands, unless the consent, in writing, of the owner thereof, or of the commissioners of the land office, when the lands belong to the people of this State, shall be previously obtained.

SEC. 8. In all cases in which a person or persons shall have discovered a mine or mines, and become entitled to work the same, pursuant to title eleven, chapter nine, part first of the Revised Statutes, and such person or persons shall form a corporation pursuant to chapter forty of the laws of eighteen hundred and forty-eight, and the several acts subsequent thereto and amendatory thereof, if the consent in writing to enter upon and break up the lands of any person in or upon whose lands said mine or mines are found, shall be refused or cannot be obtained by agreement, or by reason of the infancy or the absence of such person from the State, or other legal disability of the owner of such lands, the said corporation so formed may enter upon and break up the lands of such person for the purpose of working such mine or mines in the manner hereinafter provided, and the right and easement so to do shall be deemed granted for public use, and for the public purpose of obtaining minerals reserved to the State, and the said right and easement are hereby granted to the corporation so formed, on their filing with the commissioners of the land office a full description of the location of such land, and obtaining a grant therefor from said commissioners, who are hereby authorized to make such grant and file the terms thereof.

SEC. 9. The said company entitled to work such mines may file a petition in the Supreme Court of the State setting forth the facts upon which they claim such right, and the reasons which prevent their entering upon the land necessary for their mining operations; and upon such petition the court may appoint three disinterested persons as commissioners to examine into the matter, ascertain and affix the damages aforesaid, and report to the court. Notice of the filing of such petition shall be published in one of the papers printed in the county, or in each of the counties, where the mine or mines are situated, and in the State paper, and a copy of such notice shall be served personally upon the owners of the land; or, if they are infants, upon their guardians; or, if lunatics, or under any other legal disability, on the committee having charge of them and their property. And the publication of such notice in the State paper shall be deemed a sufficient notice to such owners as are residents in other States or in other countries, or are temporarily absent from the State, provided that when the actual residence of such absentee is known or can be ascertained, a copy of such notice and petition shall be sent them

by mail. All the parties interested shall be entitled to a hearing before such commissioners, at such time or times as said commissioners shall appoint. The report of the commissioners shall state:

(1.) The existence of the mine or mines proposed to be worked.

(2.) The names of the parties owning the land in which the mine or mines are situated, and the owners of the adjacent lands, so far as they are affected by the application, and the nature and value of their interest in the same, individually. A map of such lands, from actual survey by metes and bounds, shall accompany the report.

(3.) An estimate of the damages to such owners from the contemplated use and occupation of their lands.

(4.) Such other information as the court may direct.

SEC. 10. The report of the commissioners shall be made within a reasonable time, to be fixed by the court. An order shall be made, in the discretion of the court, either denying the petition or granting it, determining the quantity of land necessary for working the mine or mines, the damage to the property by taking possession thereof, and the annual rent or the compensation to be paid to the owner, lessee, or occupant thereof, so long as the use and occupation shall continue. And thereupon the company, in whose favor the order shall be made, upon payment of the damages and upon entering into an agreement, to be approved by the court, to pay the annual rent or the compensation and damages thus determined, shall have the right to enter upon and occupy and use the land set apart by such order, so long as they or their assignees shall work the said mine or mines, and shall pay the said annual rent or compensation.

SEC. 11. If the parties owning the land are infants, or otherwise incompetent to act, the court shall appoint guardians to take care of their interests, and shall direct how any damages assessed, or compensation or rents to become due, shall be paid and invested to their benefit.

B. CHAPTER 317, LAWS OF 1894.

This became a law April 18, 1894, but, according to its final section, would not take effect until October 1, 1894. It is entitled "An Act in Relation to the Public Lands, Constituting Chapter XI. of the General Laws," and was, therefore, doubtless prepared by the State Commissioners, who are engaged in the codification into the General Laws of the various Acts passed from time to time, but who do not always, as it seems to me, either exactly retain the sense of previous statutes or refrain from original additions. No doubt their work, when submitted to the Legislature and its committees, is less closely scrutinized than are the bills introduced by individuals. It is to be presumed that they would not smuggle into the General Laws provisions of disguised special legislation. Nor is it necessary to infer from this chapter that such motives underlie it, although, as I shall point out, it contains some objectionable features, both old and new; its chief fault, however, being that it codifies past, and to some extent practically obsolete, legislation, which was neither sufficient nor suitable to constitute a mining code. I refer, not to the

whole chapter, but to those sections of it which concern mining. These I now quote, with comments upon each section.

CHAPTER XI. OF THE GENERAL LAWS.—THE PUBLIC LANDS LAW.

ARTICLE XI.—MINES.

SECTION 80. State mines.
SECTION 81. Working of mines.
SECTION 82. Private property in mines.
SECTION 83. Notice of discovery; bounty to discoverer.
SECTION 84. Permission for entry upon lands to work mines.
SECTION 85. Entry by corporations to work mines.

SECTION 80. *State Mines.*—The following mines are the property of the people of this State in their right of sovereignty:

1. All mines of gold and silver discovered, or hereafter to be discovered, within this State.

2. All mines of other metals discovered, or hereafter to discovered, upon any lands owned by persons not being citizens of the United States.

3. All mines of other metals discovered, or hereafter to be discovered, upon lands owned by a citizen of the United States, the ore of which, on an average, shall contain less than two equal third parts in value of copper, tin, iron and lead, or any of those metals.

4. All mines and all minerals and fossils discovered, or hereafter to be discovered, upon any lands belonging to the people of this State.

This re-enacts, almost *verbatim*, Sections 1 and 2 of Statute A. There is one difference, doubtless due to ignorance and oversight. The inclusion, among the mines which are declared to be "the property of the people of this State in their right of sovereignty," of "all mines and all minerals and fossils discovered, or hereafter to be discovered, upon any lands belonging to the people of this State," is the result of a clumsy condensation of Sections 1 and 2 of Statute A into a single section. The framers of that law, which was based on the earlier statute of 1789, knew enough about the "right of sovereignty" not to claim under it the miscellaneous mineral products to which no English sovereign had ever even asserted a claim. It was left for American legislators, on the threshold of the twentieth century, not only to re-assert a monarchical theory condemned long ago by progressive civilization, but to extend it even beyond the bounds hypothetically set by King John, who, in 1201, declared that all the tin-mines in his kingdom belonged to him, but thereupon immediately gave them away to whoever desired to dig for tin.*

* See my "Historical Sketch of Mining Law," in *Min. Resources of the U. S.*, for 1883 and 1884, p. 996.—*U. S. Geol. Survey*, 1885.

Nor was it necessary to base the ownership of the people of the State in the contents of their own lands upon any such absurd extension of the "right of sovereignty." They own the minerals, according to common law, simply because they own the lands. This ridiculous assertion of another source of title has, therefore, no effect whatever; and we may pass it by without further comment.

SECTION 81. *Working of Mines.*—Any citizen of this State discovering a valuable mine or mineral upon lands belonging to the State and filing the notice of discovery required by this article may work such mine, and he and his heirs or assigns shall have the sole benefit of all products therefrom on the payment into the State treasury of a royalty of 2 per centum of the market value of all such products. Such valuation shall be made when such products shall first be in a marketable form. A statement of the amounts sold or removed from the premises covered by such notice of claim, and of the trees cut or destroyed upon such lands, shall be made semi-annually, under oath, to the Secretary of State, and payments of such royalty shall be made semi-annually to the State Treasurer, under oath as to the amount thereof, on the basis of such semi-annual statement to the Secretary of State. Any wilful falsehood in the contents of such statement to the Secretary of State or State Treasurer in regard to such royalty shall work a forfeiture to the State of the value of the whole amount mined during the period covered by such statements.

This is practically copied from Chapter 411 of 1890, which extends the provisions of Sections 4, 5, 6 and 7 of Statute A (which mentioned gold- and silver-mines only), so as to make them cover any "valuable mine or mineral" discovered upon State lands, and also introduced certain new references to the cutting of timber, due, no doubt, to the public interest in the forest-preservation. I think that the Act of 1890 was probably procured by parties desiring to work mines of other character than gold or silver and situated on the forest-reserve of the State. The two per cent. royalty need not be discussed here, as it has since been abolished by Statute C.

SECTION 82. *Private Property in Mines.*—All mines of whatever description, other than mines of gold and silver, discovered, or hereafter to be discovered, upon any lands owned by a citizen of any of the United States, the ore of which, on an average, contains two equal third parts or more in value of copper, tin, iron and lead, or any of those metals, shall belong to the owner of such land.

This is Section 3 of Statute A, and, like that section, it reserves to the State mines of manganese, zinc, quicksilver, nickel, cobalt, platinum, iridium, etc. I have discussed its vagueness and unwisdom in my former paper.

SECTION 83. *Notice of Discovery; Bounty to Discoverer.*—No person discovering a mine of gold or silver within this State shall work the same until he gives written notice thereof to the Secretary of State, which shall be registered in a book to be

kept by such Secretary, describing particularly the nature and situation of the mine. Such person, and his executors, administrators and assigns, shall be exempted from paying to the people of the State any part of the ore-produce or profit of such mine for the term of twenty-one years, to be computed from the time of giving notice of such discovery, and after the expiration of such term the discoverer, or his representatives, shall be preferred in any contract for the working of such mine made with the Legislature, or under its authority.

Substantially the same as Sections 4, 5 and 6 of Statute A.

SECTION 84. *Permission for Entry upon Lands to Work Mines.*—Nothing contained in this article shall affect any grant heretofore made by the Legislature to persons having discovered mines, nor be construed to give any person a right to enter upon or break up the lands of any other person, or of the State, or to work any mine in such lands, unless the written consent of the owner thereof, or of the Commissioners of the Land Office, when the lands belong to the State, shall be previously obtained. Permission to erect buildings for working mines upon State lands within the forest preserve may be given by the Forest Commission, and elsewhere by the Commissioners of the Land Office, when such lands are entirely denuded of timber or when such commission or commissioners are satisfied that the erection or occupation of such building will not be detrimental to the interests of the State. Nothing in this article shall authorize any person working a mine upon State lands to cut or destroy any timber whatever, except such trees as it may be actually necessary to remove in order to uncover or make a road to such mine. For each tree measuring four inches or more in diameter at a height of one foot from the ground, which shall be so cut, the party operating the mine shall pay into the State treasury the sum of one dollar.

The first part of this section is identical with Section 7 of Statute A. The second part, referring to buildings and timber, is taken from the Act of 1890, and evidently contemplates mining in the forest reserve.

SECTION 85. *Entry by Corporations to Work Mines.*—Corporations formed for the purpose of working and having lawful authority to work mines found within this State may acquire the right and easement to enter upon and break up lands necessary for the operation of such mines, and if the written consent of the person in or upon whose lands such mine or mines are found shall be refused or cannot be obtained by agreement, or by reason of the infancy or absence of such person from the State, or other legal disability of the owners of such lands, every such corporation may acquire such right and easement by condemnation, which right and easement, when so acquired, shall be deemed to have been so granted for a public use and for the public purpose of obtaining minerals reserved to the State. Before instituting any proceeding for such condemnation, the corporation shall file with the commissioners of the land office a full description of the location of such lands, and obtain a grant of the right to acquire such right and easement from such commissioners who are authorized to make the same and fix the terms thereof.

This section seems to repeat the provisions of Section 8 of Statute A, but Sections 9, 10, and 11 of that law, which prescribe in detail

the proceedings required for the acquirement of right of entry by condemnation, are not re-enacted, and the changes of the phraseology of the former law which appear in the new one are, perhaps, significant. In one particular the alteration is an improvement. Section 8 of Statute A says that if a corporation, such as described, cannot, for the reasons enumerated, obtain right of entry, etc., by private agreement, it may enter upon and break up private lands "for the purpose of working such mine or mines in the manner hereinafter provided, and the right and easement so to do shall be deemed granted for public use, and for the public purpose of obtaining minerals reserved to the State, *and the said right and easement are hereby granted to the corporation so formed, on their filing with the Commissioners of the Land Office a full description of the location of such land, and obtaining a grant therefor from said Commissioners, who are hereby authorized to make such grant and file the terms thereof.*"

The words which I have italicized in this quotation apparently give to the Commissioners of the Land Office the power to grant, without the consent of the owner, and without judicial proceeding, the right of entry on private land. The only qualification appears to be in the words "in the manner hereinafter provided," at the beginning of the passage quoted; and these words may be held to apply to the working of the mines rather than to the right of entry. Moreover, if they really refer to the manner of obtaining that right, the "hereinafter" may as well indicate the proceedings before the Land Office as the judicial proceedings prescribed in Sections 9, 10, and 11. Indeed, considering the phrase with which Section 9 begins ("The said company may"—not must—"file a petition in the Supreme Court," etc.), and the positive language of Section 10 ("an order shall be made, in the discretion of the court . . . . And thereupon the company in whose favor the order shall be made, ... shall have the right to enter upon and use the land set apart by such order, so long as they or their assignees shall work the said mine or mines, and shall pay the said annual rent or compensation"), it might be plausibly argued that Statute A designated two ways of obtaining the right of entry for mining upon private land; namely, a direct grant from the Land Office of the State, or, at the option of the applicant, judicial proceedings for condemnation.

Section 85 of Statute B removes this ambiguity by requiring judicial proceedings in all cases, and prescribing as a necessary preliminary to such proceedings the filing of a full description with the

Commissioners of the Land Office, and the obtaining from them of a grant of "the right to acquire," on terms fixed by them, the easement referred to—*not a grant of the easement itself.* This is an important improvement, and if the Commissioners of the Land Office would intelligently exercise the right hereby conferred upon them, by preparing a thorough and just code of the conditions upon which their approval could be obtained in such cases, we might secure under this provision a real and wholesome mining law.

But this remark is subject to modification, in view of the later act of the legislature, now to be considered.

C. Chapter 745, Laws of 1894.

This bill was introduced in the Senate, January 9, 1894, by Mr. Harvey J. Donaldson, of Ballston Spa, representing the 18th Senatorial District, which comprises the counties of Saratoga, Schenectady, Montgomery, Fulton, and Hamilton. Hamilton county, in particular, includes a very large area of the State lands in the Adirondack region, and its senator may be presumed to know something about the interests of the State in that region, and the private enterprises likely to affect them. Moreover, Mr. Donaldson was a member of the Senate Committee on General Laws, to which committee the bill he introduced was immediately referred. The same committee must have considered also Statute B, which originated in the Assembly, and, having passed both houses, became a law April 18th, to take effect October 1st. It is a circumstance requiring explanation that Mr. Donaldson's bill, superseding many of the provisions of Statute A, was passed April 26, 1894, *to take effect immediately.* In other words, the work of the State Commissioners was abolished one week after it was enacted, and before it had taken legal effect.*

* The legislative history of this bill is as follows:

January 9, 1894. Mr. Donaldson introduced a bill entitled "An Act to amend the Laws of 1840, providing for the locating, owning and working of mines of gold, silver and other precious metals in the State of New York, and defining the duties of the mine discoverers and locator either on lands now owned by the State or the lands previously sold by the State to private individuals or corporations, and to more particularly define the rights of both the mine owner and the freeholder of any such lands as have heretofore been sold by the State, and to define the duties of the county clerks in the State of New York as to the recording of location and transfers of ownership of mines in the county records of the county where such mines are located," which was read the first time, and, by unanimous consent, was also read the second time, and referred to the Committee on General Laws, and ordered printed.

Statute C is entitled, "An Act in Relation to Mines," and runs as follows:

SECTION 1. A person who has discovered or shall discover a mine which is the property of the State, or the executors, administrators or assigns of such person, by giving notice of such discovery in writing to the Secretary of State, describing particularly the nature and situation of the mine, he and his executors, administrators and assigns shall be entitled to work such mine and entitled to all the ore, product and profit of the same, including all metals, minerals (except garnets) and fossils in such mine, for the term of twenty-one years from the time of giving such notice; and shall be exempt from paying to the State any part of such ore, product or profit of such mine during said term, and shall be preferred in any contract for the working of such mine made with the legislature or under its authority after the expiration of such term. Such notice shall be registered in a book to be kept by said Secretary for that purpose. A person filing such notice shall be known as a locator, and his interest in the lands described in such notice as a location.

This section gives to the discoverer of "a mine which is the property of the State," his executors, administrators or assigns, the right to work the mine for twenty-one years without any payment to the State, and a certain preference thereafter, upon the simple condition

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The Committee on General Laws was composed as follows:

Harvey J. Donaldson (18th District), Ballston Spa; Charles W. Stapleton (24th District), Morrisville; Cuthbert W. Pound (29th District), Lockport; Frederick D. Kilburn (21st District), Malone; William H. Reynolds (3d District), 273 Hancock Street, Brooklyn; Thomas C. O'Sullivan (12th District), 253 Broadway, New York City; Joseph C. Wolff (11th District), 80 Nassau Street, New York City; Charles L. Guy (13th District), 2 Wall Street, New York City.

March 1, 1894. Mr. Stapleton, from the Committee on General Laws, to which was referred the bill introduced by Mr. Donaldson, reported in favor of the passage of the same, with amendments, and the title amended so as to read, "An Act in Relation to Mines," which report was agreed to, and said bill committed to the Committee of the Whole.

March 6, 1894. The bill was considered in the Committee of the Whole.

March 28, 1894. The bill was read the third time, and passed by the following vote: In the affirmative, Messrs. Childs, Collins, Coggeshall, Kilburn, Lamy, Lexow, McMahon, O'Conner, O'Donnel, Owens, Parker, Parsons, Pound, Rice, Robertson, Saxton, Smelzer, Stapleton, Wolfert. In the negative, Messrs. Ahearn, Bradley, Coffey, Reynolds.

April 26, 1894. The Assembly returned the bill, with amendments, consisting in the insertion, in two places, of the words "except garnets." On motion of Mr. Donaldson, the Senate concurred in these amendments, and the bill was passed, the following senators voting in its favor: Messrs. Ahearn, Bradley, Cantor, Coggeshall, Collins, Smelzer, Donaldson, Higgins, Kilburn, Lamy, Lexow, Stapleton, Mullin, O'Conner, O'Donnel, O'Sullivan, Owens, Sullivan, Parker, Parsons, Persons, Pound, Saxton, Wolff, Wolfert. The Governor signed the bill May 22d.

In my comments on Statute C, I have assumed that it effectively repeals all parts of Statute B which would conflict with it, if Statute B had been in effect at the time of its passage. This I understand to be, in fact, its legal force, but the following

of filing with the Secretary of State a notice of such discovery, describing particularly "the nature and situation of the mine;" and with fine sarcasm, this claimant, who is not required to prove his discovery, or to define any boundaries for his claim, is to be known as a "locator," and his vague interest as a "location!"

It will be observed that this is an extension of the provision of Section 81 of Statute B, which covered only mines upon land belonging to the State, whereas this includes all mines "belonging to the State," though they be on private lands. Moreover, it abolishes the requirement of 2 per cent. royalty imposed by that section.

Further remarks on the extraordinary grant here enacted will be better understood after the citation of the rest of the act. I will only call attention here to the peculiar parenthetical reservation of *garnets* from the grant to the discoverer, which is repeated in the following section. So far as I can see, the legal effect of this reservation is to prevent all extraction of garnets from the rocks of the State of New York. I have not been able, so far, to discover the reason of this curious exception (which originated in the Assembly), and can conceive only two alternatives. Either some rural legislator, who had found garnets in the rocks on his farm, may have threatened to oppose the bill unless he was protected against garnet-miners, or else there is another neat little act in preparation, which shall favor a garnet-mining scheme. The special object of Statute C is, of course,

curious and subtle question, suggested by the peculiar time-relations of Statutes B and C, is worthy of notice.

Statute B was passed April 18th, to take effect October 1st. Statute C was signed May 22d, to take effect immediately. Statute B requires sundry proceedings and conditions which Statute C does not require. Among these is the payment of 2 per cent. royalty to the State. Statute C does not explicitly repeal Statute B, and the question is whether, under these circumstances, Statute C may not be considered as applicable only (in the particulars in which it differs from B, for instance, in the matter of royalty to the State) to the period between the time of its passage and the time when B was to take effect. I understand the rule to be that any provision of previous legislation inconsistent with a later act is *ipso facto* repealed by it. But it is conceivable that the provisions of Statute B may be held not to be inconsistent with those of Statute C; or, in other words, that the free gift of mines under C may be held to have terminated October 1, 1891, when B took effect. In that case, mines discovered and recorded before October 1st would be free of royalty, and mines subsequently discovered would be liable to royalty. If Statute C were, in form, legislation for the benefit of private parties, this question might be more serious than I now think it to be, for private acts are strictly construed against their beneficiaries. But it has been drawn in the form of general legislation, and I presume it would be held to have repealed absolutely all previous provisions imposing royalty, and all safeguards, such as I have pointed out, which it does not specifically require.

not garnets, but something the mention of which is carefully avoided —very probably, as we shall see, platinum.

SECTION 2. Such locator, his executors, administrators or assigns may enter upon or break up the land of any person where said mine so discovered is found for the purpose of working the same, and may excavate, blast and erect machinery, buildings or other structures and to do all such work as may be necessary for the production of ore and extraction of minerals (except garnets), metals or fossils, therefrom without hindrance from the owner, lessee or occupant of such lands; and the right and easement so to do shall be deemed granted for public use, and for the public purpose of obtaining minerals reserved to the State and for encouraging the discovery and production of such minerals. But before any such locator, executors, administrators or assigns shall break up the surface of any land he shall deposit with the county treasurer of the county in which said land or any part thereof is situated, such sum of money or such securities as shall be fixed and approved by the county judge of the county, or a Justice of the Supreme Court of the judicial district within which such land or any part thereof is situated as security for the payment of any damages that may be awarded the land-owner in any proceeding brought as hereinafter described; or execute a written undertaken in such penal sum as such judge or justice prescribes, with sufficient sureties, conditioned to pay all such damages so awarded. The order fixing and approving the amount of money or the securities or the undertaking prescribed by this section shall be made on application of such locator, executors, administrators or assigns, and on such notice to such land-owner, as such judge or justice directs. All damages sustained by the land-owners from such entries, excavations, blasting, or erection of machinery, buildings or other structures shall be paid to the land-owner by such locator, executors, administrators or assigns; but should he or they and the land-owner fail to agree in writing as to the amount of such damages so sustained, the damages, if any, shall be ascertained and awarded in a proceeding instituted for that purpose in accordance with the provisions of the condemnation law. But in assessing damages the Commissioners shall take into account all moneys previously paid by the discoverer to the land-owner for the privilege of prospecting, and the final order of the court shall be final and conclusive. Such proceedings must be instituted within two years from the time of filing the certified copy of the notice of discovery with the county clerk, as provided in the next section. If such proceeding is not so taken, or should such locator, executors, administrators or assigns fail to pay the amount of damages awarded by the final order of the court within thirty days after the service of a copy of such order, such location shall be deemed abandoned, and the land-owner may maintain an action to eject such locator, executors, administrators or assigns from said lands, or to enjoin him or them from further work on the same or for both such ejection and injunction, and for damages.

The audacity of this section almost surpasses belief. It will be seen upon careful study, that the said "locator" or his legal representatives may enter upon private land for the purpose of working any mine "so discovered" (*i.e.*, notice of the discovery of which has been given to the Secretary of State), without any preliminary legal notice or proceeding whatever. The owner of the land must submit to have his property overrun by alleged "locators," so long as they

do not "break up the surface." When they want to do that, they must get the right to do it; but this is to be acquired by simply depositing with the county treasurer such sum or such security as the county judge or a Justice of the Supreme Court may deem proper to assure the payment of damages, if any should be subsequently awarded; or, the locator may simply execute a bond acceptable to the court, for the payment of such damages. This seems to sweep away all the safeguards attending ordinary proceedings in condemnation of private property for public uses. There is no authority anywhere to refuse the grant. The grant is already positively given by the law, as soon as the notice of discovery has been filed. The Commissioners of the Land Office are left out altogether. The court must act on the application of the locator, not to decide whether he has really made a discovery, and whether the "mine" discovered belongs to the State at all, but solely to fix the amount of the bond or deposit preliminary to the commencement of actual "breaking-up of the surface." I do not see that the Secretary of State has any right to refuse to receive and record a notice of discovery until satisfactory proof of its existence and nature has been given. The land-owner has no opportunity to dispute the alleged fact. The notice itself is not made public, nor is the location required to be marked on the ground. The only way in which a land-owner can ascertain whether he is threatened with mining operations is to visit daily the office of the Secretary of State at Albany, and look over his book. And if, some day, a "notice of discovery" should be found in that book, the land-owner has no remedy under this statute. The right of entry upon his property is already *ipso facto* given away; and the right to break up the surface is irrevocably granted also, subject only to the opinion of a judge as to the proper security for the damages which may be subsequently decreed, in a proceeding begun within two years. Is it possible that this astounding oppression can be permitted, even to the Legislature of the State, under the Constitution? Is this the taking of private property for public uses, under the right of eminent domain, by "due process of law?"*

* It is not my purpose to go into the constitutional argument in detail on this occasion. But I may be permitted to make some brief suggestions:

1. Statute B (Section 85) declares that a corporation may obtain by condemnation the right to enter and break up private lands for the purpose of "working mines, which right and easement, when so acquired, shall be deemed to have been so granted for a public use and for the public purpose of obtaining minerals reserved by the State." But the mere declaration by the legislature that a certain enterprise "shall be deemed to be" for a public use, does not preclude the inquiry, whether

Again, it is to be observed that, whereas Statute B confined certain privileges to incorporated companies, this one extends its still wider privileges to individuals. The change is, so far, a bad one, because the State has powers and means of inspection and control over corporations which it cannot easily employ with regard to individuals. On the other hand, the individuals here specified must be actual *discoverers* or their legal representatives. I do not see how any mine previously known can be re-discovered and acquired under this law. But perhaps Section 85 of Statute B still survives, in which case a corporation may attack any mine, old or new.

SECTION 3. The county clerk of the county in which such location is situated, shall record and index as a conveyance of real estate from the people of the State, a copy of the notice of discovery of any mine heretofore or hereafter located, to be furnished by such locator, executors, administrators or assigns, and certified by the Secretary of State; and such locator, executors, administrators or assigns, shall have priority of right to and in such location according to the priority of record thereof with such county clerk. Conveyances or mortgages of such location or mine or of any interest therein shall thereafter be recorded in such county the same and with the same effect as other conveyances or mortgages of real estate.

SECTION 4. Nothing in this act contained shall affect the rights of any person in and to a mine or location heretofore discovered upon lands belonging to the State, if a certified copy of the notice of discovery thereof is filed in the office of the clerk

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the purpose is in fact public, in such a sense as to justify the exercise of the right of eminent domain. And whatever might be the answer to this question under the conditions of Statute B, which makes the State profit by the mines to the extent of 2 per cent. royalty, there is little doubt that under Statute C, which grants the mines without royalty, the enterprise is not "public." The general indirect benefit to the State from having mines worked, is not enough to make the purpose a "public" one. This principle has been settled in several leading cases, in New York and other States.

2. Statute C, proceeding on the theory that the right of eminent domain will cover the case, does away with all the safeguards by which the exercise of that right has been hitherto surrounded, and practically takes private property without any due process of law. This is sufficiently shown in the present paper.

The general rule is, that private property can be taken for public purposes only after regular judicial proceedings in condemnation, and usually only after the owner has been paid. This rule has been modified somewhat in the New York elevated railroad cases, in which the roads have been erected first, and the damages settled afterwards. But in those cases the real estate of private owners was not taken before condemnation; and, as I understand it, the courts have recognized a distinction between such a trespass upon real estate and the indirect injury wrought by deprivation of light and air, etc., through a structure erected outside of private property. In other words, no elevated railroad has ever been permitted to cross private property or take possession of it for switches, stations, etc., without first paying for it. These cases, therefore, do not cover the intrusion upon private property before and without judicial condemnation, authorized by Statute C.

of the county where such mine or location is situated, within one year after the passage of this act.

SECTION 5. This act shall take effect immediately.

Remarks on these sections will be found among the general observations below:

From the legislative history of Statute C, it appears that the original bill introduced by Mr. Donaldson referred to the precious metals only, as its title clearly indicated. The enlargement of its scope to cover all mines "belonging to the State," must have been effected in the committee. In this connection, I quote from the stenographer's report the remarks made in the Senate by Mr. Stapleton, which constituted *the whole of the debate on the bill:*

"This bill has been before the Committee on General Laws for nearly two months. The only change this bill makes in the existing law is entirely for the benefit of the land-owner. It simply provides that locators and prospectors may enter upon lands and stake out claims, upon doing which they must file with the Secretary of State a certificate of location. Also that an order may be procured from the county court or Supreme Court compelling them to file an undertaking to pay any damages that may arise or accrue to the land-owner or deposit a sum of money for that purpose.

"This bill also differs in one other respect from the existing law. As the law now stands there is nothing to prevent one man from locating on top of another's claim. This proposed law prevents that, and gives the right to the first locator. These are the only changes from the original law.

"The committee were unanimously of the opinion that the law ought to pass for the benefit of people who desire to locate mines in this State.

"I withdraw my request to be excused from voting, and vote aye."

These statements totally misrepresent the nature of the bill. Yet they passed without contradiction, and apparently the Senate accepted them as justifying favorable action. As an examination of the several acts shows:

1. The bill makes no change "for the benefit of the land-owner."

2. It does not provide "that locators may enter upon lands and stake out claims" before filing a certificate of location.

3. It does not provide that "an order may be procured" compelling locators to give security for damages.

4. The previously existing law did not leave it possible for one man to locate "on top of another's claim."

5. The changes specified by Mr. Stapleton are not the only ones made in the existing law. He suppresses mention of the radical changes pointed out in this paper.

In short, every assertion contained in this brief speech is untrue;

and the statement at the beginning, that the only change made by the bill "is entirely for the benefit of the land owner," is curiously contradicted by the final statement that "the committee were unanimously of the opinion that the law ought to pass *for the benefit of people who desire to locate mines in this State.*"

For greater clearness and comprehensiveness, I will arrange under several principal headings some further observations upon the system of mining law thus suddenly thrust upon this State.

1. *Minerals Upon State Lands.*—Although much that will be said under other headings below is equally applicable to the lands of the State, it is convenient to consider here more particularly the effect of the above laws upon the public domain of New York.

As I have already observed, the absurd claim to ownership of all the minerals upon State lands by "right of sovereignty" was not necessary. Statute A asserts this ownership by right of property, which is quite sufficient. There is nobody to dispute the title, whether it was derived originally from the North American Indians, or the King of Great Britain, or the King of France, or the Pope of Rome—all of whom, at various times, have laid claim to it, and have surrendered whatever claim they asserted. The land of the State, together with all its contents and appurtenances, *usque ad inferos et usque ad coelum*, unquestionably belongs to the people of the State until, through their authorized representatives, it is, wisely or foolishly, in whole or in part, granted away. That the mineral right may be segregated and separately disposed of by the State, as by any other land-owner, is a familiar principle of the common law.

The State being thus possessed of a large area of wild lands, undoubtedly containing mineral deposits of value, one would think that the legislature should regard this mineral wealth as a trust to be carefully administered for the benefit of the people. This could be done in two ways: either by selling or leasing the mineral deposits, so as to benefit thereby the treasury of the State, or by granting the right to mine without payment to the State, for the sake of the general public benefit resulting from such an industry. The latter policy has been pursued with good results (however impaired by features of the United States law, which do not concern us here) by the Federal government in the administration of its public domain; and, no doubt, something may be said in favor of its adoption for the State of New York. That question will not be fully discussed at this time, but I feel bound to say that I think a thorough consideration of it would show that there is no such need of the exploration and

settlement of the public lands of New York as has existed with regard to those of the United States in the west; that there is no such special benefit to be derived from mining on the New York lands as would compensate the State for giving its rights away; and that the forest-problem, which is far more important to the commonwealth than the immediate development of any mineral deposits now known to exist in its public lands, will be seriously complicated by the granting of mining rights and easements, here and there, throughout the forest reserve. But, assuming for the moment that the policy of free grants of mining rights is a wise one for the State, we may still fairly insist that the question is too deeply important to be answered by hasty and partial legislation, and that such a policy should be expressed in a well-considered mining code, containing the safeguards suggested by the experience of centuries.

The United States, it is true, grants to its citizens the free right of mining on the public lands, but it requires them at the same time to perform a certain minimum amount of work annually, and to obey other regulations necessary to the maintenance of possessory title. The Legislature of New York, on the contrary, holding in trust the land of the people, deliberately gives away to the discoverer for twenty-one years any mine discovered thereon without requiring him to work it to any given extent, or for any given continuous period, or at all. Neither the State itself nor any citizen can acquire, during that period, the right to work that mine if the discoverer or his assigns do not make it productive. This is distinctly a law to discourage mining, and to encourage the speculative location of mines without the intention of serious immediate development. The only active mining which I can think of as possibly to be stimulated by it might work a very considerable injury to vested interests in the State. For the principal mineral deposits now likely to prove valuable on the New York public lands are deposits of iron-ore; and iron-ore is already largely mined in the State by parties who either own the lands they mine, and are subject to the taxes and the interest on cost, or else pay rent or royalty to the landowner. The legislature, squandering the assets of the State, now comes forward with the offer of free iron-mines, subject to no ground-rent or taxes on real estate, under the pretense of encouraging mining. If this measure should have any extensive effect, it is easy to say that it might be a serious injury to enterprises already established. I think its chief effect, for the present, will be to put the mineral resources of the public lands into the control of speculators.

Concerning the limit of twenty-one years, I say without hesitation that it would be better to give the title forever. The promise that the discoverer or his representatives shall be preferred in any contract for working the mine made after the expiration of that term simply hampers the legislature without imposing upon the mine-operator any such prudence and care as actual ownership dictates to intelligent self-interest. This point, and its bearing upon the economic development of the mineral wealth of the State, deserve a brief further discussion.

The mineral deposits on the State lands may be divided into two classes: those which will not be and those which will be more or less immediately exploited in good faith under this law. With incredible stupidity, the law recognizes no difference between them, and, as I have already pointed out, deposits of the first class are simply given away to speculators. But it is worth while to consider what would be the effect of the law with regard to the second class.

A mine is discovered on State lands, and after some years of preliminary development is worked on a large scale, and with large profit, by a capitalist to whom the discoverer has sold it. The owner becomes a millionaire through the bounty of the State, and by virtue of a simple robbing of the mine, extracting recklessly its richest and most accessible ores. It is his interest to get as much as he can out of the property before the end of his twenty-one years of free occupancy; and it is clearly not his interest to sink deep shafts, run long adits, or fill dangerous openings with permanent supports for the sake of the future beyond that period. In the worst case, he will gut the mine and let it cave, leaving it well-nigh inaccessible for further working. In the best case, he will certainly not inaugurate, during the last few years of his term, any work of permanent importance, whether in the way of explorations underground or of improved machinery above ground. If he understands his business, he will have the mine, at the end of the term, exhausted of available reserves. Even if he desires to continue working it, under the new contract contemplated by the law as to be made or authorized by the legislature, he will not be such a fool as to put the mine into promising condition, so as to stimulate rival bidders or attract the attention of law-makers to its apparent value. For the preference to which he is entitled under the law amounts only to this, that he can have the mine to work if he will pay as much as any one else offers, or as much as the legislature may demand, for the privilege.

But it is not necessary to rely upon an imaginary illustration.

Abundant experience shows the folly of leasing mineral lands without giving to the lessee (as does the United States law) the option of purchase. An instance in our own country is furnished by the history of the anthracite regions, in which the operation of the coal-mines by lessees (usually for twenty years) wasted untold millions in coal lost by reckless mining, and well-nigh destroyed forever the value of many mines. Yet in that case the leases were surrounded with safeguards as to methods of mining, etc., all of which are conspicuously absent from this crude and silly repetition of a ruinous experiment.

The only possible way to carry out such a policy would be to provide for a rigid inspection and control by expert State officials of the details of mining operations, with a view to preventing careless or wanton destruction of the mineral property of the State. Such a system would probably be both odious and ineffective; but to adopt the rest of the monarchical programme, and leave that out, is worse yet.

2. *The Mine.*—No attempt has been made by the legislature to define what is meant by "a mine" in either of the statutes quoted. Whatever it is, it cannot be a mine in the ordinary significance of the word, namely, an opening made upon a mineral deposit, because such a mine cannot be "discovered." Nor can it be a mineral deposit of known commercial value and suitable for development by mining; for these facts cannot be ascertained by simple "discovery." We are forced to conclude that the "mines" referred to by the law are all metallic mineral deposits. With respect to the State lands, building-stone, marl, phosphate-rock, clay, talc, cement, petroleum, coal, emery, sulphur, salt, fluorspar, graphite, asbestos, barytes, etc., as well as the ores of the metals, are clearly included in the phrase, "all mines and all minerals and fossils," and are given away as recklessly as are the metals. With regard to private lands, there is a grant to the land-owners of copper-, tin-, iron-, and lead-mines, carrying two-thirds of their value in these metals; and with this exception mines of gold and silver or other metals are declared to be the property of the State. I suppose this leaves the non-metallic deposits, as the common law leaves them, to the land-owner; yet the distinction is not perfectly clear after all.

A mine of pyrites, for instance, is worth less for iron than for sulphur. Is it included in the "all mines of other metals" which belong to the State?

Or, the ore of a copper-mine, carrying very little copper, may be

first roasted to produce sulphuric acid and subsequently treated for copper, and the commercial value of the copper may be less than two-thirds of the whole. Does that mine belong to the State? If it does (and so runs unquestionably the letter of the law), then the State is "encouraging mining" by taking away a land-owner's copper-mine if it is poor, and letting him keep it if it is rich.

Or, a lead-mine carries, together with galena, a varying proportion of zinc-blende. Does it belong to the State whenever the zinc-value rises above one-third, and does it revert to the land-owner whenever the zinc-value falls below one-third of the total value of the ore "on an average?" What is "an average," anyhow, in either of the three cases just supposed?

3. *The Discovery.*—The statute which gives away so freely both public and private property to the "discoverer" of a "mine," omits to define what shall be deemed a legal "discovery." No opportunity is afforded, at any stage of the proceedings contemplated, for a challenge of the bare assertion of the alleged discoverer. According to the statute, anything is a mine if the discoverer calls it such; and if it be situated on private land, his bare unsworn "notice of discovery" seems to be sufficient to prove: (1) that he is really the discoverer; (2) that it is really a metallic mine; (3) that all the copper, tin, iron, and lead which the ore may prove to contain will not amount to two-thirds of its total value; and a copy of this *ex parte* notice is to be recorded and indexed by the county clerk "as a conveyance of real estate from the people of the State." I feel sure that this provision, if not others, in Statute C, will be pronounced unconstitutional in so far as it involves the immediate and unconditional right of entry upon private lands, as I have explained above. But if I am wrong in that expectation, there remains another proposition of some importance. This law says nothing about the *manner* of the discovery. But a discovery upon private land cannot well be made without prior entry, and entry upon private land without the consent of the owner is a trespass; and it is well settled, I think, that no man can initiate a right by his own wrong-doing, and, therefore, that a discovery made by trespass could not be the basis of a valid legal title.

Section 2 of Statute C, however, clearly contemplates a discovery made without entry and without breaking the surface; for it grants these rights of entry to the "locator" (*i.e.*, "the person filing such notice" of discovery) only after the discovery which makes him a "locator." Such a "discovery" may have been made by looking

over the fence, or from a highway, into the private land, or by picking up pieces of "float-ore," which had evidently come from that land, or by tracing a vein outside to a point where it entered the land. Would any of these methods of discovery initiate a valid claim? In short, can any mineral discovery be legally made upon private land, even under this law, without the consent of the land-owner? I hope not; I almost think not; but the statute itself evidently contemplates such a thing. So far as it is concerned, a "discovery" practically consists in *filing a notice!*

Land-owners in this State could not do better, under the circumstances, than to make immediately upon their lands some sort of mineral discoveries, file notice of the same at Albany, have them recorded by the county clerks, and thus receive "from the people" the "real estate" which they thought they owned already.

4. *The Grant.*—Every mining law heretofore heard of, limits the extent of the mineral right granted to an applicant. But Statute C sets no limits. It gives away "the mine," which may be the whole mineral deposit, or a whole system of mineral deposits capable of being worked together. There is no trouble here about end-lines, or side-lines, apexes, spurs, dips and angles. The mine may extend (as Section 2 of Statute C intimates) into another county. As a matter of fact, the great iron-deposits of this State can sometimes be traced for a mile and more. Phosphate-rock and other deposits of economic value may extend for miles; and all these, if situated on public land, are subject to the same unlimited grant. On private land, the range of choice is more limited; but with respect to mines of zinc, quicksilver, manganese, aluminum,* nickel, cobalt, chrome iron-ore, antimony, molybdenum, wolfram, and platinum, there seems to be no limit to the "discoverer's" claim on a given piece of private land, except the boundary of the land. Whether he could also enter adjoining land on the strength of the same discovery, is a question. Certainly he could file another "notice of discovery," and so get another right of entry.

I have already shown that no authority is lodged anywhere by the law, to interfere with this course of spoliation. In one case only is a "location" to be "deemed abandoned," namely, if the locator does not pay the damages adjudged by the court to the land-owner. In that case, the land-owner may maintain an action of ejectment; but after ejectment, may he still become the victim of another "locator,"

* Bauxite is now clearly an ore of aluminum; and there is no knowing when clay may become so.

or does the mine, to which the "discoverer's" right has been extinguished, and which can never have another "discoverer," belong at last to the land-owner? I suppose it reverts to the State, to become the subject of further mad experiments in mining law.

On the public lands, however, there are no damages to be paid, and there is no provision under which the location can be "deemed abandoned." For the period of twenty-one years, the "locator" is "entitled," but not *obliged*, to work the "mine," on any scale, large or small; by any method, crude or perfect; for any purpose, speculative or commercial; with any amount of interruption, or not at all—just as he pleases; and this privilege is extended to his executors, administrators and assigns. Is there anything left, in the way of mineral wealth, which the Legislature of the State could grant away? If so, it must have been omitted by oversight; for Statute C seems to be an honest endeavor to bestow the whole, without reservation or condition.

The term of twenty-one years applies to private as well as public lands; and what I have said concerning it is equally true in both cases. It puts a premium upon the destruction of the mine.

5. *The Record.*—Every mining law worthy of the name provides for permanent and separate records of mining-titles and for the publication of notices to interested parties. But this wretched parody of a mining law declares only that the Secretary of State shall keep a book to receive the *ex parte* "notices of discovery;" and that the county clerks shall record and index such notices as conveyances of real estate from the people. John Smith has filed a notice of discovery upon certain private land of John Brown, which he describes by township- and section-lines, giving the name of the owner or not, as he chooses. Perhaps he does not know the name of the owner. That makes no difference; for the land-owner has no rights anyhow at this stage. Afterwards it may be necessary to hunt him up (if the county judge says so) in order to give "such notice to such land-owner as such judge or justice may direct" (Section 2, Statute C). But this is not required for the notice of discovery, which need only describe "the nature and situation of the mine." So the county clerk of any county containing any part of Brown's land records and indexes the "notice of discovery" as a conveyance from the people to Smith. It is thus ingeniously smothered in the index under two names, neither of which hints at Brown's ownership of the land; and it might become a matter of the greatest difficulty to discover the record of an easement which constitutes a cloud upon Brown's title.

6. *The Purpose.*—In my former paper (*Trans.*, xvi., 775) I mentioned that I had been consulted in behalf of a citizen of the State who had discovered on his own land what he believed to be a deposit of platinum and wanted advice as to his legal position. It appeared that there was nothing in the law, as it then stood, to meet his case, since, although a platinum-mine would clearly belong to the State, the Legislature had never made any provision for the recording and working of other State mines than those of gold and silver.

I have heard no more concerning that particular property; but the discovery of sperrylite, or arsenide of platinum, at and near Sudbury, Canada, where, I am told, some mining and shipping of that rare platinum-ore has taken place, has led to various alleged discoveries of platinum in the Adirondack region, and one of these, situated on the eastern shore of Lake George, has blossomed out into a somewhat pretentious mining enterprise, with a smelting-works as well as a mine. In several instances within my knowledge reputable chemists and assayers have failed to discover traces of platinum in the "ore," and it is not clear to me on what evidence the parties interested in this mine have expended their money thus far. But I have reason to believe that this scheme, whatever it may amount to, is connected with the passage by the Legislature of Statute C. If I am correct in that belief, the law referred to was intended, in somebody's mind, to enable the platinum company to control private lands along Lake George, and it is currently reported in that region that, by some means or other, this company or syndicate has acquired mining rights for a great distance along the lake. If this has been done in any case against the will of the land-owners, I trust it will be contested, so that the real force of the new law may be judicially determined.

Whether any of this company's claims are on State lands I do not know. If they are, then Statute C is significant in its omission of all mention of the Commissioners of the Land Office and the Forest Commissioners, either of which boards, under Statute B (*previously passed at the same session*), could have refused the permission which the new law grants unconditionally. It certainly looks as if Statute B had been found to contain too many safeguards of public interests and private rights and Statute C had been "put through" to remedy this difficulty by removing them all.

This subject would have been worthy of the consideration of the late Constitutional Convention, but that body gave it no attention.

LOSSES OF GOLD AND SILVER IN THE FIRE-ASSAY.

BY H. VAN F. FURMAN, DENVER, COLO.

(Bridgeport Meeting, October, 1894.)

To those engaged in the purchase and sale of gold- and silver-ores, mattes and bullion, the frequent differences in the gold- and silver-contents, as reported by different assayers, is a source of much annoyance and expense.

The desirability of uniform methods of assay has long been recognized by certain refiners who specify in their bullion-contracts the method to be adopted in assaying the bullion. One large producer of base bullion employs at each refining-works with whom it has a bullion contract an assayer whose duty it is to see that the bullion is sampled and assayed according to contract-specifications. For the assay of gold- and silver-bullion, standard methods are now generally adopted; the result being that differences seldom occur.

While we cannot hope for anything near perfection from fire-assay methods for ores, mattes and base bullion, as carried out commercially, such methods may be made to yield results which answer all commercial requirements, and they will probably be retained for obvious reasons.

The object of the present paper is to point out some of the sources of error and to show the importance of more uniform methods of assay and close attention to the details of the work.

The sources of error in the assay of ores and mattes are: improper sampling; losses of the precious metals during fusion, scorification and cupellation; imperfect elimination of the base metals on the cupel and imperfect extraction of the silver during the parting in nitric acid.

It is not the object of the present paper to enter upon the subject of sampling; but a few remarks may be of value. There has been considerable discussion recently in Colorado upon the frequent differences which occur in the assays of Cripple Creek gold-ores, and these differences have been attributed to various causes. The writer is informed by two of our most prominent smelters that since they have adopted 100-mesh screens (the general practice is 80-mesh) the

differences have ceased, so far as their assays are concerned. For gold- and silver-ores 100-mesh screens should always be used, and this is especially important in the case of gold-ores. In weighing out the samples for assay the ore should be spread out in a thin layer, the sample being taken upon the point of a steel spatula and care being exercised to take the sample from all portions of the layer and to drive the spatula down to the bottom of the layer each time.

The loss of precious metals during fusion, etc., is the greatest source of error. Messrs. Mason and Bowman * have recently published a table of experiments on the losses of gold and silver during scorification and cupellation. The averages of their results are as follows:

Silver in scorification, 0.55 per cent.; in cupellation, 1.99 per cent.; total, 2.54 per cent. Gold in scorification, 0.574 per cent.; in cupellation, 0.296 per cent.; total, 0.87 per cent. Unfortunately, they do not give any details adopted other than to state that they were such as would prevail in careful commercial work. Mr. F. P. Dewey,† in a recent article on "The Accuracy of the Commercial Assay for Silver," gives a number of results showing the loss of silver in the slag and in the cupel on different classes of material. The following tables, giving the results of a number of experiments made by the writer, show how these loses may be affected by differences in conditions during cupellation and parting. Many of these results exhibit unusual losses, far greater than would result in ordinary commercial work. Many of the experiments were made under unusual conditions for the purpose of showing the effect of such conditions and the necessity for the adoption of standard or uniform methods.

The three conditions which affect the losses during cupellation are temperature, amount of lead present, and amount of other base metals present. The temperature is the most important point; for, as the temperature is raised, the losses increase. An examination of Tables I. and II. fully proves this to be the case. The only safe rule to adopt in the assay of ores, mattes and base bullion, is to run all cupellations at the front of the muffle, and at such a temperature that the cupels will show "feather litharge." The lead-buttons resulting from fusion and scorification should not be too large. A

* *Journal of the Am. Chem. Society*, vol. xvi., p. 313, May, 1894.

† *Journal of the Am. Chem. Society*, vol. xvi., p. 505, August, 1894.

large button presents no advantage, but is the source of an excessive loss of gold and silver during cupellation, and requires more time for cupellation. The size of button preferred by the writer for ordinary ores (up to 10 ozs. gold and 300 ozs. silver per ton) is about 7 grammes. Whilst this is a somewhat smaller button than is usual, it is sufficiently large to collect all the precious metals, and avoids the above disadvantages. The presence of considerable amounts of base metals in the lead-button increases the losses. Hence, in running a crucible-assay of ores containing much arsenic, antimony or zinc, the fusion should be performed in an open crucible, and the charge should not be covered. Berthier was the first to point out the advantages to be derived by this method. It permits the oxidation of the arsenic, antimony and zinc, which unite with the soda and pass into the slag. Zinc appears to be the most objectionable element. That the fumes of zinc oxide have a tendency to carry off both gold and silver is well known. The following is a good illustration of this: A short time since the writer had occasion to examine the dust cleaned out of the flues connected with two furnaces used for the melting of gold bullion. This dust, which contained a large percentage of zinc oxide, assayed $18,400 in gold and $300 in silver to the ton. Copper also increases the losses, since, should the amount present in the lead-button be large, repeated cupellations with pure lead will be necessary for its complete removal. A better plan in such a case is to scorify the button with pure lead and borax-glass prior to cupellation.

As to errors arising from the imperfect elimination of base metals during cupellation, little need be said. This is, however, a point which needs close attention; and where the cupellations are run sufficiently cold to show feather-litharge the cupels should invariably be pushed back in the muffle just before the buttons "brighten." Should this precaution be omitted, the buttons are liable to solidify before the last traces of base metal have been removed. This is especially important when the lead-buttons contain much copper and gold.

Errors arising from imperfect extraction of the silver during parting are more common than is generally supposed. A short time since the writer examined some gold resulting from the assay of a number of samples of ore by one of our prominent smelting establishments. This gold was found to contain an average of 1.15 per cent. silver. The assays had been made in the regular manner em-

ployed at this establishment, but one acid (quite dilute) being used in parting. Those having experience in the assay of gold bullion, and in parting gold and silver, are well acquainted with the difficulty of removing the last traces of silver from the gold. The method of parting adopted by the writer is as follows: The buttons, which should contain at least five parts of silver to each part of gold, are flattened by a few blows on a polished steel anvil. They are then placed in small porcelain crucibles and dilute nitric acid (sp. gr., 1.13), is added. The crucibles are placed on a hot plate and heated until all action of the acid on the button has ceased. The acid should be at a boiling temperature towards the last. This acid is now poured off and strong acid (sp. gr., 1.27) is added. This acid is kept at a boiling temperature for four minutes, when it is poured off, the crucible and its contents are washed twice with distilled water, and then dried, and the gold is annealed and weighed. It is proper to observe that the conclusions above given are the result not only of the experiments recorded in the following tables, but also of numerous other experiments and several years' experience in assaying for gold and silver.

In the following tables, each part of gold or silver represents 0.5 milligrammes. The gold and silver used was especially prepared by the writer and its purity thoroughly tested. The purity of all fluxes, etc., used was also tested.

Table I. shows the results of experiments undertaken to determine the losses in gold during cupellation under various conditions.

Table II. presents the results of a number of experiments undertaken to determine the losses in the assay of base bullion under various conditions. The variation is considerable and forcibly illustrates the necessity of cupelling sufficiently cold to have the cupels show feather-litharge. The results do not appear to show that any advantage is to be derived by scorification of the bullion prior to cupellation, except in the case of extremely base bullion. A number of years' experience in the assay of base bullion and numerous tests by the writer but confirm this opinion, although some smelters insist that all bullion should be scorified prior to cupellation, contending that the loss in scorification is much less than in cupellation. In the case of bullion containing considerable amounts of zinc, scorification is always preferable.

The parting in every case was performed in two acids of 1.15 sp. gr. and 1.27 sp. gr. respectively.

Table I.

No.	Parts Au Taken.	Parts Ag Taken.	Grammes Pb Taken.	Parts Ag and Au After Cupellation.	Parts Au After Parting.	Parts Ag.	Loss Au Per cent.	Loss Ag Per cent.
A 1	799.8	.....	7	795.4	.....	.....	0.55	
2	799.8	.....	7	795.0	.....	.....	0.60	
3	799.8	.....	7	795.0	.....	.....	0.60	
4	800.3	.....	7	794.8	.....	.....	0.69	
5	799.9	.....	7	795.7	.....	.....	0.52	
6	799.9	.....	7	798.0	.....	.....	0.23	
B 1	200.7	.....	4	194.4	.....	.....	3.14	
2	200.8	.....	4	194.6	.....	.....	3.09	
3	200.6	.....	4	195.3	.....	.....	2.64	
4	200.4	.....	4	197.3	.....	.....	1.55	
5	200.4	.....	4	199.1	.....	.....	0.65	
C 1	200.1	.....	8	192.4	.....	.....	3.85	
2	200.0	.....	8	191.4	.....	.....	4.30	
3	200.1	.....	8	193.7	.....	.....	3.20	
4	200.0	.....	8	196.2	.....	.....	1.90	
5	200.6	.....	8	199.4	.....	.....	0.60	
D 1	800.0	200.2	7	987.7	.....	.....	1.23	Loss Au and Ag Per cent.
2	799.8	199.7	7	987.7	.....	.....	1.18	
3	799.8	199.6	7	989.0	.....	.....	1.04	
4	799.8	200.0	7	989.0	.....	.....	1.08	
5	799.9	199.7	7	994.2	.....	.....	0.52	
E 1	200.2	1000.6	7	1173.6	200.2	973.4	0.00	2.75
2	200.1	1000.8	7	1177.5	200.4	977.1	†0.15	2.34
3	200.7	1001.2	7	1185.4	201.4	984.0	†0.35	1.65
4	200.0	1001.4	7	1190.9	200.0	990.9	0.00	1.05
F 1	100.35	1000.	7	.....	100.35	.....	0.00	
2	100.35	1000.	7	.....	100.25	.....	0.09	
3	100.9	1000.	7	.....	100.45	.....	0.44	
4	100.4	1000.	7	.....	100.25	.....	0.15	
G 1	101.9	1000.	7	.....	101.7	.....	0.19	
2	102.2	1000.	7	.....	102.4	.....	†0.19	
3	101.5	1000.	7	.....	101.1	.....	0.39	
4	102.0	1000.	7	.....	102.2	.....	†0.19	
H 1	100.6	1000.	7	.....	99.3	.....	1.29	
2	100.0	1000.	7	.....	99.8	.....	0.20	
3	100.1	1000.	7	.....	100.8	.....	†0.69	
4	100.3	1000.	7	.....	100.5	.....	†0.19	
5	100.0	1000.	7	.....	100.9	.....	†0.90	
						Average	0.86	

† Indicates a gain in place of loss.

Remarks:

A.—No. 1 was cupelled at the back of the muffle, the others being placed in order up to No. 6, which was at the front. The muffle was at a lower temperature than is ordinarily employed.

B.—No. 1 run at the rear and No 5 in front of the muffle. The temperature was higher than in A.

C.—The conditions were the same as in B.

D.—The conditions were the same as in A.

E.—The conditions were the same as in B. In parting but one acid (sp. gr. 1.15) was used.

F.—No. 1 at the rear and No. 4 in the front of the muffle. The temperature was higher than is usual in commercial work. The parting was performed in two acids of 1.07 and 1.27 sp. gr. respectively.

G.—The conditions were the same as in F, except that the first acid was 1.20 and the second acid 1.27 sp. gr.

H.—The conditions were the same as in F, except that in the parting but one acid (sp. gr. 1.15) was used.

TABLE II.—*Losses of Gold and Silver in the Assay of Base Bullion.*

No.	Parts Au Taken.	Parts Ag Taken.	Grammes Pb Taken.	Parts Ag and Au After Cupellation.	Parts Au After Parting.	Parts Ag.	Loss Au Per cent.	Loss Ag Per cent.	
A 1	3.6	300.6	14.5	295.8	3.4	292.4	5.55	2.67	
2	3.55	301.0	14.5	282.6	3.4	279.2	4.25	7.24	
B 1	3.5	300.6	14.4	296.5	3.3	293.2	5.71	2.46	
2	3.0	300.7	14.4	296.9	2.9	294.0	3.33	2.23	
3	3.7	300.2	14.4	280.6	3.4	277.2	8.11	7.66	
4	3.45	300.3	14.4	281.6	3.05	278.5	11.59	7.26	
C 1	4.3	300.5	14.2	297.8	4.2	293.6	2.32	2.23	
2	4.1	300.4	14.2	278.8	4.0	274.8	2.50	8.52	
D 1	3.8	300.7	14.2	290.1	3.5	286.6	7.90	4.68	
2	3.7	300.0	14.2	288.5	3.4	285.1	8.10	4.96	
3	3.5	300.5	14.2	271.0	3.35	267.6	4.30	10.94	
E 1	3.6	300.0	14.	287.6	3.3	284.3	8.33	5.23	
2	3.0	300.0	14.	288.1	2.7	285.4	10.00	4.87	
3	4.2	300.2	14.	275.7	3.8	271.9	9.52	9.43	
4	3.2	300.6	14.	270.2	2.8	267.4	12.81	14.37	
F 1	4.2	300.0	14.	291.4	3.9	287.5	7.14	4.17	
2	4.2	301.8	14.	279.5	4.1	275.4	2.38	8.74	
G 1	8.7	331.0	14.5	334.8	8.65	326.15	0.57	1.46	Aver.: Au 1.53 Per ct. Ag 1.80 Per ct.
2	5.15	340.8	14.5	340.6	5.05	335.55	1.94	1.54	
3	5.1	326.9	14.5	326.8	5.00	321.8	1.96	1.56	
4	5.5	334.7	14.5	334.7	5.40	329.3	1.82	1.61	
J 1	3.4	317.1	14.5	314.4	3.37	311.03	0.90	1.92	
2	3.6	332.65	14.5	327.2	3.56	323.64	1.11	2.71	
K 1	4.8	332.4	14.4	326.0	4.65	321.35	3.12	3.32	
2	4.4	349.6	14.4	339.8	4.2	335.6	4.54	4.00	
L 1	3.9	314.6	14.4	308.9	3.7	305.2	5.13	2.98	
2	3.7	315.8	14.4	311.4	3.5	307.9	5.40	2.50	
M 1	3.95	302.0	14.2	290.9	3.72	286.18	5.82	5.23	
2	3.2	306.9	14.2	296.3	3.00	293.3	6.25	4.43	
N 1	3.6	300.0	14.	292.4	3.55	288.85	1.30	3.71	
2	3.3	312.0	14.	306.9	3.25	303.65	1.51	2.68	

REMARKS:

A.—No. 1 was cupelled in the front and No. 2 in the middle of the muffle.

B.—Nos. 1 and 2 in the front and Nos. 3 and 4 in the middle of the muffle; 300 parts of copper were added.

C.—No. 1 in front and No. 2 in the middle of the muffle; 600 parts of copper were added.

D.—Nos. 1 and 2 in front and No. 3 in the middle of the muffle; 600 parts of zinc were added.

E.—Nos. 1 and 2 were in front and Nos. 3 and 4 in the middle of the muffle; 300 parts of copper, 350 parts of zinc and 300 parts of antimony were added.

F.—No. 1 in front and No. 2 in the middle of the muffle; 200 parts of copper, 250 parts of zinc, 200 parts of antimony and 200 parts of iron sulphide were added.

In all of the above cases the buttons were cupelled at too high a temperature to show feather-litharge on the cupel.

In the following experiments the cupellations were all performed in the front of the furnace and all cupels showed litharge crystals:

G.—The gold and silver were wrapped in pure lead and cupelled cold.

In the following experiments the charges were scorified prior to cupellation, the buttons after scorification weighing from 5 to 6 grammes:

J.—14.5 grammes of lead were used.

K.—300 parts of copper were added.

L.—400 parts of zinc were added.

M.—300 parts of zinc and 300 parts of copper were added.

N.—300 parts of zinc, 250 parts of copper, 300 parts of antimony and 350 parts of sulphide of iron were added.

Table III. shows the results of a few experiments undertaken to determine what losses might be expected in the crucible-assay of ores. Whilst these results show too great a variation for any averages to be derived from them, they tend to show that the crucible-assay for gold is more accurate than is generally supposed. The average loss in silver (2.58 per cent.) in these experiments is quite close to that found by Messrs. Mason and Bowman in the scorification assay (2.54 per cent.).

TABLE III.—*Losses of Gold and Silver in Crucible-Assay.*

No.	Parts Au Taken.	Parts Ag Taken.	Parts Au and Ag After Cupellation.	Parts Au After Parting.	Parts Ag.	Loss Au Per cent.	Loss Ag Per cent.
A 1	4.7	216.3	215.6	4.65	210.95	1.06	2.47
2	4.0	205.5	206.5	4.0	202.5	0.00	1.93
B 1	4.0	208.0	208.7	4.0	204.7	0.00	1.58
2	3.6	217.6	217.2	3.6	213.6	0.00	1.84
C	202.5	2000.	.....	202.6	.....	+0.04	.....
D	200.8	.....	200.3	.....	.....	0.28	.....
E 1	24.25	306.0	321.0	24.23	296.77	0.08	3.01
2	24.6	317.5	330.1	24.5	305.6	0.40	3.74
F 1	26.4	320.7	340.4	26.4	314.0	0.00	2.08
2	22.3	308.9	324.0	22.3	300.7	0.00	2.65
G 1	13.4	226.8	235.3	13.3	222.0	0.74	2.11
H 1	14.5	215.2	225.8	14.5	211.3	0.00	1.81
2	15.3	214.9	223.0	15.2	207.8	0.65	3.30
K	14.4	232.0	235.4	14.25	221.15	1.04	4.66
					Average...	0.30	2.58

REMARKS:

A.—½ A. T. of pure SiO_2 was added.

B.—10 grammes of SiO_2 and 5 grammes of ZnO were added.

C.—½ A. T. of SiO_2 was added. The button was not weighed prior to parting.

D.—½ A. T. of SiO_2 was added. No silver was added to this assay.

E.—5 grammes of SiO_2, 5 grammes of $BaSO_4$ and 5 grammes of FeS were added.

F.—10 grammes FeS, 3 grammes ZnO, and 2 grammes Cu were added.
G.—7.5 grammes of SiO_2 and 7 grammes $BaSO_4$ were added.
H.—5 grammes SiO_2, 5 grammes $BaSO_4$ and 5 grammes CaO were added.
K.—7.5 grammes SiO_2 and 7 grammes FeS were added.
To the above charges there were added the proper fluxes to produce a fluid slag and a button of from 7 to 12 grammes in weight. The cupellations were performed so as to show "feather-litharge" on the cupels in each case. All partings were performed in two acids of 1.15 sp. gr. and 1.27 sp. gr., respectively.

It appears to the writer that the practice, now quite generally adopted in Colorado, of determining the gold by crucible-assay and the silver by scorification-assay presents many advantages. As the crucible-assay permits of larger quantities of ore being taken, errors from variations in weighing out the sample and the final gold-bead do not multiply themselves, as is the case where $\frac{1}{20}$ A. T. or $\frac{1}{10}$ A. T. is taken. Where a wind-furnace is at hand, in which to perform the fusion, the California practice of using large charges (2 A. T. to 4 A. T.) presents some advantages, especially in the case of low-grade ores.

In Colorado, the fusion is usually performed in the muffle-furnace, so that two or more crucible charges of $\frac{1}{2}$ A. T. each are run for gold, silver being added to the charge, unless the ore is known to contain sufficient to insure parting, and three or more scorification-charges of $\frac{1}{10}$ A. T. are run for silver. In the case of very high-grade ores, the amount taken for the silver assay is $\frac{1}{20}$ A. T. In the case of many low-grade oxidized silver ores (as some lead carbonates, slags, etc.), the writer prefers the crucible-method for both gold and silver; otherwise the assays are made as above.

OUR POSSIBILITIES.

BY HENRY M. HOWE, BOSTON, MASS.

(Extract from the Presidential Address at the Virginia Beach Meeting, February, 1894.)

. . . . So, beyond and above the immediate and tangible ends of membership, ever-present and evident, selfish and generous alike; beyond and yet at the root of that genuine affection for our association which is so strong in many of us, there lies earnest sympathy with the ultimate and essential purpose of its being—that to which as a nucleus all other purposes are but contributory—which is, to

increase and diffuse our professional knowledge, and thus progressively to improve our methods, so that each of the products of our arts shall represent a smaller expenditure of labor, and conversely, that a given expenditure of labor shall be the commercial equivalent of a larger quantity of those products; that a day's labor shall purchase a greater amount of comfort and happiness; and that, of the total expenditure of human energy, a smaller proportion may be needed for supplying purely material wants, and a larger proportion be available for still further increase of knowledge, for culture, and for refinement. That such ends, perhaps rarely enunciated, perhaps often only hazily conceived, mixed though they are with other and more immediate purposes, are in the final analysis the most important part of the foundations of societies such as ours, I cannot doubt.

Looking at our Institute broadly, then, it is an organization, an instrument, not for the benefit of our members alone, but for the good of the human race. How grave, then, the responsibility of its wise direction, how important that its course should have the most earnest attention, the most serious thought, not alone of the Council whom we elect to execute our wishes, but of those from among whom our future councils must be selected, that is to say, of the whole membership.

Every new achievement raises a man to a higher standpoint, shows him new possibilities, opens larger fields to view. As with men, so with societies. We, too, are to grow. We, too, shall find new channels of usefulness. Even if we already more than fulfill the expectations of the founders of our society, yet our size and strength may well open to us a future greater than was to be thought of in those early days.

It seems not inappropriate that a presidential address should touch on our possibilities, and offer the suggestions which long and strong friendship for the Institute awakens. Their fruit may not be recognizable—how much is an oak like an acorn?—but into the minds of our Council, and of those who some day are to be our Council, they may drop seeds which by and by may yield a harvest.

With these reflections in mind, let us now consider our *Transactions*—perhaps our greatest work and chief means of usefulness, certainly our greatest source of expense.

If we measure their value by that of the publications of kindred societies, we have good reason to be proud of them. But let us not say, "Our *Transactions* are as good as others, perhaps better;"

rather let us ask, "Are they as useful as we can make them? Are we doing all that lies in us?" Let not our aim be to excel others, but to equal our own possibilities. Take what I shall now say in this light, not as ungenerous criticism, but as urging that the work, already noble, be encouraged by its very nobility to still greater achievements.

The value of our *Transactions* may be measured jointly by their quantity and their quality. It is of the first that I shall speak.

The question, "Can we enlarge them?" should be resolved into two: "Is there further suitable material to be had?" and "Can we afford to take more?"

I am firmly convinced that, were we in a position to accept more, we could greatly increase the volume and value of our papers, and still more greatly the volume and value of our discussions. I am convinced that the results of many important investigations now go unpublished which would, on a judicious appeal, yield papers of value. Indeed, it is not always the most valuable matter that offers itself.

I am confident that an overwhelming majority of those who have carefully considered the matter strongly favor stimulating discussion actively, for they prize it most highly, as a light in which to read the paper discussed, as an estimate of its merit and trustworthiness by the best judges, and as fixing a vast amount of information which is often full of suggestion and implication, and which would not otherwise be recorded.

The papers of the Chicago meeting, which, with their discussions, will occupy two large volumes, give an idea of what can be had, though the special attractions of that meeting were doubtless an additional spur to writers. Any material increase in the volume of papers, however, would involve great expense, not alone for printing and engraving, but for the work of the Secretary's office in editing and revision, a work the severity and extent of which probably few of our members fully appreciate. And any material increase in the amount of discussion would, I believe, involve proportionally a much greater expense; for the labor of correspondence, in getting their remarks from those who discuss, and in arranging their revisions of those remarks, is very great, and demands intelligence of a high grade.

But no unexpended surplus distends our vaults; our exchequer gives no suggestion that we have gathered less than we can care for; no hint that we can prudently increase our expenses. What matters, then, an excess of harvest over store-house?

Of the fields of usefulness which are open to us, there is another well worthy of our consideration, the publication of a digest of technical advances. In such a matter it is well to avoid duplicating the established work of others. The *Journal of the Iron and Steel Institute* contains a most valuable abstract of the advances in the metallurgy of iron and steel, with notes on the mining and preparation of iron-ore, of refractory materials, and of fuel. The *Proceedings* of the Institution of Civil Engineers give us lucid digests of the advances in the arts to which that society chiefly devotes itself. Might not our Institute well supplement these by an abstract of the progress in mining, in concentration, and in non-ferrous metallurgy? For this, the labor of men of special qualifications would be needed, but would it not increase greatly both our usefulness to our race and the value of our *Transactions* to ourselves?

In short, like so many other societies, had we more to spend we could spend it profitably. Can we then increase our resources?

Two ways suggest themselves—the establishment of a large fund and an increase in our annual assessments. In offering these suggestions, I remind you that a sister society adopted the second of these two expedients a little more than two years ago, and apparently without retarding its own growth. It is economical to allow others to experiment; it is wise to study carefully their results, and to see what light they cast on our own problems.

Of course, no such step should be taken without most thorough and deliberate study. As the conditions of the two societies are far from identical, it may indeed appear, on closer examination, that for us this course would be unwise or even dangerous. But the general resemblance between the conditions is so close as to raise a strong presumption that what has been so successful in one case would succeed in the other.

Success in this experiment might well increase very greatly our power of usefulness. Just as each man, taken as an individual, is morally bound to give his powers their fullest possible development, so I take it a society of men has no more moral right to neglect to develop its powers of usefulness as fully as possible. Here the presumption seems to me so strong in favor of so great an increase in our powers as to press this question home upon us, to raise it from the level of mere detail to that of broad general policy, and to make it well worthy of presentation even in a presidential address.

The present time of financial depression, though unfavorable for

the execution of such an experiment, may not be a bad one for beginning to study its advisability.

Finally, would it not be well that important researches in mining and metallurgy should be directly stimulated by prizes, or perhaps by pecuniary assistance under proper restrictions? Are there not some among us who would gladly found such means of inciting our investigators and of rewarding their labors? Would not our Council be an excellent mechanism for administering such foundations?

These are questions easy to ask, but to be answered with care; these are projects readily proposed but difficult of execution. Raising them not thoughtlessly, nor unmindful of obstacles, I commend them to your serious and deliberate consideration.

PYROMETRY AND THE HEAT-TREATMENT OF STEEL.

BY HENRY M. HOWE, BOSTON, MASS.

(Extract from the Presidential Address at the Virginia Beach Meeting, February, 1894.)

I NOW call your attention to two directions in which very important progress may be confidently hoped for—pyrometry and the heat-treatment of steel.

We already find in the market more than one pyrometer which, in spite of serious defects, may be used advantageously for determining and controlling the temperature of many of our metallurgical processes. It is to be hoped, and with much confidence, that by the rapid extension of the use of such instruments, the economy and efficiency of these processes may undergo an increase which, in the aggregate, will be of the greatest importance. In this connection, the lucid paper of Prof. Roberts-Austen,* and the learned bulletin of Mr. Barus,† are most welcome.

But much remains to be done in the way of devising pyrometers suitable for manufacturing purposes. While some of the present pyrometers are most convenient and accurate in the laboratory, serious difficulties attend the use, in factories, of those which I have

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* *Trans.*, xxiii., 407.

† *Bulletin No.* 54, *U. S. Geol. Survey*, 1889.

examined. I am inclined to believe that, for a great number of purposes, by far the most promising and the most practicable pyrometric method should be the measurement of the heat or light radiated from the hot object or space which is under examination. It should permit wider and much easier use than any method which demands that some part of the pyrometer itself, or of its system, be brought to the high temperature which is to be measured.

The papers of the past year, which I proposed to group together under the title of the "Physics of Steel,"* are not the only evidence of the serious study this most important question is receiving. One result of these studies will be a better adaptation of the chemical composition of the metal to the needs of each special use; but a far more important result, I believe, will be a great improvement and extension of the methods of treatment by heat, at least for steel, and possibly for the other metals of construction.

In the hardening and tempering of tools, in the annealing of steel castings, in the chilling and annealing of car-wheels and other cast-iron objects, and in the manufacture of guns, projectiles, and armor, we have already extensive and careful if empirical heat-treatment. Even in the manufacture of rails, plates, and tires we have been in a measure forced into a kind of heat-treatment; for we may class under this head the regulation of the finishing-temperature.

The evidence before us scarcely permits us to doubt that, by proper treatment of steel, we may at will very greatly increase the tenacity, the elastic limit, the ductility, the resistance to wear, or a combination of these properties; and that we can thus fit our metal much more closely to its needs, with the result of greatly improving the objects which it forms, or of greatly lightening them. For this there is good reason to believe that relatively simple and inexpensive methods will suffice. If such methods of greatly increasing the usefulness of steel lie before us, simply awaiting exploration and application, it is certain that they will be explored and applied.

For instance, a ready and safe means, such as we may hope for, of attaining in steel greater resistance to repetition of stress, should permit us to decrease greatly the weight of many structures, especially of those in which weight is objectionable in itself, as it is in the cases of ships, of bridges, and rolling-stock, and in many others.

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* See *Trans.*, xxii., 105, 243 and 546; xxiii., 37, 113, 466 and 608; also the discussion on this subject in the present volume, p. 759.

We are promised a great increase in the wearing-power of rails through lowering their finishing-temperature, and by slightly varying that of steel plates it has been found possible to regulate their physical properties.

In what we have hitherto done in the study and in the application, whether scientific or empirical, of heat-treatment, we have only nibbled; we have hardly done more than to gain an idea of the vastness of the unsailed ocean of usefulness that stretches before us.

We are not to expect extremely rapid progress, for two distinct and very serious obstacles lie in our way—our own ignorance and that of the users of steel. We have only scanty and isolated fragments of knowledge as to how heat-treatment should proceed in order to induce this or that specific set of properties in steel; and the users of the metal know certainly as little as to what combinations of properties are best fitted to the needs of actual practice.

Each problem has great difficulties. On the one hand, not only the *rationale* but the laws of the influence of heat-treatment are most elusive; on the other, we can hope to learn what combinations of properties are best suited to the conditions of indefinitely prolonged use, only by extensive and almost indefinitely prolonged experiment.

Fortunately many investigators, some of them well equipped, are now earnestly studying each problem. Not only those who experiment directly with heat-treatment, but also the students of the relation between composition and properties, the microscopists or metallographers, and the etchers, are jointly approaching the former problem from many directions. They attack with arms of precision, improved pyrometric, photographic and micrographic methods. And many important mechanical laboratories, among the very foremost of which stands our government equipment at Watertown, are firmly laying the foundations of thorough knowledge of the properties needed to fit steel best for its most important uses.

May the fruitfulness of their labors be increased by co-operation. Learning from each others' work the points of strategic value, may each concentrate his attack on their lines of least resistance.

I for one hope to see the day when the use of untreated steel for our important purposes will be regarded as we should now regard the use of cast-iron for rails.

BIOGRAPHICAL NOTICE OF J. H. BRAMWELL.

BY E. C. PECHIN, CLEVELAND, O.

(Bridgeport Meeting, October, 1894.)

SECRETARY'S NOTE.—Mr. Bramwell was born in 1846, at Liverpool, England, his father being English, and his mother American, of Scotch ancestry. Brought to the United States in early childhood, he was educated at the public schools and graduated from the College of the City of New York. He subsequently studied his profession as a mining engineer at the Royal School of Mines, Berlin, Prussia, and was for a short time in the service of the Prussian government at Königshütte. Returning to this country in 1871, he was employed during succeeding years as mining engineer, chemist, and manager at various furnaces and iron-works in Pennsylvania and the Southern States until, in 1883, he became actively engaged in the development of the Flat-Top coal-field of southwestern Virginia, in which region the town of Bramwell was named after him, in recognition of his services as a pioneer of the great industry which he did so much to promote. After prolonged and fluctuating ill-health, he died, July 13, 1894, at Paris, France.

At the opening session of the Bridgeport meeting, the Secretary announced his death, and Mr. Pechin, being called upon, made the remarks reported below.—R. W. R.

WIDELY as Joseph Herbert Bramwell was known, I think I knew him somewhat better than any one else. After finishing his studies abroad, he came to this country, a very young man, seeking an engagement. This was about the beginning of 1871, as I remember, and I was then trying to make iron at Dunbar, at the foot of the Allegheny mountains, in Fayette county, Pa. His engaging manner and charming *personnel*, combined with his letters of introduction, made me more than satisfied in securing his services as my chemist. At the outset we lived a sort of pioneer life, and for many months he was practically a member of my family, and the affection then formed remained unbroken to his dying day.

I vividly recall the circular of April, 1871, signed by Eckley B. Coxe, R. P. Rothwell and Martin Coryell suggesting a meeting at Wilkes-Barre, Pa., for the purpose of forming the American Institute of Mining Engineers. At the last moment, prevented from going, I sent Bramwell; and I am more than glad that his name appears among those whose sagacity established an Institute that has become illustrious by reason of its numbers, the character of its members, its invaluable papers and its enormous influence for good

the country over. Of that little band of twenty-two only seven names now appear upon our roll. Bramwell's interest in the Institute never for a moment flagged.

Ordinarily men are not lovable; but he was a lovable man. In many ways he had the moral and mental susceptibilities of a woman; while at times as gentle and simple as a child, he always had the heart of a lion and nerves of steel. Those who knew him in his early days will recall his splendid *physique* and manly presence. He did not know what personal fear was. A few incidents in his career, not generally known, I think, will furnish all the eulogy he needs.

He was in Germany at the time of the Franco-Prussian war, when typhus fever in a malignant form broke out among the French prisoners. Volunteers being called for to nurse them, this young man, almost a boy, with everything to live for, willingly gave his services to the sick and dying strangers of a race not his own.

After leaving Dunbar, he had charge of a small furnace, likewise close to the mountains, where he was annoyed and harassed by moonshiners coming in and selling whiskey to his men. This became at last so intolerable that he determined to put a stop to it at any cost. One day, towards evening, a desperado came in for his usual traffic, whom Bramwell determined, if possible, to arrest. Starting with an associate, whom he outran, Bramwell chased the man for several miles, and as he gained upon him the ruffian turned and emptied a revolver at his pursuer, fortunately without effect. Overtaken at last, and secured single-handed, he was held until assistance arrived, and he was taken to jail. I asked Bramwell how he felt when the fellow was shooting at him, and his reply was characteristic: "I don't think I thought at all, except that I would catch him if I could."

Some of you may remember the burning of a theater, on Broadway, New York, a number of years ago. At that time there was an old-fashioned, three-story building in front, with projecting window-sills and cornices. During the height of the fire, when the theater in the rear was in flames, the ghastly, bleeding head of a man was seen to appear for an instant at one of the smoking third-story windows and then to fall back exhausted. As yet the ladders had not reached the fire, and the destruction of the unfortunate victim seemed imminent and inevitable. At this moment a fashionably-dressed young man in the crowd threw off his overcoat and was raised by willing hands until he could grasp the projection over the door of the adjoining building. Drawing himself up, he

clambered from window to window until he reached the one where the man had been last seen. Disappearing in the thick smoke, he shortly reappeared, bearing the disabled wretch in his arms, and held him until the ladder was raised and both were rescued. The hero, who refused to give his name to the surging and enthusiastic crowd below, was Joseph Herbert Bramwell.

These incidents reveal the man. He was keenly alive to the misfortunes of others, and freely lent a helping hand. No one will ever know of his many and unostentatious benefactions. He himself had more than once passed through the bitter waters of business adversity and learned a lesson that he never forgot when fortune smiled upon him.

Commencing with nothing but a well-stored mind and enormous energy, for a number of years filling responsible but not remunerative positions, he subsequently become the lessee of Quinnimont Furnace, in West Virginia, and after several years of indefatigable exertion and incessant toil he left Quinnimont with a considerable sum of money, every cent of which was sunk at Ironton, Ohio, not by his own fault, but by a combination of circumstances he could not control.

Once again, with nothing but his enlarged experience and superlative courage, he started afresh in a new and then comparatively unknown field, the Flat-Top coal-field of West Virginia. He went as a pioneer into those wild mountains and gorges, suffering all sorts of personal discomfort and privation day after day, week after week, month after month, performing his duties with absolute faithfulness and ultimately bringing a large success to himself and associates.

Splendid as was his business zeal, he was as gallant in his fight with death when attacked by a violent disease, the result of exposure. For five years he was a constant, brave struggler against the inevitable. Racked with intense pain, which the best skill could only alleviate; at times undergoing severe surgical treatment; careless of himself with partially returning health and strength; in the prime of life, with his financial condition assured, with the unbounded respect and esteem of his business associates, and, in addition, the affection of unnumbered friends, he met on a foreign shore the grim enemy from whom, sooner or later, there is no escape.

His whole life is a shining example to the young men who are following his professional footsteps in the Institute, of which he was an organizer and, to the last, a member, honored and beloved.

DISCUSSIONS.

GEOLOGICAL DISTRIBUTION OF THE USEFUL METALS IN THE UNITED STATES.

Continued Discussion of Mr. Emmons's paper. (See vol. xxii., pp. 53 and 732.)

(Virginia Beach Meeting, February, 1894.)

William Hamilton Merritt, F.G.S., Toronto, Ont. (communication to the Secretary): With reference to Mr. Emmons's remarks on the nickel-deposits of Sudbury in the district of Algoma, Province of Ontario, Canada, and his general argument in connection with igneous rocks as the source of supply of ores, I would draw attention to two series of facts personally observed in connection with the Sudbury deposits.

1. *The Diorite as a Source of Supply.*—The diorite, as a rule, is speckled with pyrrhotite, and to a less extent with chalcocite. In the diorite I have seen a speck of free gold quite visible to the naked eye. The same diorite, decomposed, has been run through a prospector's stamp-mill in an earthy or gossany state, and free gold and the new platinum mineral, sperrylite ($PtAs_2$), perfectly crystalline, have been collected from it. Near by, a quartz-vein, cutting the same diorite formation, has yielded rich samples of free gold, which occurs also in the diorite walls beside the vein, the wall-rock being, in some places, thickly interlaced with threads of gold.

2. *Secondary Action in Concentration has Produced Some of the Ore-Bodies.*—As has been noted by Mr. Emmons, the theory of differentiation, or the concentration of the ore-bodies in the fused magma, is offered to explain the source of the Sudbury nickeliferous pyrrhotite masses in the intrusive diorite. This would not appear unreasonable. Yet I am satisfied that a secondary concentration must have taken place to explain the presence of some of the ore-bodies; at all events, where the contents of copper run comparatively high. The presence of the horses of country-rock cemented by the ore (alluded to by Mr. Emmons), and which I have observed sharply brecciated, seems to strengthen this belief; for if they had been floating in a fused magma, surely all abrupt demarcation would be obliterated.

Moreover, I have seen such signs of secondary action as thin films of native copper in connection with the ore-body; also a band of

"fluccan" cutting across it, which may indicate a portion of the chief channel which may have filled the cavities caused by movement. Native copper grains are also seen in the crystalline hornblende-rock associated with the deposit. I have not observed serpentine, which might be expected to occur. In another portion of the same ore-body there is free gold visible in chalcopyrite; and where a very small crack or vug had occurred in the ore, wire-gold has been developed. The ore-body is distinctly more quartzose than the diorite in which it occurs.

JAW-CRUSHERS.

Continued Discussion of Mr. Douglas's paper. (See vol. xxii., pp. 321 and 647.)

(Virginia Beach Meeting, February, 1894.)

C. E. Dewey, Georgetown, Colo. (communication to the Secretary): In his remarks on crushing-machinery, Mr. Douglas says (*Trans.*, xxii., p. 322):

"To insure a smaller and more uniform size of particles in the crushed material, the Dodge crusher still communicates motion to the upper extremity of the movable jaw, which oscillates on fixed pivots projecting from its lower extremity."

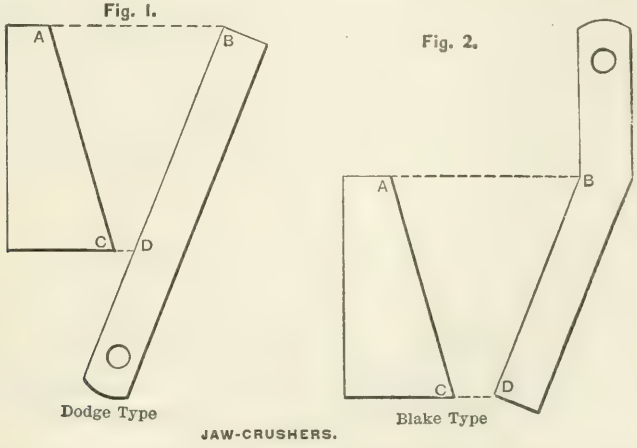
The notion that, in order to obtain a fine and uniform product from a rock-breaker, the movable jaw must be pivoted at its lower extremity, seems to obtain generally among mill-men, as well as among manufacturers of machinery. My experience with machines of both types does not confirm this belief. It is true that, to obtain a uniformly fine product from a jaw-crusher, the stroke at the lower extremity of the crushing-surface must be short; but that the movement at that end must be less than at the other or receiving-end is, I think, an erroneous assumption.

Figs. 1 and 2 are intended to illustrate the two types of machine.

It is found in practice that the capacity and the nature of the product of either machine, as usually constructed, are governed by the opening, C D, and the length of stroke of the movable jaw at D. This being the case, machines of either type, of the same width of crushing-surface, running at the same speed, and having the same adjustment as to opening and stroke at D, should give, when fed

with the same material, practically the same product both in fineness and in total amount.

The principal difference in the working of the two types lies in the manner in which they attack the coarse ore. In the Dodge type the movable jaw has from two to three times the velocity and length of stroke at B that it has at D, while the Blake has but one-half to one-third the velocity and stroke at B that it has at D; consequently (the adjustment at D being the same in both), the velocity and stroke of jaw at B in the Dodge type will be from four to nine times that at the same point in the Blake. The result is, that the Dodge type breaks the large lumps much as a man would break them with a hammer, while the Blake, by comparatively quiet pressure, cracks



the large pieces, allowing them to settle down as room is made for them. That the latter method does an equal amount of work with less shock to the machine and to the building, and is, in general, more satisfactory, I think will be admitted by those who have had experience with both kinds.

Since, by the proper adjustment of parts, any desired length of stroke of jaw can be obtained in either machine, it seems clear that any greater uniformity or fineness of product from one machine as compared with the other, will be obtained at the expense of capacity.

AMERICAN BLAST-FURNACE PRACTICE.

Continued Discussion of Mr. Potter's paper. (See vol. xxiii., pp. 370 and 577.)

(Virginia Beach Meeting, February, 1894.)

James Gayley, Braddock, Pa. (communication to the Secretary): Mr. Potter has made reference to our practice at the Edgar Thomson works in the direction of increasing the life of the lining, and mentions the product of one of our furnaces as having reached a total of 370,000 tons. It may be interesting to know that this furnace has been put out of blast with a record of 452,685 tons, after a run of three years and nine months. A companion-furnace to this will produce, before the present year is ended, half a million tons. As Mr. Potter has quite correctly designated this as "the newest problem in furnace-practice," a description of the construction of this furnace-lining may be welcome to members.

The bricks used in the lining are 9 and 13½ inches long, with the requisite quantity of straight and key-shapes to turn the circle. These bricks are well suited for enclosing the numerous bosh-plates used for cooling. The bosh-plates (the efficient fortress of the lining) extend from the top of the tuyere arch to the mantel, and are inserted in rows, 18 inches to 2 feet apart. These form a perfect defence against encroachment on the bosh-lining, and leave the bosh at the end of the blast of the same thickness as at the beginning. The protection afforded by these bosh plates is the principal cause of the increased duration of the furnace-lining. But it must be noted that the shape and material of the plate are essential elements. The most serviceable plate is that made of bronze or copper. We have tried every form of iron plates without obtaining satisfactory results. These were replaced with bronze plates, built into the lining, which gave excellent results so long as they did not leak; but when this occurred, as it did in time, the water was shut off, and, since the plates could only be removed with difficulty, they were allowed to burn off. This permitted encroachment on the bosh-walls, which thus became enlarged beyond the limit required for economical practice. With the introduction of the removable plate, made tapering on the sides, with plain surfaced top and bottom, these troubles have

been overcome. A plate can be easily removed when a leak occurs, and a new one can be inserted in less than half an hour. By this means the bosh-wall is preserved in shape throughout the whole blast, and economy of practice is maintained.

One point, particularly worthy of mention, has been observed recently, viz., that the removable bosh-plate (of bronze or copper) protects not only the bosh-wall, but also the whole of the upper lining, from the point where the stock strikes against the top-lining down to the mantel. The wear of the lining, for double the production, is only about half what it was when the bosh was protected differently. The result, as amply demonstrated by practice, is, that the fuel-consumption does not increase with the length of the blast, as has been the usual experience; and it also follows that the production maintains its regularity.

Mr. Potter has ably summarized the great progress made in American blast-furnace practice during the past twelve years; yet, great as it has been, I venture to predict that the next twelve will show as long a stride beyond our present achievements.

THE PHYSICS OF STEEL.

Continued Discussion of various papers on this subject. (See vol. xxii., pp. 105, 243 and 546, and vol. xxiii., pp. 37, 113, 466 and 608.)

(Virginia Beach Meeting, February, 1894.)

Albert Sauveur, South Chicago, Ill. (communication to the Secretary): Mr. Howe remarks (*Trans.*, xxiii., 656) that, running through my paper, there is a tacit assumption that there is a constant and known relation between the coarseness of the grain of steel and its quality. The above assumption was not made tacitly. On the contrary, it is based upon the very experiments embodied in my paper. Indeed, most of those experiments were undertaken with that end in view, *i.e.*, to ascertain the existence of such a relation.

Mr. Howe calls attention to the fact that of two pieces of manganese-steel, originally from the same bar, the coarser-grained showed the greater strength and ductility. This refers to the fracture of the pieces; whereas, I have considered the micro-structure only, independent of the fracture; and I do not think that a coarse fracture

necessarily corresponds to a coarse micro structure. It would be interesting and instructive to examine, microscopically, a polished and etched sample of each piece, and see whether, in this special case, the larger micro-grain gives the greater ductility. The same remark applies to Mr. Hadfield's mention (*Trans.*, xxiii., 613) of a German casting, which gave good results in the testing-machine, and showed a very coarse fracture.

I would add, that my experiments related to carbon-steels only, and it would be hazardous to extend my conclusions to manganese-steel, and other steel-alloys, without further investigation.

It may be well, in connection with this discussion, first, to recall very briefly the main conclusions to which I was led in studying the microscopic structure of steel, as set forth more in detail, together with the evidences sustaining them, in my paper, on the "Micro-structure of Steel," presented at the Chicago meeting (*Trans.*, xxii., 546).

All steels crystallize when quietly and slowly cooled from a temperature, x, or higher. This temperature, x, varies with the chemical composition of the metal. Generally speaking, we may say that each impurity lowers x, although in very different degrees. That is, the more impure the steel, the lower will be the temperature at which crystallization ceases, and, consequently, the coarser the grain in the finished piece, for a given heat-treatment.

The higher the initial temperature, from which the steel is allowed to cool undisturbed, and the slower that cooling, the larger the micro-grains for a given chemical composition.

There is a constant relation between the physical properties of steel and the size of the micro-grains, provided we compare with each other steels of the same class only; that is, containing the same, or nearly the same, amount of carbon. We find, then, that the smaller the micro-grains the greater the ductility of the metal, as judged from its percentage of elongation and of reduction when submitted to tensile strain. The relation which the grain bears to the ultimate resistance has not been so clearly demonstrated. Up to a certain size of grain, the tensile strength diminishes together with the elongation and reduction, but not by any means so rapidly. Beyond that point, it shows a tendency to increase, while the ductility continues its rapid fall.

In most of our experiments, samples of Bessemer rail-steel were used, and for this class of metal, only, was a sufficient number of cases recorded to justify our proposition concerning the relation be-

tween micro-structure and physical properties; but, there seems to be no reason why it would not apply to any class of steel. Indeed, it is quite natural and logical to infer that its application is general.

Work does not affect the size of the micro-grains. It is true, that if two pieces of steel, identical in composition, size, and shape, and similarly heated, are worked down so that one of them will receive a greater reduction than the other, the former will show a smaller grain; but this is due to the lower temperature at which that piece was necessarily finished, and not to the greater amount of work it has received. Therefore, hot-work, *as such*, has no effect on the micro-structure of steel; but, *indirectly*, by retarding crystallization, it does indeed influence it most decidedly.

The practical deductions from the foregoing propositions are numerous and valuable. If we accept the micro-structure as a symbol of the physical character of the steel (which, I think, has been satisfactorily established), our last-stated proposition has an important corollary: In a sound piece of steel, which has been rendered such by a sufficient amount of work, further work will not improve the quality, unless indirectly by retarding crystallization. If this beneficial retardation could be accomplished by heat-treatment alone, or by any other means, the result would be the same.

Whether the micro-structure is affected by the casting temperature, or by the heating previous to forging or rolling, has not been ascertained. No doubt, these two factors have an extremely important influence on the quality of the finished product, mainly by their intimate correspondence, the former with the disposition and size of blow-holes, the latter with the closing of the same. But I am inclined to think that if they do affect the micro-structure, it is only indirectly, by their influence on the finishing-temperature.

One fact which the microscope has demonstrated in a most satisfactory way is the effect of heat-treatment on the micro-structure of steel and consequently on its physical properties. It shows the close relation which exists between size of grains, finishing-temperature and rate of cooling. When the use of the microscope shall have become more general in connection with steel-making, its usefulness in this direction will be readily established and greatly appreciated. Too little attention is generally paid to the extremely important part played by heat-treatment. Micro-structure is a function both of chemical composition and of heat-treatment. Chemical composition alone cannot confer the desired quality on a finished piece of steel, where heat-treatment has not been equally well planned. The hurt-

ful effect of some impurities is greatly attenuated by proper, and greatly intensified by defective, heat-treatment. This is not new; but it is too often lost sight of, and only imperfectly understood.

If we want to secure the maximum toughness which a certain composition will permit, we must finish our piece at as low a temperature as practicable. The beneficial effect of forging, and hence finishing, at low temperature has received due recognition in the new steel-rail specification of the New York Central and Hudson River Railroad Company, which asks that the rails be rolled at "as low a heat as possible."

When hardness, or rather power to resist abrasion, is required together with toughness, as is the case for rails, it might be inferred that by finishing at a low temperature and thereby securing a tougher metal, we would at the same time reduce the wearing-quality of the rail. To this objection it may be answered that what causes hardness is the amount of carbide of iron present in the metal. Carbide of iron (Fe_3C, probably) is the hard constituent of unquenched carbon-steel. (We do not consider here the hardness due to other impurities; but with regard to them our reasoning would be the same.) Now the amount of carbide does not vary with the finishing-temperature. By lowering that temperature we do not in any way reduce it, hence we do not alter the hardness; we simply retard crystallization, thus securing a smaller grain, which means increased ductility.

The problem which the steel-maker should try to solve is, how best to combine chemical composition and heat-treatment in order to secure a desired structure. Of course it is seldom, if ever, that the conditions of manufacture are such that these two factors can be varied at will. It is usually only within limits, and often very narrow ones, that they can be altered. Sometimes for practical, mechanical or economical reasons the heat-treatment cannot be changed, at least not to the desired extent; the chemical composition only being controllable, and that within limits. Sometimes the heat-treatment alone is susceptible of some variation, the composition being perforce a constant, or nearly constant factor. But in all cases there is a combination of the two factors, which lies within the possible limits, and which will yield the best results obtainable under the circumstances.

The microscope can greatly assist us in solving this problem. The changes in the micro-structure of a finished piece due to corresponding variations of composition and treatment, can be noted and

followed more readily and much more accurately (especially for slight variations), than the corresponding changes in the physical properties. This is indeed a promising field, opened to microscopical metallography.

W. Bell Dawson, Montreal, Canada (communication to the President): With regard to the vexed question of the relation between the percentage of carbon in steel and the physical properties of the metal, I venture to made a suggestion which I trust may give rise to some discussion on the subject. I may add that it has occurred to me in making tests of Bessemer steel for rails, ranging from 0.30 to 0.55 per cent. carbon, and low in all other elements; and also tests of railway car-axles.

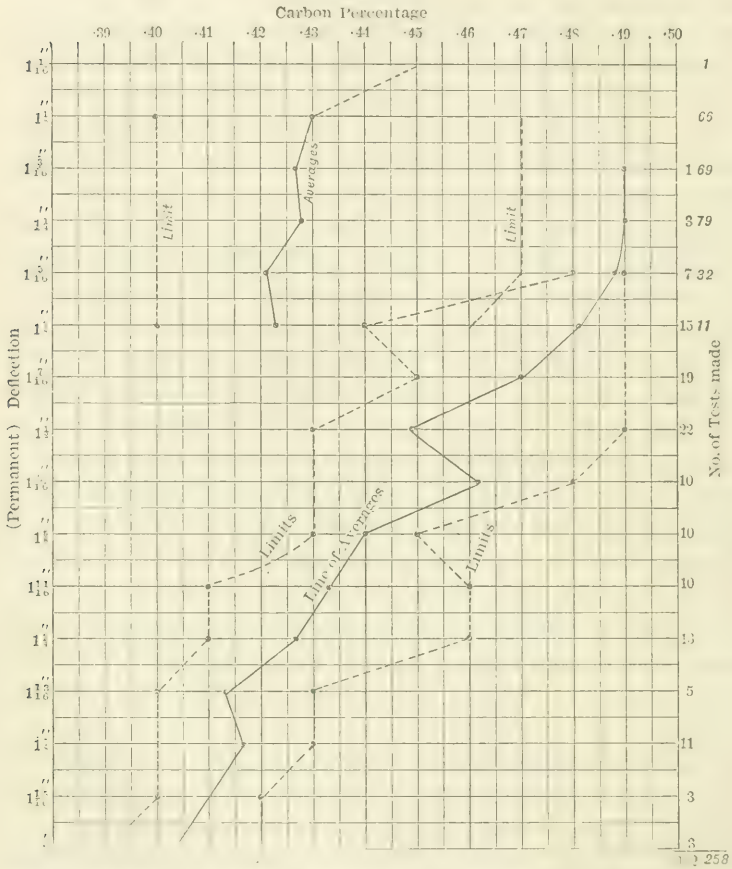
It is now known from the researches of Abel and others, that carbon exists in such steel in two states (neither of which is graphitic), and which may be called "hardening-carbon" and "cement-carbon." This explains how steel of the same tenor in carbon may have such different physical properties; since the relative development of these two states of the carbon depends on amount of working, the temperature down to which working is continued, and also upon the "rate of cooling," which latter term may be taken to include all rates from slow annealing to sudden chilling or quenching. Unfortunately, no satisfactory chemical method is yet known for the quantitative determination of these two states of the carbon. There is Abel's method, and there are also some German ones, which permit the carbon in these states to be detected and distinguished; but the relative amounts still depend too largely upon the strength and temperature of the solutions used, etc., to make them reliable practically for analysis.

To meet this difficulty, the matter might best be taken up from the other side. Given the total carbon which the steel contains (of which, of course, none is graphitic), and assuming the steel to be reasonably low in all other elements; given also the mechanical tests; calculate from this the amount of strength which the steel has relatively to the carbon it contains; and take the result to indicate how far the strength has been developed in manufacture from the grade of steel in question. Thus with two steels of the same tenor in carbon, the one which has the greatest strength is the best-worked; and this method would show how far the process of manufacture has really developed the best qualities possible from a given grade of material.

It is necessary, however, to define what we mean by the "strength"

of the steel; and here again we are usually at fault on the mechanical side. The stress per square inch at which the material breaks, or its breaking-strength, is not a true measure of its real strength for practical purposes, except, perhaps, in the case of steel for structures.

FIG 1.



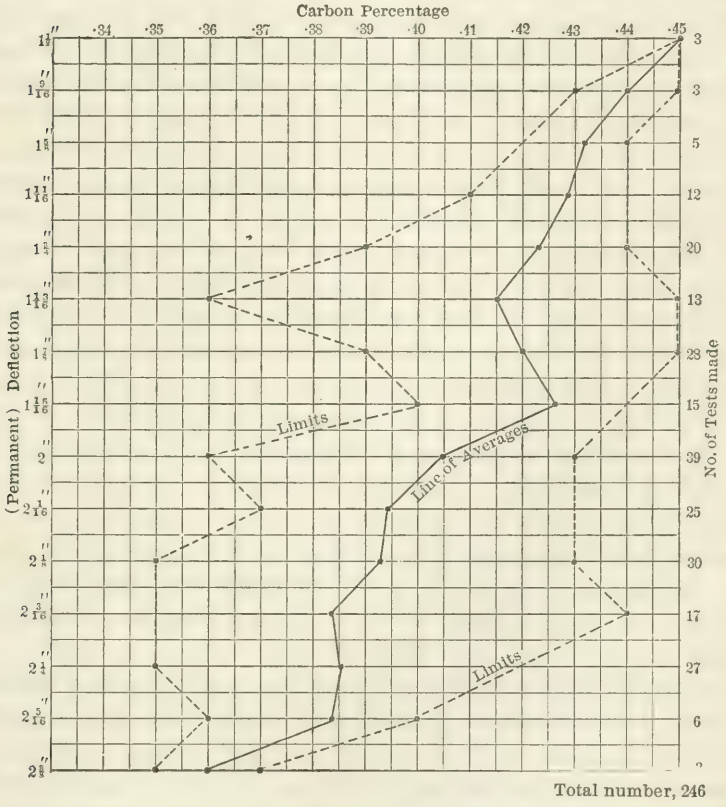
Comparison of deflection under drop-test with percentage of carbon in 72-pound rails. (Rail $4\frac{1}{16}$ inches high, $4\frac{1}{2}$ inch base; moment of inertia, 23.53. Drop-test, 2240 pounds, falling 9 feet on head of rail set on bearings 3 feet apart.) The right-hand column of "No. of tests" (in italic figures) refers to the left-hand curve of the diagram. The two curves represent rails manufactured by two different makers.

But in the case of rails, tires, and axles, they have to stand severe usage from rolling and impact, and from stresses applied or reversed with great suddenness. It is also easy in the grades of steel used for these purposes, by means of suitable tempering, to increase the break-

ing-strength in a high degree, even to the verge of brittleness, provided the amount of elongation is sacrificed to a corresponding extent.

It is clear therefore that, in these cases, the idea of strength includes toughness, and that it must be expressed in some way that will combine both breaking-strength and elongation.

FIG. 2.



Comparison of deflection under drop-test with percentage of carbon in 72-pound rails. (Rail $4\frac{1}{16}$ inches high, $4\frac{1}{2}$ inch base; moment of inertia, 23.53. Drop-test, 2240 pounds, falling 12 feet upon head of rail set on bearings 3 feet apart.)

This principle is admitted in some recent specifications and rules on railways. For example, in the United States Navy the steel for armor-plate is accepted on a sliding scale of breaking-strength and elongation, which are allowed to vary in contrary directions within certain limits. Also, an arbitrary rule is in use in Germany for

steel rails, by which the breaking-strength and the reduction in area, expressed in metric units, must make up a certain total when added together. The requirement in both these instances is, substantially, that the elongation must be greater as the breaking-strength decreases in order that the one may make up for the other.

This shows, when viewed in a mathematical light, that the true measure of strength will be obtained by expressing it in terms of the "work done" in causing rupture of the material. We may thus define strong material to be that which combines the greatest breaking-strength with the greatest elongation, expressed presumably as a product, and within reasonable limits; and assuming further that the elastic limit bears a satisfactory ratio to the breaking-strength, as it almost invariably does in reasonably good steel. The strength so measured will indicate both resistance and toughness in the material, and will exclude weakness and brittleness. This method of expressing the strength also permits a measure for it to be obtained by means of the drop-test, as well as from the results of a tensile test; because in the drop-test also the amount of work done in breaking the piece is readily computed, and can be brought into relation to the deflection. This is illustrated in Figs. 1 and 2. It will be noticed that in Fig. 1 the drop is 9 feet; in Fig. 2 it is 12 feet.

To sum up for the purpose of discussion, I may conclude with the following statements:

1. The attempt to determine the physical properties of steel which correspond with a given tenor of carbon is based upon an incorrect assumption, because it is now known that these properties depend much more upon the state of the carbon than on its total amount.

2. The correct method is to grade the steels according to their carbon, and to determine in the steels of each grade the amount of strength, measured as here indicated, which has been developed by its treatment in manufacture. The special points in the treatment which will influence its strength are the lowness of the temperature down to which it has been worked, and the final rate of cooling; as these are the chief factors which modify the state of the carbon for each grade of steel.

By this method the comparative value of different ways of treating the material could be investigated.

President H. M. Howe, Boston, Mass.: I take this occasion to offer some further comments upon the papers concerning the physics of steel, read at the Chicago meeting.

Micro-Structure.—On the micro-structure of steel we have three important papers. First, an admirably lucid exposition, by M. Osmond, of the condition of our knowledge, with the most instructive grouping of photographs which I have yet seen. His statements seem accurate and just in general, but his declaration under the head of "C. *Relations between the Structure and the Composition of Steels*" (*Trans.*, xxii., 253), that steels of different hardness and in different states differ only in the proportion and distribution of the three apparent chief mineralogical constituents, ferrite, pearlyte, and cementite, while probably near the truth, bears, I think, a more positive form than our present knowledge justifies, and one that is, perhaps, unintentionally positive.

Under "*b. Influence of Time*" (p. 256), M. Osmond says that rapid cooling yields a finer grain than slow cooling. This of course is meant to apply to the grain as revealed by polished sections. But previous observations seem to show that the grain of the fracture is about as large in suddenly-cooled as in slowly-cooled steel. In other words, heating, under usual conditions, seems to afford time enough for the grain of the steel to grow nearly or quite to the maximum size corresponding to the temperature reached, so that the rate of cooling, though it affects the texture of the grain, does not seem to affect materially its size (unless, indeed, the grain be broken up by forging just before the cooling begins, in which case slow cooling affords time for the grain to grow again). M. Osmond's assertion thus seems to imply a marked difference between the effect of rapid cooling on the grain as shown by fracture, and on the grain as shown by polished sections. In view of this, we wish that his photographs of hardened (suddenly-cooled) steel told from what temperature the sudden cooling occurred, so that, by comparing their grain with that of pieces cooled slowly from the same temperature, we could directly verify his assertion.

As different as possible from M. Osmond's paper, which interprets and explains, is that of Prof. Martens (*Trans.*, xxiii., 37), which presents a veritable mine of direct, minute and careful observations, purposely and perhaps needlessly refraining from co-ordination and inference. We cannot praise too warmly the conscientious care to which this paper testifies, nor thank him too heartily for the valuable information which he lays before us.

Prof. Martens criticizes, and perhaps with some justice, the theorizing in which some have indulged concerning the nature and functions of the components of steel. To attempt to replace established

theories by means of others which explain no new facts is a grave error. To assert that a theory is proved when it remains a mere conjecture is a sin—would it were a crime. But these are abuses of theory; and one need not dwell long on this earth before discovering that liability to abuse is no warrant for neglect. Which of the good and useful things of this world is not abused?

A first and even a second reading of Prof. Martens's paper, and especially of the paragraph beginning in the middle of p. 58, produce the impression that he has erred in the direction of neglecting theory, or rather the co-ordination and grouping of results, and one even fears that he intends postponing the explanation of what he sees till he can invariably recognize each component unerringly. Now, if we may judge by other branches of science, this simply means never.

A theory which groups the facts together so that the mind can grasp them, and can convey its thoughts about them to other minds, even if it be known to be untrue, is better than none. If we wait till we get a perfect comprehension of the nature of anything before enunciating a theory, we shall never enunciate one; for of the inmost nature, even of the simplest things, we are ignorant.

The project set forth in Mr. Sauveur's paper (*Trans.*, xxii., 546), of determining the relation between the average size of the grains of steel, as measured by a planimeter, on the one hand, with the physical properties of the metal, and with the heat-treatment which it has undergone on the other hand, is full of promise and interest. His propositions seem, in general, to accord with experience; using them as first approximations, may we not still further sharpen our definition? For instance, taking his first proposition, that undisturbed slow cooling from a certain high temperature enlarges the grain, is it slow cooling as such, or the exposure to a high temperature as such, that enlarges the grain? If, as I believe, it is the latter, then we have a much wider proposition, one to which his proposition would stand as a corollary.

Taking next his third proposition, that the temperature x is lowered by the presence of carbon and phosphorus, does the evidence really support it? We find that the size of the grain increases with the proportion of carbon and phosphorus. Does this prove that the grain of the phosphoric steel continues growing after the temperature has fallen below the point at which the grain of non-phosphoric steel ceases growing? Is not the larger grain of the phosphoric steel due rather to more rapid and larger growth in the summer of

high temperature, than to further prolongation of the period of growth into the late autumn of the cooling?

Again, is not his Proposition V., that, "The slower the cooling the larger the grain," true only within narrow limits? Is it not a corollary to the more general proposition that, at any high temperature the grain reaches the size corresponding to that temperature, not instantaneously, but by a gradual and continuously retarded growth?

We must admire the instructive way in which the photographs which compose Plate IV. are groupe l, so as to show the influence of finishing-temperature on the grain. Yet, if the positions from which sections 3 and 5 were taken are accurately shown, is it not surprising that their grain is so much finer than that of sections 7, 9, 12, and 14?

Mr. Sauveur's Fig. 3 indicates that, as the size of the grain increases, the tenacity at first decreases slightly, and then seems to increase greatly. Though, as he justly intimates, no strong conclusion can be drawn from so scanty data, it is interesting that this apparently unexpected result harmonizes with one of mine which was certainly unexpected. In Table 18 of my paper on the heat-treatment of steel,* soft tool-steel has greater tenacity when heated to 851° C., and thence cooled slowly, than when cooled slowly after heating to 725° C., though in the former case its grain should be much greater than in the latter.

Perhaps Mr. Sauveur misunderstands my reference to his supposed "tacit assumption." In giving my conjecture as to his attitude, and in using it as a peg to hang a plea on, I aimed not at all to criticize but rather to justify his remarks. He indeed asserts that the elongation and the reduction of area are inversely as the size of the grain, and he gives valuable evidence in support of this assertion. These properties, however, are but one component of merit; tenacity and elastic limit are for many purposes far more important. Now, the tacit assumption which I thought underlay his remarks, and which I expressly approved, was that the general merit and value of the metal are inversely as the size of the grain. My exact words were "the finer the grain the better the quality."

I do not see that he asserts explicitly anywhere that this is true, much less that he has proved it. I understand that he, tacitly and justifiably, assumes it for purposes of argument and otherwise, as all the rest of us have done. His paper contains valuable steps towards

* *Trans.*, xxiii., 527.

changing this assumption to a demonstration. My plea was for more such steps. Many remain to be taken; and I look to Mr. Sauveur as one of the men with the best opportunities for taking them. Till they are taken, and wherever an assumption is required, I for one shall go on assuming, tacitly or explicitly as may seem appropriate, that general merit is inversely as the size of the grain; and I hope and believe that Mr. Sauveur and all other intelligent men will do the same.

Segregation.—There is a tacit assumption in M. Pourcel's paper (*Trans.*, xxii., 105), that rapid cooling restrains segregation. Is this true? On first considering the subject, one naturally thinks so; but, on reflection, the reasons for this opinion are not strong enough to permit us to hold it confidently on purely *a priori* grounds, and without direct evidence. What does direct evidence show? Can any one inform us on this very important question?

Segregation, indeed, is especially marked in large ingots, and large ingots cool slowly. But does this mean that the slow cooling favors segregation, so that, of two like ingots, the slower cooled will suffer the greater segregation; or does it simply mean that segregation is more noticeable in the larger ingot, because the segregated mass is proportionally larger?

R. W. Raymond, New York City: Mr. Howe has asked for direct evidence as to the relation of slow or rapid cooling to segregation. Does not our blast-furnace process furnish such evidence, with regard to the segregation of carbon in graphitic form? It is a familiar fact to all furnace-men that the segregation of graphite is favored by slow cooling.

President Howe: As Dr. Raymond points out, if we cool cast-iron suddenly, we restrain the separation of graphite; but, is this case parallel with that of the segregation which occurs in steel ingots? When siliciferous cast-iron cools slowly, graphite indeed separates out, but it remains relatively uniformly distributed through the mass. The segregation which we dread in steel ingots, however, consists not in a uniform distribution of the segregated elements, but in their concentration into the last-freezing part of the ingot. In cast-iron, the graphite is probably the first-freezing substance, and the metallic portion is the real segregation in one sense. May not the same be said of the splitting up of copper-alloys?

These cases, as well as the crystallization of aqueous solutions, certainly suggest very strongly that slow cooling should favor segregation; indeed, both such *a priori* reasoning as I have followed,

and such evidence as I have seen, suggest this strongly. Yet I am forced to admit to myself that both reasoning and evidence are far from conclusive, when one friend, with excellent opportunities for observation, insists that slow cooling actually does oppose segregation in steel ingots, and when another very learned one shows by a mathematical process, which evidently satisfied him, though not me, that slow cooling ought to oppose segregation. Therefore I seek direct and unequivocal evidence.

Next, we naturally assume that we ought to restrain segregation; and so we should, provided that we cannot separate the part of the ingot which contains the segregation. But, suppose that we can get the segregation into a part of the ingot which we can throw away, or use for some purpose for which the segregation does not unfit it, then, should we not rather try to favor segregation? For the objectionable elements, phosphorus and sulphur, seem to segregate most; segregation, then, is a purifying or improving operation, concentrating part of these objectionable elements into a part of the ingot which can be cut off.

Now the segregation will be found in the last-freezing part of the ingot, and this part will be the nearer the top of the ingot, the more slowly the ingot cools. Hence is it not in many cases well to meet the problem of segregation by making large and hence slow-cooling ingots; by allowing them to cool extremely slowly and on end, for instance by setting them in soaking-pits as soon after casting as possible, and thereby raising the segregation as near to the top of the ingot as we can: and finally by shearing off enough of this top to remove the segregated mass?

In order to answer this question we must know how near to the top of the ingot it is practicable thus to raise the segregation. The expediency of this course must still be determined for each special set of conditions.

A. E. Hunt, Pittsburgh, Pa.: At the suggestion of our President, I call attention to the fact that aluminum has been found, when added in proportion of less than 1 per cent., to prevent materially the segregation of phosphorus in large ingots. The usual proportion of aluminum added for this purpose is only from one-fourth to one-half pound to the ton of steel.

My own suggestion as to the reason for this action is, that the aluminum causes the molten steel to solidify much more evenly. Steel with aluminum added to it does not "chill" at the surfaces of the mould and "freeze" towards the center, leaving, for a consider-

able period, a mass of molten "mother-liquor," as it were, in the center of the ingot, as does ordinary metal. Steel with aluminum added to it, when the temperature is lowered to the solidifying-point, seems to "freeze" altogether; and the needles of solidifying metal can be seen to dart across the whole surface of the top of an ingot, and the metal at the top of the surface of the ingot to become solid almost instantaneously, when the "freezing" operation once commences, although the metal, as a whole, seems to remain molten a longer time with aluminum added than metal similarly treated without the addition of aluminum. Segregation is in this way restrained by preventing the mass of metal in the interior of the ingot from remaining molten for a much longer time than that on the surface —which has been considered the physical reason for segregation.

President Howe: Mr. Hunt's observation that aluminum restrains segregation is interesting and important. Let us hope that he will give us the quantitative results of some direct experiments on this point.

The explanation which he puts forth could readily be tested. Let us prepare two like ingots; let the final additions to one contain aluminum; let those made to the other contain none. A few seconds after solidification has set in, let each be emptied. If Mr. Hunt's explanation be true, the ingot which has received aluminum should have much thicker walls than the other.

But, even if this explanation be true, there will be some difficulty in comprehending it. In order that a body like a steel ingot shall freeze across suddenly, *i.e.*, in order that its interior shall freeze at the same time as its exterior, it is necessary that interior and exterior shall reach the freezing-point simultaneously, and hence that the temperature of the mass shall be uniform at this time. The escape of heat, which permits the mass to cool towards its freezing-point, occurs only through the exterior, which, in order to conduct heat away from the interior, must be cooler; and the temperature cannot be uniform throughout the mass, unless rapid circulation counteracts this effect which conduction has, of cooling the outside faster than the inside. Uniform temperature, therefore, implies rapid circulation. Now, there is a considerable range of temperature between the point at which molten iron, as it cools, begins to grow viscid, and that at which it becomes completely solid. To this it owes its ease of welding. As it cools through this range, circulation becomes more and more difficult and slow. We should expect it to become so slow that the complete solidification of the outside would occur

some considerable time before that of the center. But, during all this interval between the time when the metal becomes viscid enough to impede circulation, and thus to make the cooling of the inside drag behind that of the outside, and the time when the metal becomes truly solid, segregation may go on. The metal is too viscid to be of constant temperature, and yet mobile enough to permit segregation.

If aluminum is to change this state of affairs, so as to permit freezing to occur simultaneously throughout the mass, it seems that it can do so only by causing the metal to pass suddenly from a highly liquid to a completely solid state, as water does, and by thus eliminating the long range of increasing viscosity and plasticity through which iron passes. But is this probable? Does steel, to which aluminum has been added, show, in forging or welding, any token of such an abrupt change? Has it not the same ease of welding, the high degree of plasticity, shading off gradually into viscosity and incipient liquidity, that we find in otherwise similar steel which has not received aluminum?

The fact that the upper surface of an ingot may freeze across suddenly, as great needles shoot across it, should not mislead us here. The whole of the upper surface may be at the same temperature, and hence freezing may occur across it suddenly, just as it may occur suddenly along the whole of one side exposed to the mould. But this is no indication that it thus occurs suddenly across the interior of the ingot.

Henry D. Hibbard, High Bridge, N. J.: A few practical observations may be appropriate.

Blow-Holes.—In the manufacture of soft steel ingots, say with less than 0.30 per cent. of carbon, there are two methods which may be followed relating to treatment of the gases, and thereby of blow-holes in the steel.

The first is the plan usually followed in the manufacture of ingots for boiler-plates, wire, and similar purposes, which we may call the *evolution* method, inasmuch as the gases are not kept in solution during teeming, but their free escape or evolution is encouraged at that time, giving the well-known scintillating effect in the moulds. This effect is due to the escape of myriads of bubbles of gas, which throw up a shower of small scintillating sparks. When this is properly done, the ingot will have a solid outer layer from 1 to 2 inches thick; then a zone containing numerous gas-bubbles with bright surfaces; and, inside of that, a pretty solid center. Such steel may be called incipient rising-steel, as it will rise a little if allowed

to do so just before final solidification, because the metal last to solidify is too pasty for the free evolution of gas. Ingots made by this method will be free from pipes.

The holes above mentioned stick together pretty well in rolling, and cannot be found in the finished piece; that is, unless one knows where and how to look for them.

The position of the zone of blow-holes is primarily dependent on the casting-temperature. The higher this temperature, the nearer to the surface will be the zone. When the heat is too high, the holes are so close to the surface that only a thin skin of steel covers them, and the broken ingot shows a structure an inch or more deep around the outside, closely resembling honey-combs, giving the well-known pitted steel with which steel-melters are but too familiar.

The second plan, almost universally followed in the production of high-carbon steel ingots, and to some extent of soft steel ingots as well, we may call the *solution* method. By the addition to the melted steel, shortly before casting, of metals having great solvent power on the gases, the latter are kept in solution, and the resulting ingot is practically free from blow-holes, but will have a pronounced pipe, unless some mechanical means are employed to prevent it. In the crucible-process, the solvent added is silicon, which is reduced from the slag and from the material composing the pot. In the other processes the additions are made from outside sources.

In the Swedish exhibit at the World's Columbian Exposition, there were ingots for the manufacture of plates, ranging from 0.15 to 0.50 per cent. of carbon, made by the solution method, the solvent being evidently aluminum. They were absolutely free from blow-holes and made a very fine showing.

Pipes.—For many purposes, where the very highest grade of product is not needed, incipient rising-steel is commercially better than strictly piping-steel. In the former the pipe is prevented or displaced by the few blow-holes in the ingot, and the expensive necessity of scrapping a quarter or perhaps a third of the ingot is thereby obviated. The few blow-holes themselves, being scattered and having bright surfaces, stick together to some extent in the rolling and hammering processes, as stated above. They do no appreciable harm, and are seldom, if ever, heard from.

On the other hand, split rods and wires and split ends of boiler-plates not infrequently result from the use of top-ends of piped ingots. The temptation to use as much of the ingot as possible

occasionally causes the line between sound and unsound steel to be overstepped.

Casting-Temperature.—The influence of casting-temperature upon steel is manifold.

As remarked above, it vitally affects the position of the zone of blow-holes in soft steel made by the evolution method.

It influences greatly the amount of segregation in ingots. The higher the casting-temperature the more segregation.

The rate of solidification of a steel ingot is influenced chiefly by the casting-temperature, and has, as noted by Mr. H. H. Campbell, a great influence on the molecular structure or crystallization of the outer layer of the ingot, which has a direct bearing on its behavior when worked by rolls or hammer. If an ingot which has been made by the solution method, and cast hot, be broken across, a sort of columnar structure, or arrangement in lines or planes normal to the cooling-surface, is seen on the fractured surface, and the hotter the steel the farther into the ingot this structure extends. Probably, the metal is weakened along these planes, and liable to draw apart, at some of them, in the rolls. Annealing the ingot breaks up this formation.

The effect, if there be any, of high casting-temperature on the chemical composition is obscure. Possibly, at high steel-temperatures the affinity of manganese for oxygen is so much weakened that the deoxidation of the metal is incomplete, and the red-shortness of the steel cast too hot may then be due, partly to oxide of iron and oxide of silicon in the steel, partly to blow-holes too near the surface, and partly to the molecular structure.

Segregation.—Mr. William Metcalf announced, in 1881, at the Philadelphia meeting of the Institute (*Trans.*, ix., 385), the discovery that, with the exception of carbon, all desirable ingredients of steel are paramagnetic, while all undesirable ones, except oxygen, are diamagnetic. Carbon alone, of the desirable elements, is diamagnetic. The behavior of the ingredients of steel, with respect to segregation, seems to present a parallel case. As far as the analyses given in connection with the papers under discussion go, they show that the desirable elements in steel, with the exception of carbon, do not segregate strongly, while the undesirable ones do. It would be very interesting to have information on the segregating action of any or all of the long list of less common ingredients of steel, or rather, one should say, the less commonly determined ingredients of steel, for some (copper and oxygen, for instance) are almost always, if

not always, present. It is to be hoped, that some of the investigators of the Institute will find opportunity to follow up the trail of some of these elements, in this regard, while the scent is still warm.

To guard against segregation in ingots, the following precautions may be observed :

1. Cast ingots of the smallest practicable size.
2. Cast ingots of as cold steel as practicable.
3. Cast ingots as slowly as practicable.
4. If the ingots must be large, and segregation is very objectionable, then, purer stock than is otherwise needed must be used, or allowance made to cut off from the top or out of the center, or both, enough steel to remove the parts containing the worst segregation.
5. The method mentioned above, of making what I have called incipient rising-steel, must, it would seem, give ingots free from segregation. The steel in the mould is constantly in motion until the moment of solidification, and the impurities have no opportunity to collect anywhere. This method being applicable to soft steel only, however, will give no assistance in overcoming segregation in large high-quality hard-steel ingots.

James E. Howard, Watertown, Mass. (communication to the President): My purpose in submitting the following observations concerning tests of steel under repeated stresses made at the Watertown Arsenal is to invite criticism and discussion of the methods pursued and proposed in these investigations. In general terms, what is attempted consists in studying the physical properties of the metal in its original state and the changes they may undergo from time to time after the metal has been overstrained, and in striving to determine, when decided change of any property has been recognized, whether that change is permanent in character, or, on the other hand, a restoration of the original quality may be effected. In short, we are endeavoring to locate definitely the dividing-line between safe and unsafe treatment or use of material.

A series of experiments on repeated stresses by transverse loads on rotating shafts was begun at the Watertown Arsenal in 1888, and has continued since that time, the tests still being in progress.

Shafts 1 inch in diameter are employed, supported at the ends, 33 inches apart, and exposed to a load either on one central bearing or on two bearings 4 inches apart. The latter method of loading is used to obtain a section at the middle of the length of the shaft, uniformly loaded, of sufficient length for subsequent examination by direct tensile test.

The material experimented upon has been steel ranging in carbon from 0.15 to 1.10 per cent. comprising hot-rolled metal, also cold-rolled steel, wrought-iron and cast-iron.

The speed of rotation has generally been maintained at 400 per minute, with some tests at higher speeds; 2200 per minute having been the maximum thus far reached. It is intended, however, to experiment with speeds up to 22,000 per minute, or higher, if found practicable.

It is not so much the purpose of this note to state specific results as it is to invite attention to the manner of carrying on the investigation. Indeed, any attempt to draw extended deductions would be at this time premature.

The total number of rotations or repetitions of stresses which the shafts will endure under different fiber-stresses before rupture is in most cases determined; but an earnest effort is being made to ascertain what phases the material passes through, prior to complete rupture.

It is desired to ascertain in what deterioration consists, as illustrated in the physical properties, and whether the progress of deterioration may be arrested and a restoration effected, and by what means—defining at what stages, if any, such restoration is possible.

We have found that a load of one-half the tensile elastic limit and less than one-third its tensile strength will in a comparatively short time, by means of repeated stresses, rupture one of these rotating shafts.

The usual effect of cold-working, such as cold-rolling and wire-drawing, is an elevation in tensile strength; and, while there is dissimilarity of treatment in these cases, the results raise the query whether metals of the classes experimented upon do not have a tendency to pass through a state of maximum cohesive resistance, and whether final rupture occurs under real or apparently diminished stresses.

Being familiar with the fact that certain kinds of cold-working introduce changes in density and internal strains, and that internal strains may reach a state of intensity or concentration nearly or quite sufficient to cause rupture in a metal of exhausted toughness; and furthermore, that alternate stresses tend to exhaust the toughness; with these facts before us we can hardly assume that it has yet been shown that the cohesive resistance between adjacent particles of steel does not go on increasing under the influence of cold-working until the moment of final rupture.

To assist in the solution of questions of this nature, fundamental in the use of metals under prolonged stresses, certain of the experimental shafts will be examined after having performed a smaller number of rotations than is required to produce complete rupture. Annular specimens will be taken from the middle of the length of the shaft, and tested by tension in the direction of the axis of the shaft. The object of employing annular specimens is, of course, to remove the metal nearest the neutral axis of the shaft, and test that part only which has been exposed to the higher stresses.

The investigation has not advanced far enough to enable us to speak with confidence upon the results obtained hitherto; but in one instance an extraordinary gain in strength, over the primitive strength of the metal, was displayed in an annular specimen from an overloaded shaft; and unless this can be explained in some other manner, it appears to indicate that the curve representing the tensile strength of the metal in question, as shown by the apparent behavior of the rotating shafts, was not a straight line from its primitive condition to that of final rupture by repeated stresses.

In the series of tests proposed, shafts will be run under different fiber-stresses, and after establishing the number of loadings necessary to produce rupture with each fiber-stress, other similar shafts will be run for a smaller number of rotations, and specimens will be taken out and tested. Some of the specimens thus taken out will be annealed before testing by tension. In this manner it is expected that valuable information will be obtained.

Most tests of the series have been carried on at ordinary atmospheric temperatures, at least the shafts have not been allowed to acquire a very high temperature. A number of shafts, however, have been allowed, under the influence of a high fiber-stress, to attain a temperature of nearly 400° F. and perhaps more. Several shafts acquired a blue heat. At this higher temperature the shafts endured from four to ten times as many rotations before rupture as the duplicate shafts run at temperatures ranging from 70° to 110° F.

This extraordinary behavior of the hot shafts will be fully investigated. At present there is no reason for believing these results to be exceptional, startling as they may appear.

In the zone of temperature between 400° and 600° F., the highest tensile strength is found; but up to this time the elastic limits show a decrease over the values found at ordinary temperatures.

The modulus of elasticity at 400° F. is doubtless lower than at 70° F., showing a greater mobility of the metal within the elastic limit at the higher temperature.

The shafts thus showing superior endurance when hot were loaded with a high fiber-stress; tests to follow will be made with lower stresses. All of the tests of this series have been made with higher fiber-stresses than would be involved in the ordinary service of the metal, and perhaps the same comparative results would not be found under different conditions of loading.

The relation which the elastic limit and the toughness displayed in a tensile test bear to the endurance of the metal under repeated alternate stresses, has not yet been made apparent in these tests. As already remarked, shafts have been ruptured with stresses of one-half the tensile elastic limit; and metal which will elongate 20 per cent. in a tensile specimen is ruptured with almost inappreciable elongation in the rotating shaft.

It may be remarked in passing, that the jog in the tensile curve often displayed soon after the elastic limit is passed, is eliminated by first over-straining the metal in compression.

Shafts which show small or no measurable permanent sets in deflection, when first loaded, often display appreciable sets after having been run a few hundred or thousand rotations. An interval of rest commonly diminishes the sets, which appear when the test is resumed; but the gain in this respect is soon lost after a few additional rotations.

When a high fiber-stress is first applied—one which would cause a very decided permanent set—the power required to rotate the experimental shaft is greater than is subsequently needed, after a few hundred rotations have been made.

That the deflection of the shaft is not affected by the speed of rotation, when the fiber-stress does not cause sensible permanent sets, may be asserted as true, within the limits of these experiments, which have embraced a speed of 2200 rotations per minute, and fiber-stresses of 50,000 pounds per square inch in combination.

When, however, the fiber-stress exceeds the elastic limit of the metal, the deflections are diminished; the shaft in the meantime rapidly acquiring a higher temperature, under a high speed of rotation.

A cast (gun-iron) bar displayed remarkable endurance while under a maximum fiber-stress of 15,000 pounds per square inch. This shaft ruptured at 47,283,500 rotations; and yet, at each rotation there was doubtless developed a measurable permanent set in the metal. Making a comparison of endurance, according to the magnitude of the permanent sets developed at each rotation, this bar equaled any steel bar experimented with.

Final rupture is commonly accompanied by a wobbling motion of the shaft, as fracture rarely penetrates the metal around the entire circumference.

After the first indications of impending rupture, the fracture is generally completed within a few hundred rotations.

J. S. Robeson, Braddock, Pa. (communication to the Secretary): In Mr. Hartshorne's paper on "The Basic Bessmer Steel Plant of the Pottstown Iron Company" (*Trans.*, xxi., 743), a description is given of certain blacksmith- or forge-tests used in those works to determine the quality of the metal made at each heat.

It was my privilege to work under Mr. Hartshorne, in various capacities, for a number of years; and I had many opportunities to study and examine these tests; in fact, for quite a long period before the accurate rapid methods for determining sulphur and phosphorus had been introduced in the laboratory, the steel was graded and placed on the various orders, according to the estimate of its contents of carbon, sulphur, phosphorus, and manganese, made under my supervision. These estimates, based on the appearance of the surface, the size, color, and location of the crystals and fiber in the fractured ends of the tests, were afterwards checked against the reported analysis from the laboratory, and it was found that very accurate results could be expected from this method of "observed analysis."

It would make a very long story to attempt to tell of all the details that were studied on a test in order to form an opinion as to its chemical contents; and it suffices to say in this particular, that the appearance of the fracture was, perhaps, the most important of all. It was very soon found, that unless the 4 by 4-inch ingots, from which these pieces were hammered, received exactly similar treatment at the hands of the blacksmith, widely varying results would be obtained, and that the appearance of the fractured pieces of one blow would be no guide as to the pieces from the next blow, though they might have an exactly similar chemical composition. By always using the same smith for this work, and by close attention to the heat-treatment in the shop, it is believed that a great degree of uniformity was obtained.

From the results thus obtained at Pottstown, it may be said that a trained observer, working on pieces of steel that have been hammered and heated in a like manner, can deduce fairly accurate figures as to the chemical composition of said pieces.

It occasionally happened, for one cause or another, that the 4-inch

ingot was not cast; and, in order that the records might be complete, it was necessary to procure these tests in another way. Under these circumstances, a shear-test would be cut. That is, when one of the large ingots had been heated and rolled, and was being sheared to the proper bloom-size, a small piece would be cut off, taken to the blacksmith shop and forged in the same way as the 4-inch test. These pieces had evidently gone through a very different experience from the usual tests. They had been cast in another manner, had been heated oftener, and had had a very much larger amount of work put upon them; still, their fracture and general appearance was identical with those produced by the standard method. In this case, certainly the amount of work had nothing to do with the appearance of the fracture.

The 4-inch square test-ingot, 10 to 15 inches long, was cast during the teeming of a 12-ton Bessemer blow; and the casting-temperature of the steel often varied very greatly from blow to blow. So far as the tests were concerned, the casting-temperature was not considered, and variations in it, however great they might be within the range that permitted of successful casting, could not always be detected by the appearance of the fracture, the subsequent heat-treatment having apparently, in some cases, removed the differences that originally existed. Though the casting-temperature affects the original structure, it seems possible to alter its effects, as seen in the fracture, by subsequent work, *i.e.*, by heat-treatment.

Nevertheless, leaving out of consideration steel castings (finished material), this temperature must not be disregarded, since its effects can plainly be seen in other directions. Blow-holes and surface-flaws are, undoubtedly, effected by changes in it, and it is possible that segregation may increase or diminish as this temperature rises and falls.

In regard to segregation, M. Pourcel notes, in his paper, that it takes place in the following order:

Carbon,
Phosphorus,
Sulphur,
Silicon,
Manganese.

But, in the example which he cites, of a Bessemer steel roll cast at La Louvière, the greatest segregation is shown by the phosphorus, then follow the sulphur, carbon, silicon, and manganese; and, in a number of examples which I have at hand, representing two grades

of steel, one containing 0.10 to 0.12 per cent., the other, 0.30 to 0.40 per cent. of carbon, the same order, namely,

Phosphorus,
Sulphur,
Carbon,
Silicon,
Manganese,

is invariably maintained.

JOSEPH HARTSHORNE, Stowe, Pa.: I had prepared some few remarks on one or two of the subjects proposed for discussion at this meeting, but Mr. Hibbard has covered my ground so thoroughly, and my agreement with him is so complete, that he has left me little to say. There are one or two points, however, that strike me as being of sufficient importance to warrant the risk of some repetition.

After struggling with the problem of segregation for more than fifteen years, I am in about the same state of mind as I was at the beginning. I do not yet know of any method of preventing it. It can be modified, and, perhaps, minimized, by casting slowly, by using small ingots, by keeping the casting-temperature as low as possible, etc.; but every now and then, in spite of all precautions, it will occur to a disastrous degree, and can never be wholly prevented. The most homogeneous metal in appearance will show signs of it, if carefully investigated. At least, I have never yet failed to find signs of this trouble, when I have looked for it carefully. I have been forced, therefore, to the conclusion that segregation cannot be prevented. In many instances it does not occur to an extent that makes much difference, and for many purposes it can be disregarded; but there is always the chance of the one time when it may take place with fatal results. The only solution yet presented of this problem, which is at all satisfactory to my mind, is that the impurities shall be kept so small in amount that segregation, if it does occur, will do no harm. In fact, I begin to fear that this will prove to be the final solution. Of course, I am speaking now of high-quality steel only.

I agree with Mr. Hibbard in thinking that the hard spots which occur in steel may often be attributed to a local segregation. The usual explanation, that is, badly mixed or unmelted recarburizers, is inapplicable in so many cases, that some other cause must be found. This one appears to me to be, at least, very plausible.

I think it might be well to repeat here, what I have before rather strenuously insisted upon, that the casting-temperature of steel has a

very decided effect upon the physical characteristics of the metal when cold, and that this effect is carried through to the finished product. Of course, I do not claim any originality for this remark. But it seems to me to be a point that is often overlooked, and it is one that has been denied.

It is well understood by all that the casting-temperature has a great deal to do with the number and position of the blow-holes in an ingot. If the heat be too hot, the blow-holes will be very numerous and very close to the surface. This means that, when the ingot is heated, the outer crust will scale off in the furnace, and these holes will open to the atmosphere. Their surfaces will then oxidize, and whatever chance they may have had of welding-up will disappear entirely. Thence come cracks and laminations in the steel, and a greater tendency to tear apart in the ingot when being rolled. It is largely for this latter reason that hot heats appear to be more red-short than those that have been cast at a lower temperature.

Besides this physical effect of a high casting-temperature, there are other effects less thoroughly understood, and yet even more harmful. There is no doubt that steel which has been cast at too high a temperature will show definite characteristics, not produced by the blow-holes. These characteristics must be produced either by the temperature itself, or by something which accompanies or causes the excess of temperature.

For instance, steel which has been cast at too high a heat will be much more brittle than other steel of the same composition (according to the analyses usually made), which has been cast at the proper temperature. The test-pieces made from such steel will show the same characteristics in fracture and appearance as those made from steel which has been over-heated in the furnace, or finished at too high a heat. It may be that this is the effect of some molecular change produced by the high temperature itself. I know of no direct evidence to support this supposition; and the indirect evidence is against it. If the effect were due to molecular changes, we should expect it to be modified or eliminated by the next heat-treatment. But this does not happen to any great extent when the subsequent heat-treatment is normal, although the existing condition may be intensified by subsequent over-heating.

Again, it often happens that open-hearth steel is cast at a temperature which would be entirely too high for Bessemer steel of the same composition. The same thing happens when Bessemer steel is blown from high-manganese pig-iron, or has undergone special

treatment, to raise the temperature, during the last minute or two of the blow.

All these facts, and others of a like character, point to the notion that there is some difference of a chemical nature, not disclosed by the analyses as ordinarily made; that is, those including carbon, silicon, sulphur, phosphorus, manganese, chromium, etc. The generally accepted theory is, that this difference is in the amount of oxygen contained in the steel. The preponderance of evidence is certainly in favor of this view. The question is, however, hardly settled as yet; and it still remains to be shown why a hot heat should have more oxygen in it than a cold one. It remains also to be discovered whether this oxygen is present as a gas or as an oxide, and how it can possibly exist together with large amounts of manganese.

Of course, it is easy to suppose that a hot heat may hold more gas in solution than a cold one can; but then this excess of gas should be expelled as the steel cools in the ladle. This does not appear to take place; at least, I have never been able to satisfy myself that any particular advantage, in this respect, is to be gained by allowing a heat, which has been finished too hot, to cool in the ladle. There is, of course, great gain as regards the blow-holes.

It would seem, therefore, that the most probable state in which oxygen can be present is that of oxide, which, for some reason, has escaped the action of the deoxidizers. In some cases a hot heat has been made so by burning the iron itself, intentionally or otherwise. Here we have an instance in which a hot heat may be supposed to be an over-oxidized one, due to an easily traced cause. Generally, also, a hot heat has been, at the same time, a longer one than usual. This means that the iron has been exposed to the action of the blast for a longer time than in the cooler heats. In the basic Bessemer process, in which most of my observations have been made for the last ten years, a hot heat is always a long one, in my experience, as compared with those of the proper temperature made at the same time. The high temperature in this case is almost always due to excess of phosphorus. Since phosphorus is not a complete protector to the iron, the longer the blow, and especially the longer the after-blow, the more iron will necessarily be oxidized.

It is probable, moreover, that manganese will not act on the oxide so readily at a high temperature as at a lower one; and the presence in the steel of oxygen and comparatively large amounts of manganese at the same time, may be accounted for in this way.

In this connection, note may be made of a fact which, to say the least, does not help to explain matters. This is, that the same proportionate amount of deoxidizers will render a very much overblown heat (say, half blown away) fit to roll, as would answer for the full heat, provided that the deoxidizers are introduced free from the influence of the slag. This seems to indicate that blown metal will only hold a certain amount of oxygen or oxide, and all above that amount will pass into the slag.

However this all may be, there is no doubt that something happens in a hot heat which has a very decided influence upon the physical characteristics of the solid metal—an influence which continues to manifest itself to a greater or less extent throughout all the subsequent manipulations.

Each heat of steel at Pottstown was tested both chemically and physically, as described in my paper, read at the Reading meeting (*Trans.*, xxi., 757). This gave us a very good idea of its nature and characteristics. We thoroughly assured ourselves that a heat which had been cast at too high a temperature gave a more brittle steel than did others of the same composition which had been cast at the proper temperature. When I say "same composition," I must be understood to mean the same as far as our analyses carried us. We determined carbon, sulphur, phosphorus, and manganese; and we also had assured ourselves that there was no appreciable amount of chromium, arsenic, copper, or any other rarer elements in the steel.

We sometimes made comparative tests from the 4-inch ingot, made at the time of casting, and from the blooms of the same heat when rolled. These tests almost invariably showed the same characteristics, and were practically alike in their indications. This proves that the effect of the casting-temperature was carried at least as far as the bloom. It also shows that work, beyond a certain amount, does not have much, if any, effect upon the metal as rolled. In the case of the 4 inch ingot the reduction to the test-piece is only ten times, while in the other case, from the 17-inch by 21-inch ingot, the reduction is two hundred and thirty times. Only enough work to make the steel solid is necessary. That object once attained by any means, further work does not improve the quality of the metal.

We also had evidence which seemed to show that the influence of the casting-temperature is carried through to the finished product. As regards this, I am not willing to make so positive an assertion;

although there is no doubt in my own mind as to the truth of the fact. I am still in doubt, however, as to the amount of this influence; that is, whether it is carried through to the full original extent or not.

In speaking of casting-temperature and its effect on the physical effects of the cold metal, I confine myself, for the present at least, to the Bessemer process, both acid and basic, as ordinarily carried out; and I mean the temperature at which the blow is finished, and the metal is ready to be poured from the vessel, not the temperature at which it is cast into the moulds. Cooling-off in the ladle has little, if any, effect, that I have been able to discover, except on the blow-holes or piping.

PRESIDENT HOWE: This question, why high temperature causes its undoubted injurious effects, is an important one. Is it high temperature as such, or some concomitant of high temperature that is the real cause? If the latter, may we avoid the concomitant, and thus avoid the evil effect, while preserving the convenience which high temperature offers when we are to cast a charge of steel in many small pieces?

Many have referred the injury which accompanies high temperature to its inducing the absorption of nitrogen; and some evidence has been gathered in support of this view. On this point evidence should be easy to get, and is much needed.

WM. R. WEBSTER, Philadelphia, Pa. (communication to the Secretary): The suggested lines for this discussion, prepared by our President, are very complete; and we are under obligations to him for the pains he has taken in this matter.*

The experiments of Mr. Howe on the heat-treatment of steel are of the greatest value, and throw much light on the changes which occur in the structure of the metal. I trust that such experiments

* NOTE BY THE SECRETARY.—The following extract from Circular No. 3, of 1893, is here given for preservation, and in the hope that it may serve to incite and to guide further discussion of the subject:

A.—*The Physics of Steel.*

(See papers of Messrs. Martens, Osmond, Pourcel, Sauveur, Hadfield, Howe and Webster, in vols. xxii. and xxiii.).

The following lines of discussion are suggested, but are not in any way exclusive:

I. Correspondence between chemical composition and fracture, micro-structure and physical properties.

II. Influence of—

will be continued on a larger scale, in connection with the work of rolling or forging at different temperatures. A test-report (Table I.) received from Mr. S. M. Vauclain, Superintendent of the Baldwin Locomotive Works, gives an interesting illustration under this head. This report gives the result of tests made upon two $8\frac{1}{2}$-inch steel blooms, furnished to the works to be forged into axles. They had been bought on a specification calling for a certain tensile strength and elongation in a test-specimen cut from the blooms and drawn down under the hammer to 1 inch square. Some large driving-axles, forged from such blooms, having failed to meet the physical requirements of the purchaser—although the test-specimens had met the requirements of the specification, and although such failure had never been experienced before, even with forgings from $6\frac{1}{2}$-inch blooms—it was suggested that the cause of the failure might be that the particular blooms from which the axle had been forged had received little or no work in the mill; in other words, had been rolled from small ingots instead of large ones. In order to ascertain the effect of extra working, two sample blooms were forged from $8\frac{1}{2}$ down to 7, 6, 5 and 4 inches successively, and test-specimens were taken at each stage. The results obtained from these specimens are given in the table.

---

(1) Casting-temperature.
(2) Manner and temperature of heating.
  (*a*) For rolling.
  (*b*) For annealing.
(3) Work.
(4) Finishing-temperature.
(5) Rate and mode of cooling.
  (*a*) After forging.
  (*b*) For casting.

} on {

(*a*) Fracture.
(*b*) Micro-structure.
(*c*) Physical properties.
(*d*) Tensile properties.
(*e*) Residual stress.

III. Segregation as affected by—
  (1) Composition.
  (2) Casting-temperature.
  (3) Rate of cooling.

IV. Blow-holes and pipes; their volume and position as affected by—
  (1) Composition.
  (2) Casting-temperature.
  (3) Casting-pressure.
  (4) Rate of cooling.
  (5) Special additions.
  (6) Forging.

V. Hardening: relation between tensile properties and hardness of quenched steel, and—
  (1) Quenching-temperature.
  (2) Quenching-medium.
  (3) Size of piece quenched.

It was found, as the table indicates, that the successive forgings materially enhanced the value of the steel. The elongation increased as the size was diminished by forging. It will be noticed, however, that in Bloom I. there was a slight decrease, and in Bloom II. a considerable increase, of tensile strength. This phenomenon is attributed by Mr. Vauclain to the circumstance that Bloom II. had been allowed to retain its original size at the bottom of the ingot.

On the result of this experiment, Messrs. Burnham, Williams & Co., of the Baldwin Works, changed their specification so as to require that the test-piece should be cut from the bloom furnished.

The small elongation and crystalline fractures of the larger sizes covered by this report show clearly, I think, the trouble caused by improper heat-treatment, while the greatly superior results in the smaller sizes indicate the true value of the material. By annealing the larger pieces, the stretch and fracture might be made, no doubt, equal to those of the smaller pieces in their present state, but it is only by careful experiment that the best methods of working can be established.

Another point of great interest is the difference in the determinations of carbon, manganese, phosphorus and sulphur made on the same pieces of steel by the chemists of our leading steel-works. Table II., for which I am likewise indebted to Mr. Vauclain, gives the results of six comparative tests in this respect. Mr. Vauclain writes concerning it:

"In order to obtain a correct idea of the composition of the material, it was decided to send a sample of each make of steel enumerated in the table to each of the different makers. We cut a piece of Otis steel into five parts, sending one part to Otis, one to Park, one to Wellman, one to Carnegie and one to Lukens. We did the same with a piece of Park steel, and so on through the list, adding a sixth piece purchased from parties to whom it was impossible to send for an analysis. You will see in the table that there is not a single case in which the analysis of the same piece by any two of the makers' chemists agree. Some of them vary widely. It is impossible to believe that these differences are due entirely to the carelessness of the chemists employed by these different companies. I am of the opinion that they are more largely due to irregularity in the material, and that they demonstrate how unreliable a single analysis made from a steel plate may be. The letters A, B, C, D, E and F, in the first column of the table, are those which we stamped upon the samples, so that we might be able to distinguish them without letting it be known who the manufacturers were."

The greatest differences shown in the table are:

	Per cent.		Per cent.	Plate.
Carbon, . . .	0.17	to 0.23	a difference of 0.06	in E.
Manganese, . .	0.33	" 0.42	" " 0.09	" A.
Phosphorus, . .	0.041	" 0.055	" " 0.014	" A.
Sulphur, . . .	0.019	" 0.043	" " 0.024	" C.

TABLE I.—*Tests made at the Baldwin Locomotive Works, February 3, 1894, from Two 8½ in. Blooms, Drawn Down to Different Sizes.*

(Length of test-specimen in all cases, 2 inches.)

Mark.	No. of bloom.	Size of bloom.	Section of Test-Specimen.			Broke at Pounds.	Strain per square inch in Pounds.	Per cent. of Elongation in Length of Section.	Fracture.
			Reduced Diameter.	Diameter.	Area.				
		In.	In.	In.	In.				
1A	I.	8½	.800	.875	.601	53,740	89,417	13.	Crystalline.
1B	I.	8½	.790	.875	.601	54,200	90,183	14.	Crystalline.
2C	I.	7	.770	.875	.601	53,600	89,184	16.5	Partly crystalline.
2D	I.	7	.760	.875	.601	53,520	89,051	16.5	Partly crystalline.
3E	I.	6	.750	.875	.601	52,240	86,921	18.	Coarse, cup.
3F	I.	6	.750	.875	.601	51,800	86,189	18.	Coarse, cup.
4G	I.	5	.750	.875	.601	52,650	87,603	18.	Silky, cup.
4H	I.	5	.730	.875	.601	54,090	90,000	20.	Silky, cup.
5I	I.	4	.720	.875	.601	52,760	87,787	22.	Silky, cup.
5J	I.	4	.740	.875	.601	53,180	88,485	21.	Silky, cup.
1A	II.	8½	.800	.875	.601	51,750	86,106	13.	
1B	II.	8½	.800	.875	.601	52,250	86,938	16.	
2C	II.	7	.780	.875	.601	54,950	91,430	16.	
2D	II.	7	.780	.875	.601	56,450	93,926	16.	
3E	II.	6	.790	.875	.601	59,300	98,688	17.5	
3F	II.	6	.770	.875	.601	58,600	97,504	17.5	
4G	II.	5	.750	.875	.601	58,650	97,587	18.5	
4H	II.	5	.750	.875	.601	58,900	98,003	19.	
5I	II.	4	.740	.875	.601	57,100	95,008	20.	
5J	II.	4	.740	.875	.601	56,950	94,758	20.	

They show, I think, that there is great need of improvement either in the system of taking drillings or in the methods of analysis.

TABLE II.—*Comparative Analyses of Various Steels.*

	Analyzed in Laboratory of.	Carbon.	Manganese.	Phosphorus.	Sulphur.	Silicon.
A	Otis	.16	.42	.048	.037	
	Park	.15	.34	.041	.036	
	Wellman	.15	.35	.049	.048	
	Carnegie	.16	.38	.055	.028	.018
	Lukens	.14	.33	.042	.044	
	Average	.152	.364	.041	.0385	
B	Otis	.17	.39	.024	.024	
	Park	.20	.35	.024	.026	
	Wellman	.20	.36	.031	.027	
	Carnegie	.22	.39	.028	.016	.020
	Lukens	.20	.36	.022	.036	
(Firebox.)	Average	.198	.362	.0258	.0258	
C	Otis	.16	.76	.035	.025	
	Park	.16	.71	.033	.028	
	Wellman	.16	.74	.032	.029	
	Carnegie	.17	.80	.043	.019	.060
	Lukens	.145	.80	.032	.043	
	Average	.159	.762	.035	.0288	
D	Otis	.16	.50	.012	.020	
	Park	.16	.44	.012	.016	
	Wellman	.15	.48	.017	.021	
	Carnegie	.15	.49	.010	.014	
	Lukens	.14	.47	.010	.029	
(Firebox.)	Average	.152	.476	.0122	.020	
E	Otis	.17	.60	.021	.045	
	Park	.23	.65	.019	.055	
	Wellman	.19	.67	.025	.055	
	Carnegie	.23	.68	.026	.047	.014
	Lukens	.21	.61	.019	.067	
	Average	.206	.642	.022	.0538	
F	Otis	.16	.36	.049	.059	
	Park	.20	.30	.050	.069	
	Wellman	.17	.31	.057	.067	
	Carnegie	.19	.34	.058	.059	.015
	Lukens	.185	.32	.050	.070	
	Average	.181	.326	.0528	.0648	

Another matter, to which the attention of chemists has been attracted of late, is the importance of an element not usually determined by them, namely, oxygen. Mr. A. Lantz, of the works at Peine, Germany, writes me as follows:

"Here we determine carbon, manganese, and phosphorus for each blow, and occasionally oxygen; almost entirely neglecting sulphur, on the other hand, because it never appears in any troublesome way.

"We have found during the current year (1893) that oxygen plays a part hitherto but little noticed, yet so important that, given a like tenor of carbon, phosphorus, and manganese in the blows, a blow containing a larger proportion of oxygen gives greater hardness and less ductility. This fact, which we have firmly established here, is so important to the technical world that I would strongly commend it to your consideration; the variations in oxygen bring differences of several kilogrammes per square millimeter, other conditions being similar.

"We employ at present essentially the method introduced by Prof. A. Ledebur for the determination of oxygen in iron, neglecting, however, one feature of it, namely, the heating of the drillings in a stream of nitrogen. The drillings must not be too coarse, on which account the larger pieces are sieved off. The method is very detailed, but it is, in my opinion, the best now known, and gives good results."

The method of Prof. Ledebur, to which Mr. Lantz refers, was described at length in *Stahl und Eisen* for May, 1882, by the distinguished Freiberg professor himself, under the title, "Determination of Oxygen in Malleable Iron." I am not aware that this description has ever been published in English translation, but I suppose our chemists must be more or less familiar with a contribution made to their science so long ago upon such high authority. With the aid of an excellent translation, made for my personal use by Mr. F. E. Thompson, of Pottstown, Pa., I give some extracts, together with a condensed outline of the method.

Under "malleable iron," Prof. Ledebur includes wrought-iron and ingot-iron or mild steel (Flusseisen). In the former he says that oxygen exists in combination as the proto-peroxide of iron, mechanically mixed with the metallic mass; in the latter, as the protoxide of iron, dissolved or, so to say, alloyed in the mass, like cuprous oxide in copper. In a foot-note he observes:

"The somewhat widespread assumption that the proto-peroxide of iron can exist in iron that is or has been fluid (and hence in ingot-iron generally) seems to me to be unwarranted, in view of the great excess of metallic iron which would reduce the oxide. Equally erroneous appears the opinion, sometimes expressed, that 'the oxides' (the protoxide) in ingot-iron exist only in mechanical mixture, like that of the slag in wrought-iron."

After pointing out that the oxide in ingot-iron, though usually less in proportion than that which is found in wrought-iron, exerts, by

reason of its more intimate combination, a greater effect on the quality of the metal, and that its determination is therefore scarcely less important than that of sulphur, phosphorus, etc., he proceeds to describe the special difficulties of the problem, and says that the first, and not the least, is to obtain drillings free from grease and moisture. His remarks under this head afford so good an illustration of the necessity of minute precautions against error in all work of this kind, that I quote them at some length:

"Drillings free from grease can be most easily obtained with a drill-press if a clean drill is forged, tempered in water free from grease, ground blunt and put into the machine, and the drillings are collected upon a plate free from grease. But unless all this work be personally superintended with the closest vigilance, the desired result will seldom be secured; for all the objects in the shop—the tools and implements, the hands of the workmen, even the drill-press itself—are, intentionally or unintentionally, coated with grease. Drillings made with a press "free from oil," which I received for examination from a large establishment, contained notable quantities of grease.

"If, for lack of a drill-press, the file must be used, this difficulty is increased; for the file-cutters cover their files, both before the cutting and heating and after the finishing, with a heavy layer of mixed resinous and greasy substances. Filings obtained with a purchased file often give off, when heated in a test-tube, a distinct cloud of combustion-products, and, at least, a strong odor of organic matters is clearly detected. The file-cutters cannot be persuaded to abandon the use of such substances. I caused a purchased file to be heated to glowing, dulled and recut, with express directions that no dressing should be given to it; nevertheless, the filings which I took with this file yielded the usual greasy odor. Then a large file-works kindly offered to have special files prepared for my purpose, every organic substance to be excluded during the manufacture. These files arrived, and were preserved in a specially-prepared desiccator over sulphuric acid; but when used, they showed that, even in this case, the workmen had secretly yielded to temptation, and indulged their habit of providing the files with a dressing.

"In such cases there is nothing else to do than to treat the files with ether or benzine, and then dry them at a gentle heat. The greatest part of the dressing is thus removed, and, upon evaporation of the solvent, remains at the bottom of the vessel as *corpus delicti*. Yet even filings taken with a file thus treated usually retain a trace of organic matter, which I have not succeeded in removing entirely, even by repeated treatment of the filings with ether or benzine and alcohol.

"*May not the large nitrogen-content, sometimes found by earlier investigators in various kinds of iron, be due to a neglect of these considerations?*"

I have italicized the final paragraph, because, although only a modest suggestion, it seems to me to have almost the force of a demonstration. The suggestion that nitrogen in steel has some important influence upon its quality, has been repeatedly advanced in the past, and still recurs from time to time.

Prof. Ledebur proceeds to discuss with equal thoroughness the question of moisture in the drillings, showing how easily moisture is

absorbed, and with what difficulty it is completely removed. Even at 121° C., he says, neither water nor the benzine used in cleaning from grease will entirely disappear. He recommends, therefore, the final treating of the drillings to glowing, in a stream of pure, dry nitrogen. This is the part of his method which Mr. Lantz says is omitted at Peine, being deemed, doubtless, a refinement unnecessary in general practice.

The drillings being thus prepared, the oxygen is determined by heating them to glowing in a stream of hydrogen gas, and collecting the water thus formed in a weighed absorption-tube. The result is checked by comparing the loss of weight in the drillings with the gain in the absorption-tube. Omitting the details of this operation, I give in Table III. the summarized results of various analyses reported by Prof. Ledebur, which show the range of the percentage of oxygen in various kinds of iron.

TABLE III.—*Determination of Oxygen by Prof. A. Ledebur.*

	I.	II.	III.	IV.	V.	VI.
Oxygen, per cent.....	None.	0.035	0.047	0.068	0.111	0.515

I. *Gray Pig-Iron.*—This result was foreseen, but it serves to show that the amounts of oxygen found in other irons are not due to foreign sources (scale, etc., from the files, adhering air on the filings, etc.).

II. *Open-Hearth Steel from Oberhausen.*—An average of two determinations, giving respectively 0.032 and 0.038 per cent.

III. *Ingot-Iron from Bochum.*—An average of two determinations, giving respectively 0.053 and 0.042 per cent.

IV. *Thomas Steel: Ladle-Test Before Complete Dephosphorization; from the Rhine Steel-Works.*—An average of two determinations, giving respectively 0.069 and 0.067 per cent.

V. *The Same, After Comp'ete Dephosphorization and Before the Addition of Spiegel.*—An average of two determinations, giving respectively 0.126 and and 0.096 per cent. The difference between the two determinations in this case may have been possibly due to the unequal distribution of the oxygen in the two samples. At least, the same difference in loss of weight was found upon weighing the drillings after combustion.

VI. *Wrought-Iron from Gutehoffnungshütte.*—An average of three determinations, giving respectively 0.507, 0.515 and 0.524 per cent. The large proportion of oxygen in this wrought-iron, which may be surprising to many, is easily explained upon consideration of the abundance of slag which is usually mixed with all wrought-iron.

I have subdivided into three or four groups, under each element considered, the 408 tests given in my paper read at the Chicago

meeting (*Trans.* xxiii., 113). In some cases, this does not give as many tests as could be desired, from which to calculate averages. All the results, however, are shown in Tables IV., V. and VI., which exhibit, first, the number of tests in each group, with the averages of the actual ultimate strengths, and next the averages of the estimated strengths. These are deducted from the actual ultimates, and the differences are given with their proper signs. All the above results are given for each mill in Tables IV. and V., and for both mills in Table VI.

Table VI. exhibits, under the same grouping, the percentage of tests that came within ± 3000, ± 4000, and ± 5000 pounds of the estimated ultimate strengths. It has been my endeavor, in these tables of averages, to bring out clearly the effect of each element, in order to investigate it further.

An inspection will show that the estimated ultimates agree very closely with the actual ultimates. An exception to this is the group of actual ultimates of over 70,000 pounds, which indicates that the table of estimated ultimates is not as accurate for high steels as for low; although, after making corrections for size of plates, the results are materially improved. But, in order to settle this matter, the tests were grouped by their *calculated ultimates* and the results then show that the table is just as accurate for high steel as low.

The practical application at the works of the Pottstown Iron Company, of my table of estimated ultimate strengths in the manufacture of steel, has given very satisfactory results. Out of the last one thousand blows graded in this way, 98 per cent. met the requirements in every respect, and were accepted. Having the same standard of comparison in use in all departments, gives a close check on each step in the process of manufacture, and has enabled us to improve greatly the quality and uniformity of the finished product. If a blow does not give the results indicated by its chemical composition, we follow it back through each process of manufacture; and, if the cause of the trouble is not made apparent, we have a complete analysis made of the drillings from broken test-pieces and are then generally able to locate the trouble.

When rolling heavy steel plates, trouble is often caused by finishing at too high a temperature, which gives a material with crystalline fracture, poor reduction and poor bends. In order to guard against this, and control the finishing-temperature, we use very light drafts in rolling, and produce as good results in heavy plates as in light ones.

TABLE IV.—*Summary of Averages of Universal Mill Tests.*

	Number of Tests.	Ultimate Strengths. Pounds per square inch.		
		Actual.	Calculated.	Difference.
Per cent.				
Carbon .....06 to .09.....	67	55,721	56,119	— 398
" .....10 to .12.....	82	61,250	61,516	— 266
" .....13 to .18.....	22	65,298	65,749	— 451
Sulphur.....Under .04.....	27	57,480	57,292	+ 188
" .....04 to .059.....	90	58,819	59,554	— 735
" .....06 to .079.....	54	61,976	61,927	+ 49
Phosphorus.....Under .04.....	57	55,468	56,092	— 624
" .....04 to .069.....	82	60,019	60,589	— 570
" .....07 to .099.....	32	65,911	65,164	+ 747
Manganese.....Under .40.....	57	56,938	56,955	— 17
" .....40 to .549.....	78	59,973	60,531	— 558
" .....55 to .75	36	63,028	63,415	— 387
Pounds.				
Actual ultimates.....46,000 to 54,000.....	30	51,647	53,561	— 1914
" .....54,000 to 62,000.....	89	58,415	59,607	— 1192
" .....62,000 to 70,000.....	42	64,509	62,663	+ 1846
" .....Over 70,000	10	73,461	70,703	+ 2758
Estimated ultimates....46,000 to 54,000.....	21	52,241	52,389	— 148
" ....54,000 to 62,000.....	95	58,356	58,602	— 246
" ...62,000 to 70,000.....	51	63,717	64,452	— 735
" ...Over 70,000	4	75,450	74,062	+ 1388
Estimated ultimates, corrected for size...46,000 to 54,000.....	29	52,277	51,778	+ 499
Estimated ultimates, corrected for size...54,000 to 62,000.....	101	59,165	58,520	+ 645
Estimated ultimates, corrected for size...62,000 to 70,000.....	36	64,615	64,407	+ 208
Estimated ultimates, corrected for size...Over 70,000	5	74,916	73,910	+ 1006
Inch.				
Thickness.....Under $\frac{3}{8}$.....	7	65,530	61,921	+ 3609
" ..... $\frac{3}{8}$ to $\frac{7}{16}$.....	48	61,961	61,558	+ 403
" ..... $\frac{1}{2}$ to $\frac{9}{16}$.....	54	58,733	59,516	— 783
" ..... $\frac{5}{8}$ to 1	62	57,869	58,848	— 979
Thickness, corrected for size..Under $\frac{3}{8}$.....	7	65,530	64,921	+ 609
" " " ..$\frac{3}{8}$ to $\frac{7}{16}$.....	48	61,961	61,350	+ 611
" " " ..$\frac{1}{2}$ to $\frac{9}{16}$.....	54	58,733	58,484	+ 249
" " " ..$\frac{5}{8}$ to 1	62	57,869	57,086	+ 783
All tests.....	171	59,604	59,946	— 342
All tests corrected for size.....	171	59,604	59,045	+ 559

TABLE V.—*Summary of Averages of Sheared Mill Tests.*

	Number of Tests.	Ultimate Strengths. Pounds per square inch. Actual.	Calculated.	Difference.
Per cent.				
Carbon.....06 to .09.....	104	55,436	55,316	+ 120
" .....10 to .12.....	98	61,159	60,806	+ 353
" .....13 to .18.....	35	67,927	66,982	+ 945
Sulphur.....Under .04.....	53	55,869	55,748	+ 121
" .....04 to 059	94	59 302	59,024	+ 278
" .....06 to .079.....	90	62 234	61,704	+ 530
Phosphorus.....Under .04.....	94	55,510	55,758	— 248
" .....04 to .069.....	106	60,913	60 397	+ 516
" .....07 to .099.....	37	66,532	65.212	+ 1320
Manganese.....Under .40.....	84	56,074	55,728	+ 346
" .....40 to .549.....	113	61,524	60,836	+ 688
" .....55 to .75	40	61,853	62,517	— 664
Pounds.				
Actual ultimates.....46,000 to 54,000.....	48	51,605	53,110	— 1505
" .....54,000 to 62,000.....	123	58,541	58,628	— 87
" .....62,000 to 70,000.....	46	65,685	65,639	+ 46
" .....Over 70 000	20	71,863	68,809	+ 3054
Estimated ultimates...46,000 to 54,000.....	37	51,858	52,148	— 290
" ...54,000 to 62,000.....	135	58,429	58,098	+ 331
" ...62,000 to 70,000.....	56	65,911	64,900	+ 1011
" ...Over 70,000	9	70,974	72,130	— 1156
Estimated ultimates, corrected for size...46,000 to 54,000.....	38	52,016	52,049	— 33
Estimated ultimates, corrected for size...54,000 to 62,000.... .	133	58,381	58,048	+ 333
Estimated ultimates, corrected for size...62,000 to 70,000.....	55	65,586	64,898	+ 688
Estimated ultimates, corrected for size...Over 70,000	11	71,622	72,076	— 454
Inch.				
Thickness.....Under $\frac{3}{8}$.....	19	64,075	61,260	+ 2815
" .....$\frac{3}{8}$ to $\frac{7}{16}$	154	59,127	58,679	+ 448
" .....$\frac{1}{2}$ to $\frac{9}{16}$	37	58,372	58,899	— 527
" .....$\frac{5}{8}$ to 1	27	61,247	62,090	— 843
Thickness, corrected for size..Under $\frac{3}{8}$	19	64,075	64,576	— 501
" " " ..$\frac{3}{8}$ to $\frac{7}{16}$	154	59,127	58,821	+ 306
" " " ..$\frac{1}{2}$ to $\frac{9}{16}$	37	58,372	57,899	+ 473
" " " ..$\frac{5}{8}$ to 1	27	61,247	60,479	+ 768
All tests.....	237	59,648	59,309	+ 339
All tests corrected for size.....	237	59,648	59,327	+ 321

TABLE VI.—*Summary of Averages of* 408 *Tests of Both Mills.*

	Number of Tests.	Ultimate Strengths. Pounds per sq. in. Actual.	Calculated.	Difference.	Per cent. within 3000 pounds.	Per cent. within 4000 pounds.	Per cent. within 5000 pounds.	After making corrections for size of Plate.
Per cent.								
Carbon.....06 to .09.....	171	55,548	55,630	— 82	74.3	88.3	95.3	
" .....10 to .12.....	180	61,200	61,130	+ 70	70.6	82.8	94.4	
" ..... 13 to .18.....	57	66,912	66,506	+ 406	61.4	75.4	80.7	89.5 per cent. came within 5000 pounds, being a gain of 8.8 per cent.
Sulphur.....Under .04.....	80	56,413	56,268	+ 145	83.8	93.8	96.3	
" .....04 to .059.....	184	59,065	59,283	— 218	72.8	86.4	94.0	
" .....06 to .079.....	144	62,138	61,788	+ 350	61.1	75.7	89.6	93.1 per cent. came within 5000 pounds, being a gain of 3.5 per cent.
Phosphorus.....Under .04.....	151	55,494	55,884	— 390	76.9	89.5	93.4	
" .....04 to .069.....	188	60,523	60,480	+ 43	68.8	84.6	95.2	
" .....07 to .099.....	69	66,244	65,190	+ 1054	62.6	70.1	85.5	92.8 per cent. came within 5000 pounds, being a gain of 7.3 per cent.
Manganese.....Under .40.....	141	56,423	56,224	+ 199	73.0	84.4	92.9	
" .....40 to .549.....	191	60,891	60,711	+ 180	70.2	85.3	92.7	
" .....55 to .75	76	62,409	62,942	— 533	68.4	80.3	93.4	
Pounds.								
Actual ultimates.....46,000 to 54,000.....	78	51,609	53,283	— 1674	74.4	88.5	94.9	
" .....54,000 to 62,000.....	212	58,493	59,039	— 546	76.9	91.0	96.2	
" .....62,000 to 70,000.....	88	65,124	63,083	+ 2041	61.2	73.9	89.8	
" .....Over 70,000	30	72,398	69,441	+ 2957	46.7	56.7	73.3	90.0 per cent. came within 5000 pounds, being a gain of 16.7 per cent.
Estimated ultimates.....46,000 to 54,000.....	58	51,997	52,235	— 238	84 5	100.0	100.0	
" .....54,000 to 62,000.....	230	58,399	58,306	+ 93	71.7	83.0	93.5	
" .....62,000 to 70,000.....	107	64,866	64,686	+ 180	60.7	76.6	86.9	
" .....Over 70,000	13	72,352	72,725	— 373	76.9	92.3	100.0	
Estimated ultimates, corrected for size,.....46,000 to 54,000.....	67	52,129	51,931	+ 198	79.1	94.0	100.0	
Estimated ultimates, corrected for size.....54,000 to 62,000.....	234	58,719	58,252	+ 467	74.9	90.1	94.4	
Estimated ultimates, corrected for size.....62,000 to 70,000.....	91	65,202	64,703	+ 499	68.1	83.5	91.2	
Estimated ultimates, corrected for size.....Over 70,000	16	72,653	72,424	+ 229	93.8	100.0	100.0	
Inch.								
Thickness.....Under $\frac{3}{8}$.....	26	64,466	61,438	+ 3028	26.9	50.0	80.8	
" .....$\frac{3}{8}$ to $\frac{7}{16}$	202	59,800	59,363	+ 437	74.3	87.1	94.1	
" .....$\frac{1}{2}$ to $\frac{9}{16}$	91	58,586	59,265	— 679	73.6	92.3	98.9	
" .....$\frac{5}{8}$ to 1	89	58,894	59,832	— 938	73.0	78.7	87.7	
Thickness, corrected for size....Under $\frac{3}{8}$.....	26	64,466	64,669	— 203	80.8	92.3	92.3	
" " " ....$\frac{3}{8}$ to $\frac{7}{16}$	202	59,800	59,422	+ 378	73.8	89.1	94.6	
" " " ....$\frac{1}{2}$ to $\frac{9}{16}$	91	58,586	58,246	+ 340	81.3	92.3	98 9	
" " " ....$\frac{5}{8}$ to 1	89	58,894	58,116	+ 778	68.5	88.8	92.1	
All tests.....	408	59,629	59,576	+ 53	70.8	84.1	92.9	
All tests, corrected for size.....	408	59,629	59,209	+ 420	74.7	89.7	94.9	

RECENT ADVANCES IN PYROMETRY.

Discussion of the paper of Prof. Roberts-Austen. (See vol. xxiii., 407.)

(Virginia Beach Meeting, February, 1894.)

PRESIDENT H. M. HOWE, Boston, Mass. (communication to the Secretary): Le Chatelier's pyrometer is certainly a most convenient and accurate instrument for the laboratory, and one that may be used under some conditions in the mill. I have used it for several years and with great satisfaction. Like Prof. Roberts-Austen and unlike Herr Blass, I have always found that its indications remain constant over long periods.

Like Prof. Roberts-Austen, also, I find it possible, in a quiet place, to read with extreme accuracy; and, by the use of a telescope, I can read temperatures to within a degree, for some delicate determinations which I have in hand. Variations in the temperature of the room affect the readings of the instrument far less than one would expect.

When the current is cut off, the mirror does not always return exactly to the same place; in other words, the zero shifts appreciably, and must be determined often, indeed every few hours if accuracy be desired. In some cases I have had it determined within a very few minutes of every important reading. To this end a switch should be introduced, so that the circuit may be broken readily. With this arrangement we can determine accurately the position of the zero within a very few seconds of the time of taking any especially important reading.

The chief causes of the variation of the zero which I have noticed are as follows:

a. The suspension may not be tight enough, so that the momentum of the galvanometer frame as it swings makes its supporting wires slip in their bearings.

b. Changes in the position of neighboring iron objects may deflect the mirror.

c. Changes in the temperature of the room may, perhaps, change the torsion of the suspension wires, and so shift the mirror.

d. Dust may attach itself to the galvanometer frame, and by friction against the surrounding fixed parts of the galvanometer, may

influence the position to which the frame and mirror swing when the current is broken.

I think that many people who have used this instrument have paid insufficient attention to the influence of the leads. The temperature of the thermo-junction, which, of course, is that which the instrument indicates, is by no means necessarily that of the immediately surrounding objects. The leading-in wires or "leads," unless they are very fine, may conduct heat away from the thermo-junction, or to it, in very considerable amount, and grave errors may thus be caused.

Two precautions against this trouble are to use very thin wires for the leads; and to arrange matters so that the leads, for a considerable distance on either side of the thermo-junction shall be very nearly at its temperature.

To illustrate this latter point, let us take the case of determining the melting-point of small masses of metal, as for instance in calibrating the instrument itself. We are usually told that the metal, the melting-point of which is to be determined, should be wrapped about the thermo-junction, and the whole imbedded in powdered clay, sand, or other slow conductor of heat. The temperature of the whole is then raised gradually; as it passes the melting-point of the metal its rise is arrested or retarded.

Though this method gives very fair results, on reflection we see that it is radically wrong, and on trial we find that a more rational method gives incomparably sharper and better results. Surrounding the metal with a powdered substance which is a slow conductor implies that the isotherms shall be near together, or, in other words, that neighboring parts of the surrounding mass shall differ considerably in temperature. As this surrounding mass of clay or sand embraces the leads closely, even close to the thermo-junction, this in turn implies that the temperature even of those parts of the leads which are near the thermo-junction, may differ considerably from that of the thermo-junction itself: or, in other words, that the leads may conduct heat seriously to or from the thermo-junction, the temperature of which will thus differ from that of the metal under observation.

If, however, we wrap some sheet asbestos about the thermo-junction and the enclosing metal, we surround the whole with a small air-chamber, the temperature in this chamber will be nearly uniform, the asbestos will touch and affect the leads but little, and they will be thus at almost the same temperature as the thermo-junction. Then we may surround this asbestos envelope with our clay or sand,

simply so as to have a mass which changes but slowly in temperature.

We may divide pyrometers into two classes, those in which some object must be brought to the high temperature which is to be measured, and those in which this is not necessary. Two of the important groups of methods which belong to the first of these classes—the calorimetric method, in which a ball of platinum, iron or copper is brought to this temperature, then plunged in a known volume of water, the rise of temperature of which indicates the previous temperature of the ball, and the method of noting which of a series of alloys or of clay cones melt or soften—have the disadvantage that, to bring the ball, alloy or cone to the temperature which is to be determined, requires a length of time that is usually inconvenient and often cannot possibly be provided. In the usual thermo-electric and electric-resistance pyrometers, like Le Chatelier's and Siemens', this difficulty is avoided, because in them we have only to bring a fine wire to this high temperature. But in many cases even this is difficult or impossible—as, for instance, when we seek the temperature of a bar or sheet in the rolls.

But even where this is practicable, these methods have other inconveniences. Of what can we make our wire? Of course, wires of iron or of copper would be destroyed at high temperatures. Platinum, to which we naturally turn, itself is rapidly destroyed at very high temperatures, by contact with another metal, or with carbonaceous or with siliceous substances. Contact with brickwork is fatal, and the spattering of fine specks of hot metal and the deposition of ash or of carbon from the flame may well be. I have never had any difficulty from this source at a yellow heat or at any lower temperature; but once the temperature reaches whiteness, contact with brickwork destroys platinum with a rapidity which increases markedly with further rise of temperature. We can, indeed, protect our platinum by lime, and probably by either magnesia or calcined dolomite. Were this done, the wires should be made thin, lest their conductivity, which is very high relatively to that of the lime covering, prevent the thermo-junction from reaching the temperature which is to be determined.

Another and very serious trouble with thermo-electric pyrometers is that we have at present no galvanometer which is at once adapted to their feeble currents and suited to industrial uses. In order to measure the very slight currents which the thermo-electric couple sets up, we use reflecting galvanometers. With these we do not

measure directly the slight deflection of the galvanometer-needle, but the enormously greater travel of a beam of light reflected, from a mirror attached to this needle, upon a scale at a considerable distance. The jarring caused by machinery or by passing trains or wagons may make the galvanometer-mirror tremble so that we read only with difficulty the movements of the ray of light which it reflects. Where the jarring is not immoderate, careful adjustment of the suspension has enabled me to read with fair accuracy even with Deprez-D'Arsonval galvanometers of the common form. Jarring as severe as that caused by heavy steam-hammers might, indeed, prevent our reading with such instruments satisfactorily. Further, the difficulty which attends re-calibrating and adjusting these instruments is an extremely serious obstacle to their use in industrial establishments.

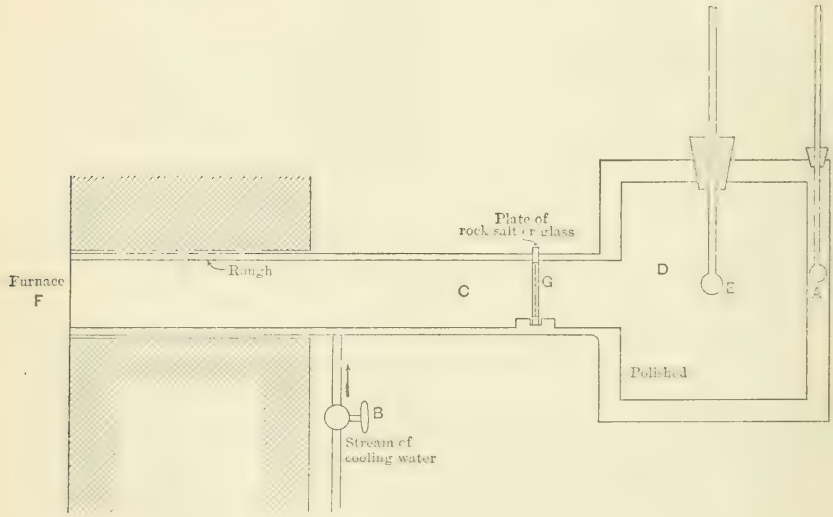
Thus, if the thermo-electric method is to be used widely in factories, we need urgently some more simple and portable galvanometer. Aperiodic or "dead-beat" ammeters are now made which are simple and portable, and which might be used for this purpose if they were made more delicate, so that the slight current which our thermo-electric couple sets up should produce a large deflection of their needle.

They indicate the current, and they would indicate the temperature, not by the deflection of a beam of light, but by the movement of an indicating pointer or hand like that of a pressure-gauge.

Such an arrangement would have the advantage that its indications could be made autographic by pencil or pen, instead of by the photographic method, which, in spite of Prof. Roberts-Austen's great ingenuity and skill, is certainly far less convenient. Indeed, I understand that since the paper under discussion was written, he has brought out a galvanometer for this purpose which gives an autographic pen-record.

Let us now turn to the second class of pyrometers, those which do not require that any substance be heated to the temperature under examination. It seems to me that this very fact of not having to heat anything to the high temperature in order to measure it should give this class of methods easy and wide application. In this second class are the pyrometers which measure the heat or light radiated from the hot space or object. Passing by the photometric group, which includes an optical pyrometer of Le Chatelier which Roberts-Austen describes, it should be practicable to devise means of measuring the radiated heat. We can do this by allowing the heat to

strike against a thermo-electric junction, though here again we have, in case of jarring from steam-hammers or other sources, to contend with the trembling of the galvanometer mirror, or by allowing it to strike against a thermometer bulb. In this case the temperature of the furnace or hot object can be read directly on the thermometer scale, which, of course, must first be calibrated by some direct method. The chief thing to bear in mind in designing such an instrument is, that the thermal conditions surrounding the thermo-electric couple or thermometer bulb, as the case may be, must be constant, so that its indications may be due only to the heat radiated to it by the hot object, and may not be influenced by variations in the transfer of heat between itself and other objects. To that end it may be enclosed in a water-jacketed case on all sides except that nearest the hot object.



Suggested Design for a Pyrometer.

I give here a sketch of a first approximation in the design of such a pyrometer for determining the temperature of large spaces, such as the interior of an annealing or heating furnace, regenerator, etc.

Within the water-jacketed box, D, stands the high-temperature thermometer or its equivalent, E. On the bulb of this the heat radiated from the interior of the furnace strikes after passing through the water-jacketed tube, C, and through the diathermic plate, G. As the temperature of the furnace rises and falls, so the amount of heat which would be radiated from the furnace upon G would increase

or diminish, and so would the quantity of heat which would strike on the bulb, E.

In order that no variable other than the quantity of heat radiated from the furnace upon E shall influence the temperature of the latter, we must guard against convection currents, and we must make the temperature of the walls of the tube, C, and of the vessel, D, constant. This we can readily do by water-jacketing them as I have shown, and by keeping their temperature constant by means of a stream of cooling water regulated by the cock, B, and the thermometer, A. The convection currents we should prevent by means of a diathermic plate, G. The ideal substance of which to make this would be rock salt, on account of its high diathermancy. But it may not be practicable to get and maintain suitable pieces of salt. For most cases a very thin sheet of glass would probably do, though mica, smoky quartz, and several other minerals suggest themselves.

Instead of the thermometer bulb, E, we may use its equivalent in some modification of the bolometer of Prof. S. P. Langley; or, for that matter, a simple thermo-junction. A modification of the bolometer would seem to be particularly appropriate for such a purpose. At first it seems better that the walls of D should be polished and those of C, rough; but further consideration may show that this is not right.

The nearer the temperature of E comes to that of the furnace the smaller, probably, will be the error. Hence the importance of making G very diathermic. To the same end it might be well that G should be in the form of a lens. It should be arranged for ready removal, cleaning and replacement, as I have sketched.

This apparatus would give relative results, but its indications may be calibrated so as to give absolute temperatures, by once for all determining the temperature of the furnace independently by some other pyrometer, such as Le Chatelier, a calorimetric one, etc.

Such an instrument should be now more easy of construction than formerly, because we now have mercurial thermometers that read to relatively high temperatures. Both Queen, and Hohmann & Maurer (Plymouth and Jay Streets, Brooklyn, N. Y.), make mercurial thermometers reading to relatively high temperatures, even as high as 750° F. (400° C.). We are promised a mercurial thermometer which will read to 550° C. (1022° F.).

The objections which might occur, that the diathermic plate, G, might transmit a different proportion of the radiated heat for different temperatures of the furnace, and that its diathermancy would

vary with its own temperature, on reflection we see are not valid, because, for the same temperature of the furnace, F, the transmission of heat would be the same. These variables would be met, and, therefore unconsciously allowed for in the calibration itself.

THE TAYLOR GAS-PRODUCER AND THE COALS OF ILLINOIS AND MONTANA.

Continued Discussion of Mr. Stetefeldt's paper. (See vol. xxiii., pp. 134 and 585.)

(Bridgeport Meeting, October, 1894.)

W. H. Blauvelt, Great Falls, Montana (communication to the Secretary): In his remarks on Mr. Stetefeldt's paper (*Trans.*, xxiii., 587), Mr. Goetz observes:

"Trouble experienced with producers at Great Falls influenced me to try Taylor producers instead of those of Wellman. But recent reports show that the Wellman producers work better, besides costing nearly 50 per cent. less than Taylor producers of equal capacity. Indiana and Illinois coals cannot be used in the Taylor producers by reason of the large size and hardness of the clinkers which they form."

I think that Mr. Goetz must have been misinformed regarding the work of this producer in Illinois. One of the large manufacturing concerns in that State, the Illinois Zinc Company, situated at Peru, which uses a coal mined in the vicinity, tried one Taylor producer, some four years ago, in competition with those of an older type, which it had been using. As a result of this trial, the company has now thirteen Taylor producers, which have been in operation for about three years. A recent report from these works says that the Taylor producers have largely reduced the loss of coal through the grate, and have also improved the quality of the gas, as compared with that produced from the same coal in the producers of the older type.

This Illinois coal is, perhaps, the worst in the country for making gas; since, besides containing from 20 to 40 per cent. of ash, it carries a large quantity of pyrites, so that the clinkers are large and extremely hard, and the capacity of the producer is most severely tested.

A water-jacket, of a design similar to those that have been so successful in gasifying the ashy clinkering coals of Montana, was used for a time on one of these producers; but, by reason of a prolonged illness of the superintendent occurring just at that time, no conclusive results were reached, although, from experience obtained elsewhere, it is probable that the use of the water-jacket would have been decidedly advantageous.

For the easier removal of the clinkers from the walls, this company has introduced in the brick-work, at the point where the clinkering was worst, a number of openings, which have been found very helpful.

At the plant in Great Falls, Montana, to which Mr. Goetz refers, recent improvements have brought the work of the Taylor producers up to a quite satisfactory condition. Greater familiarity with the apparatus has enabled the operators to overcome the difficulties formerly encountered; and the inferior and smaller grades of fuel are now used without trouble, yielding a uniform supply of gas of good quality.

Now that these Taylor producers at Great Falls are properly run, they show the same superiority over the other type represented in that plant, as do those in Illinois. The gas is of better quality and the coal-loss through the grate is much smaller. Indeed, in the case of this plant, the loss is almost *nil*.

These Taylor producers at Great Falls are nearly all water-jacketed, and the plant shows conclusively the advantage of this device where the ash clinkers seriously, as there are still a sufficient number of producers lined with brick to permit a careful comparison.

When the clinkers found are very large and hard, the revolving bottom is not as useful as it is when the better coals of the East are available; and it would probably be advisable, in some cases, to build the producer with a stationary bottom, retaining the valuable features of the central air-supply and deep ash-bed. This modification would have, moreover, the advantage of materially lessening the cost of the Taylor producer.

THE LIMITATIONS OF THE GOLD STAMP-MILL.

Continued Discussion of the Paper of Mr. T. A. Rickard. (See vol. xxiii., 137 and 545.)

(Bridgeport Meeting, October, 1894.)

NOTE BY THE SECRETARY.—In the preceding communication of Mr. Rickard, in the present discussion, as printed in *Trans.*, xxiii., the loss of quicksilver at Pestarena, reported on p. 569, as 230 and 234 grammes per metric ton, is stated as equivalent to about 7½ pounds per short ton avoirdupois—an obvious error, which escaped correction in my reading of the proofs, and was not discovered until after Vol. XXIII. had left the press. The true equivalent is 0.46 pound or about 7½ *ounces* (instead of *pounds*) per short ton.—R. W. R.

PHILIP ARGALL, Denver, Colo. (communication to the Secretary): Mr. Rickard's last contribution to this discussion* calls for some reply from me, if only by way of defending myself against the charge (p. 569) that my criticisms of his paper were "unnecessarily hypercritical and occasionally unfair." At the same time I am fully aware that our *Transactions* are not the place for mere personal controversy, and that the sole object of discussion is to define differences of opinion, and defend opposing views as a means of eliciting the truth.

Mr. Rickard disclaims the statement, which he says I attributed to him, that in the slow-crushing Gilpin county mills the pyrites remain longer in the mortar than the other portions of the ore. My understanding of his statement was based on the following passages in his paper:

"Particles of ore, which have been pulverized to a fineness which would permit of their exit through the screen, are enabled to settle towards the bottom of the mortar. It would be expected that the heavy metallic minerals occurring in the ore would, because of their greater specific gravity, be most affected by this feature of the treatment. In practice this is found to be so. The fine slimes contain a large proportion of metallic sulphides, generally valuable on account of their close association with the precious metals, while the coarsest particles to be found in the tailings usually consist of quartz and other minerals forming the less heavy gangue (*Trans.*, xxiii., 139).

* *Trans.*, xxiii., 561.

"The water discharged from a stamp-mill often transports the heavy pyrite further than the light quartz. This is due to the fact, already referred to, that the pyrite remains inside the mortar longer than the quartz and becomes pulverized to a further degree of fineness" (*Trans.*, xxiii., 142).

Mr. Rickard's statement that all the gold of the ordinary Gilpin county ore is "fine," is unfortunately vague. "Coarse" and "fine" are comparative terms; and while this gold is fine as compared with that of Bendigo, for instance, it consists of particles of unequal size, and may fairly be divided, as I divided it, into coarse and fine, comprising gold scarcely visible under a magnifying power of 200 diameters, gold visible to the unaided eye, and gold so coarse that Mr. Rickard was able to see it to be coated with iron oxide. If he did not intend to make a comparison between the coarse and the fine gold of these ores, the comparison is still practicable and pertinent.

Mr. Rickard thinks that, in my criticism, I have missed the main principle of Gilpin county milling, which, he says, originated in no idea of concentrating. I think he is wrong, but the matter is not important. However this particular system originated, and whether the introduction of concentration as a part of it is recent or not, we are confronted with the fact that concentration is now an important step in the milling-practice, as I have previously pointed out. The long drop, the roomy mortar, the fine crushing and the battery amalgamation do not, on the average, save one-half the value of gold and silver in the ore. The question is, Can any improvement be made on the present system, inherited as it has been from ante-smelting times? In my opinion, yes. The whole system is wrong, viewed from the standpoint of actual present conditions.

Fine crushing is not conducive to close saving by concentration, and it is therefore suggested that coarse crushing and concentration, to remove the metallic minerals at the least expense, should precede fine crushing and amalgamation.

The very conditions indicated by Mr. Rickard as having tended to perpetuate a milling-practice in many respects out of date, are the most forcible arguments in favor of the suggested method of treating these ores. The very reasonable freight-rates on concentrates, the low smelting-charge on these products, and the cheap fuel for milling, are just the conditions that would render concentration commercially successful. The Gilpin miners, however, cling to their fathers' idols so tenaciously that they cannot, or will not, see this fact. The times have changed, but they have not changed with the times.

Mr. Rickard said in his original paper:

"Water is the fluid used, but air also plays its part. During the time of its violent agitation under the falling stamp, the water entangles a certain amount of air. Such air exists in the form of small bubbles, which hold the finely-pulverized ore in suspension, and thus become the main agent in the floating of the slimes. Warmth causes the air to expand and the bubbles to become dissipated; therefore, any rise in the temperature of the water, such, even, as is caused by the impact between the stamps and the ore upon the die, is favorable to a diminution in the amount of slime" (*Trans.*, xxiii., 142).

In his last communication (*Trans.*, xxiii., 571), he gives the results of a test made at the Gover mill, Amador Co., Cal., showing differences in the temperature of the water before and after leaving the batteries, which range from 1° to 3.5°, and average 2.25°. This increment of temperature is far too small to help the theory above quoted, for the expansion of a bubble of air, under constant pressure, due to a rise in temperature of 2.25° F., is only 0.0045 of its volume.

But the figures of the test given do not bear examination. They are apparently vitiated by some extraneous cause, if not by errors of observation. For example:

	Deg.		Deg.		Deg.
With the air at	40,	water at	37.67,	the gain was	2.66.
" " "	48,	"	42.00,	" "	2.83.
" " "	50,	"	43.12,	" "	1.50.

That is to say, the hotter the air, relative to the water entering the batteries, the less heat the water receives, though the work developed in the batteries is in each case the same.

But this warming of the water cannot have been due to the conversion of wasted energy into heat, as Mr. Rickard suggests. The stamps in the Gover mill weigh 800 pounds and drop 6¾ inches ninety-six times a minute; and the water-consumption is 3.25 gallon per stamp per minute.* From which it follows that the energy developed per minute by a battery of five stamps cannot exceed 216,000 foot-pounds; whereas, the energy required to raise the corresponding quantity of water ($5 \times 3.25 = 16.25$ gallons) 2.25° F. in temperature would be 235,406 foot-pounds. This would require the creation of over 19,000 foot-pounds of energy more than the battery develops. The case is still worse if the observations showing the greatest rise in temperature be taken. One test gave a rise of 3.5°, and no less than three (out of ten) showed 3°. The latter figure would require 313,875 foot-pounds, or nearly 3 horse-power more

* *Eng. and Min. Jour.*, December 23, 1893.

than the falling stamps can develop; in other words, an apparent excess of 45 per cent. over the total energy of the battery. Since it will be conceded that some energy is utilized in crushing ore, it will scarcely be claimed that the so-called "wasted energy" amounts to 145 per cent. of the original total.

DOES THE VIBRATION OF STAMP-STEMS CHANGE THEIR MOLECULAR STRUCTURE?

A continuation of the Discussion arising in connection with Mr. Rickard's paper on "The Limitations of the Gold Stamp-Mill." (See vol. xxiii., pp. 143, 557, 560, 573, 574.)

(Bridgeport Meeting, October, 1894.)

Philip Argall, Denver Col. (communication to the Secretary): Dr. Raymond claims (*Trans.*, xxiii., 560), that my statement (p. 557) regarding the crystallization of iron "is beyond question incorrect." He says: "It is not even settled that vibration will crystallize iron under any conditions." While admitting that authorities differ on the possibility of cold crystallization of iron, I believe it is a settled and undisputed fact that vibration in the presence of heat will crystallize iron. Bloxam and Huntington say:* "Vibration assists in converting fibrous metal (iron) back into a crystalline state when heated, a lower temperature sufficing than in the absence of vibration." Mr. A. F. Hill,† summing up a very exhaustive review upon the crystallization of iron and steel, pronounces against crystallization, but only at temperatures below 900° F. Mr. Howe,‡ discussing the breakage of a 20-foot porter-bar with a crystalline fracture, suggests that it was attributable to heat, jointly with the jar. We see, then, that vibration of heated iron bars will induce crystallization and consequent change of structure.

Referring to my statement, I wish to point out, that I was discussing the vibration of stamps and should have said, "vibration under all *such* conditions," etc. That is, vibration attended with sharp blows, such as a stamp-stem is subjected to, will crystallize iron; yet I believe that intense vibration under any condition will

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* *Metals, Their Properties and Treatment*, London; new edition, 1888, p. 158.

† "Crystallization of Iron and Steel," *Proc. Soc. of Arts, Mass. Inst. of Tech.*, 1882–1883; quoted by Lanza, *Applied Mechanics*, 5th ed., 1891.

‡ *Metallurgy of Steel*, p. 199.

eventually crystallize iron. It is only a function of time, a very long time, perhaps, when the vibration is unaccompanied with shocks or blows.

I am not aware that any law of modern physics, or of the molecular theory of matter, would be violated by the assumed rearrangements of molecules in a viscous solid, such as iron, at ordinary temperatures. We must admit that the molecules are in motion, and that any force capable of increasing the amplitude of their vibrations may induce a change of structure. What is electrolysis but the direction, by means of an electric current, of the movement of molecules in the electrolyte to form new bodies? We have seen that vibration of hot iron bars induces crystallization at temperatures far below plasticity; so that it can be understood, that at ordinary temperatures, where the molecular motion is comparatively slow, the vibration must either be intense or long continued, in order to cause crystallization. Dr. Barus* has shown that "the chemical equilibrium of a solid changes with each change of strain." Osmond† claims that strain more or less completely converts cold metals from one definite molecular condition to another. Warburg and Ewing† have proved that hysteresis is not only associated with mechanical stress but may also be induced by magnetic stress; while the experiments of Rowland and Bell† prove that magnetic iron is more electronegative than unmagnetic iron. From which Barus infers that this phenomenon is to be interpreted as directly evidencing "a chemical difference between magnetic and unmagnetic iron."

It has also been proven by Dr. Barus‡ that, "even at temperatures slightly above mean atmospheric, the molecular configuration of glass-hard steel is always in a state of incipient change." He says:

"During the last three years I have been making experiments on the secular annealing of cold hard steel. The results are very striking, and show that mean atmospheric temperature acting on freshly quenched steel for a period of years produces a diminution of hardness nearly equal to that of 100° C. acting for a period of hours. I examined some twenty rods, the specific resistance of which, within thirty-seven months, has fallen from 46.5 to 42.5 in the case of thin rods (diameter < 0.08 c.m.) and from 43.7 to 35.4 in the case of thicker rods (diameter 0.13 c.m.)."

It is true that these quotations do not support a crystallization-theory for iron, but they do prove that the molecular structure of iron can and does change under different physical conditions and at atmospheric temperatures. This conceded, the possibility of cold

* *Bull.* No. 94, *U. S. Geol. Sur.*, p. 125 *et seq.*

† Quoted by Barus, *op. cit.*

‡ *Bull.* No. 73, *U. S. Geol. Sur.*, p. 94.

crystallization of iron becomes apparent, the "current fable" and the "myth" to the contrary notwithstanding.

Apart from the abstract theory, however, we have practical experience on the one hand and two sets of theorists on the other, from which conclusions may be drawn.

The cold crystallization of iron is a subject that has long been agitated and never satisfactorily settled. It is true, some eminent authorities have at various times settled the matter to their own satisfaction, but good, fibrous iron will continue to break with a crystalline structure, in stamp-stems, for instance; and practical men cannot accept the dogmatic assertions of the theorist as evidence against their own observation and daily experience.

In 1866, Kirkaldy* settled the matter to his satisfaction, and published his sixty-six conclusions on iron. Of these, No. 18 bears more directly on crystallization. Said Kirkaldy:

"Iron, when fractured suddenly, presents invariably a crystalline appearance; when fractured slowly, its appearance is invariably fibrous."

The crystalline appearance here referred to is very different from that of iron crystallized by the vibration and sharp shocks to which a stamp-stem finally succumbs. In the first case the fibers are not given time to stretch, but are broken off at right angles to their longer axis, whence the apparent fine crystallization; while, in the latter case, actual crystals are developed in the iron, some reaching as much as 0.25 inches in diameter. It is with this latter phenomenon we have to deal in stamp-mills.

Fairbairn† has said: "We know that in some cases wrought-iron, subjected to continuous vibration, assumes a crystalline structure." Greenwood‡ tells us that "continued hammering of iron in the cold state induces hardness and brittleness, with a more or less crystalline structure in the iron."

It must be admitted that stamp-stems invariably break with such a structure. Again, it is generally admitted that railway-axles are, on the whole, inclined to break with a crystalline fracture. The fracture of the 5-inch connecting-bar of the Washington Navy Yard testing-machine was considered by Beardslee "an unmistakable instance of crystallization."

Rankine§ sums up his conclusions as follows: "It is certain, at all

* *Experiments on Wrought-Iron and Steel.*

† Quoted by Howe, *op. cit.*, p. 198.

‡ *Metallurgy of Iron and Steel*, p. 105.

§ *Civil Engineering*, 17th ed., p. 506.

events, that iron ought to be as little as possible exposed to sharp blows and rattling vibrations."

R. W. Raymond, New York City: The subject of Mr. Argall's reply to my former remarks is so important as to justify the most extended discussion. And I would not deny that, as a part of such discussion, the citation of authorities is pertinent and valuable, although the mere heaping up of contradictory statements and opinions is not likely to determine the truth, without such further sifting and weighing of the evidence as will show what may be taken as thus far reasonably proved.

I may be permitted to recall that the remark of Mr. Argall originally criticized by me was, that "vibration, under all conditions, will crystallize iron."* This I declared to be "beyond question, incorrect," adding, that "it is not even settled that vibration will crystallize iron under *any* conditions."† In the same connection, I observed that Mr. Rickard and Mr. Argall had "adopted a current fable, which may or may not have a basis in occasional and exceptional experience, but which owes its vitality chiefly to its availability as an excuse to shield manufacturers from the blame deserved for bad work."

Mr. Austin‡ presented, in opposition to my view, some considerations to which I have already replied. I wish to recall here only the fact that I disclaimed any narrow sense of the term "crystallize," and accepted, as the subject really under discussion, the question, whether there is really a molecular change produced by vibration in iron or steel.

Mr. Argall now restates his original proposition so as to confine it to such vibration as takes place in the stem of a stamp. I accept this modification also, although I may be permitted to point out that it greatly narrows the field of inquiry originally suggested by him, and excludes most of his evidence and reasoning, as well as some of mine. It is now quite possible for him to say, that an experiment, in which prolonged vibration had no effect upon a piece of iron, is not conclusive, because the vibration was not like that of a stamp-stem in a battery. But I conceive, on the other hand, that it devolves upon him to show the difference, or else to abandon the argument from theory and analogy, and to confine himself to experimental proofs drawn from stamp-stems exclusively.

* *Trans.*, xxiii., 557.

† *Ibid.*, p. 560.

‡ *Ibid.*, p. 573.

But Mr. Argall cannot be permitted to state this question as one with regard to which practical men are arrayed on his side and theorists on the other. That is a ludicrous reversal of the situation. The wildest theorists are notoriously so-called "practical" men, when they once let themselves loose in the field of speculation; and in this case the so-called "practical" men are the only ones who have set up any theory at all. The fracture of a stamp-stem is a fact; the notion that it is due to a molecular change caused by vibration is a theory. Nobody denies that this is conceivably a true theory; but a good many observers have been led to doubt it, because there is not a single clear experimental proof of it, and because attempts to prove it by careful experiment have proved the contrary, so far as they have had any definite result whatever. It is quite out of place for the defenders of the theory to characterize as dogmatic theorists those who have no theory at all, but are simply asking for the facts.

Dr. John Percy said in 1864:*

"Another point remains to be considered, namely, whether vibration caused by impact, or otherwise, may induce a crystalline arrangement which did not previously exist, or was only imperfectly developed. I have not met with any evidence to justify an answer in the affirmative."

This is not abstract theory but practical common sense, as is also the observation which follows:

"Neglect in observing the essential connection between the character of the fracture and the particular mode in which it has been effected, has led to the conclusion that the crystallization of iron has originated from mechanical treatment, when, in reality, crystalline structure pre-existed, and was only rendered easily manifest by fracture consequent on induced brittleness."†

This declaration of Percy's represents correctly, I think, the conclusion to which any competent observer, critically examining the evidence accessible in 1864, would have been led. That the situation has not been changed in favor of the vibration-theory by any subsequent evidence, will appear in the following passage, translated from the hand-book of Prof. A. Ledebur,‡ who may fairly be called the highest, as well as the latest, authority on iron and steel:

* "Metallurgy," *Iron and Steel*, p. 12.

† Dr. Percy cites, in this connection, Kirkaldy's book, and says: "The author appears to have arrived at nearly the same conclusions as myself."

‡ *Handbuch der Eisenhüttenkunde*, part iii., "Malleable Iron," Leipzig, 1894, p. 689.

"The observation has been often reported, particularly in former times, that iron exposed to continuous shocks, as in railway-axles, crane-chains, etc., suffered a loss of strength and toughness as the result of a change in its structure; especially, that fibrous tough iron was in this way gradually altered to granular brittle iron, and that in this process could be found the cause of the occurrence of fracture in pieces which had served their purpose for decades without breaking. According to this view, continued shocks (*anhaltende Erschütterungen*) would produce the same effect as . . . . heating wrought-iron nearly to melting-point and gradually cooling it.

"This supposed observation received a seeming confirmation from the law announced by Wöhler, in 1870,* as the result of thorough experiments, according to which the fracture of a body might be brought about by numerous repeated strains, no one of which reached the breaking-limit. Later experiments† have shown that fracture is not induced, even by an unlimited number of strains, if a certain limit of strain is not exceeded.

"But the opinion that a fracture caused by repeated strains is the result of a change in structure, and particularly that fibrous wrought-iron is transformed into granular under continuous shocks, has proved to be entirely erroneous (*vollständig irrig*). On page 642 the circumstance has been pointed out that the appearance of the fracture of fibrous iron is dependent upon the manner of the breaking, and that fibrous iron shows a complete coarsely-granular fracture when suddenly broken by a heavy blow. Such a shock is usually the cause of the fractures in which it has been believed that the transformation of fibrous into granular iron was observed.

"Bauschinger, in 1878, took links from a chain-bridge erected in 1829 at Hamburg, and subjected in service to continual shocks (*Erschütterungen*), and compared them with similar links of the same material, which had remained in stock unused. (Here follows a table of tests.) The fracture of the broken pieces showed no change due to use; the pieces which had been in service showed to a large extent fibrous structure.‡

"The wrought-iron bolts of a wooden railway-bridge on the Allgau road, having been tested for strength before use, showed, when tested again by Bauschinger after twenty-five years, no diminution in strength. (Here follow the figures.)

"Again, a comparative test made by Belelubsky, in 1888, between the links of the Kiew chain-bridge, which had been forty years in service, and the links of the same material, which had remained in stock, indicated no change in these properties. All the test-pieces showed fibrous structure.§

"Numerous experiments instituted by Bauschinger in the mechanico-technical laboratory of the *Technische Hochschule*, at Munich, in which bars of iron and steel were submitted to repeated shocks, led him to the conclusion that 'Strains of iron and steel repeated frequently, millions of times, bring about no change of structure.'"

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* *Zeitschr. f. Bauwesen*, vol. xx., p. 83.

† Spangenberg in the same journal, vol. xxv., and Bauschinger, in *Mitth. aus dem mech-tech. Lab. in München*, Heft 13.

‡ The original, *Die benutzten Glieder zeigten zum grossen Theil sehniges Gefüge*, is ambiguous. It may mean that many of the pieces showed fibrous structure, or that all of them showed a predominantly fibrous structure. But it is clear that they were not characteristically granular, and the preceding statement that there was no recognizable difference between them and the unused pieces is unequivocal.—R. W. R.

§ *Stahl u. Eisen*, 1889, p. 917.

The word *Erschütterungen*, which I have translated "shocks" in the foregoing extract, is a stronger term than *Schwingungen* (vibrations). It includes both shock and vibration, and exactly represents Mr. Argall's definition, "vibration attended with sharp blows." Prof. Ledebur's conclusions are, therefore, directly contradictory of Mr. Argall's view.

Mr. Argall replies to my statement that "it is not even settled that vibration will crystallize iron under any conditions," by asserting his belief that "it is a settled and undisputed fact that vibration in the presence of heat will crystallize iron." He is, of course, aware that I was speaking of cold metal only; and I do not care to be drawn away from the issue which he has himself taken pains to confine to the conditions obtaining in stamp-mills. Nevertheless, I may venture to say that the authorities he cites do not warrant his sweeping conclusion: "We see, then, that vibration of heated bars will induce crystallization and consequent change of structure." The really "settled and undisputed fact" is that heat-treatment alone, without any vibration, will produce the change referred to, as Prof. Ledebur, in the passage I have quoted, incidentally points out. All that has been suggested as to vibration is that it facilitates the work of heat. The essential agent must still be the heat, for the simple reason that heat alone will do the work, whereas vibration alone, so far as we can find out experimentally, will not. At the very best, Mr. Argall's assertion that *heat and vibration* will produce a certain result, is no contradiction of my assertion that it is not settled that *vibration* will do it.

Moreover, there is no argument to be drawn by analogy from the behavior of iron under special heat-treatment to its behavior at ordinary temperature and after complete solidification. The very term vibration means different things in the two cases. Shock is differently transmitted in a heated bar, and structure is unquestionably under incipient obliteration. Mr. Argall's statement, "We have seen that vibration of hot iron bars induces crystallization at temperatures far below plasticity," is not warranted, even in its vagueness. The only temperature he specifies is 900° F. (482° C), which is not "far below plasticity." This temperature is, in fact, almost exactly the point at which iron exhibits a remarkable and sudden change in physical qualities, indicative of a weakening of structure. The tensile strength and elongation, as determined by Kollman* for the temperatures below and near this point are as follows:

* Cited by Ledebur, p. 691.

Deg. C.	Tensile strength kilo. per sq. mm.	Elongation per cent.
310,	33.5	33.0
340,	32.1	35.0
410,	27.0	45.0
510,	11.1	37.0

It will be seen that, between 410 and 510 degrees, the strength has diminished more than one-half, and the elongation has passed its maximum. In a word, what I suppose Mr. Argall means by "plasticity," has clearly set in.

Mr. Argall says that he is not aware that any law of physics or of the molecular theory of matter would be violated by the assumed rearrangement of molecules in a viscous solid, such as iron, at ordinary temperatures. For my part, I am not aware of such a fact either. If there were any known law thus violated, the assumption would, of course, have to give way to the law. But the arguments and citations by which Mr. Argall seeks (as I suppose, for I cannot conceive on what other ground he considers them pertinent) to render his assumption theoretically probable, fall far short of that effect; while the effect itself, if achieved, would amount to nothing. For Mr. Argall and his molecular argument are confronted by practical men, not theorists; and when he says, "Why should not this be possible?" they may reply, "We don't know; all we know is, that the thing, possible or not, has not been proved to occur." Mr. Argall seems to dislike my term "fable," and Mr. Howe's term "myth," as applied to his theory. If he prefers Professor Ledebur's term, "entirely erroneous," he is welcome to that. I beg to observe, however, that a fable, as I understand the term, is not necessarily an impossible, but simply an untrue story—in this case, a product of the "scientific imagination."

But what does the theoretical argument of Mr. Argall really prove?

1. Barus, he says, has shown that "the chemical equilibrium of a solid changes with each change of strain." Dr. Barus, in the paragraph quoted, refers not to vibration at all, but to steady pressure; and not to molecular or physical equilibrium, but to chemical equilibrium. And his conception of chemical equilibrium is measured simply by electrical resistance. Moreover, his experimental basis was a non-fibrous solid, namely, glass.

2. Mr. Argall's second quotation from Barus asserts a change in the molecular configuration of cold glass-hard steel, produced by time alone. This change is in hardness—not necessarily in struc-

ture. But here, also, it will be observed, we have a non-fibrous material, and one which is already under intense internal strains. All that Dr. Barus has proved is, that these strains, existing between molecule and molecule, readjust themselves at ordinary temperatures, in the course of years, almost as much as they would do at 100° C. in the course of a few hours. There is no tangible necessity here for a change in "molecular configuration" even; but, besides that, I do not understand that Dr. Barus means by "molecular configuration" molecular arrangement, or what we call structure.

3. Mr. Argall quotes from Dr. Barus the statement that Osmond claims, "that strain more or less completely converts cold metals from one definite molecular condition into another." I may be permitted to quote, in addition, the remark of Dr. Barus, which immediately follows:

"I have been unable to find, however, that Osmond has any direct evidence to support this assertion, and I have already pointed out some of the difficulties which Osmond must surmount before his view can gain general credence."

This shows pretty plainly where Dr. Barus stands on the question here under discussion. It is evident that he does not draw from his own experiments on glass and glass-hard steel inferences favorable to Mr. Argall's theory of stamp-stem crystallization. But M. Osmond is an observer of recognized acuteness and authority; if he has really asserted the general proposition, apparently attributed to him by Dr. Barus, his assertion of it has weight, whether Dr. Barus agrees with it or not; and that weight bears unquestionably in favor of Mr. Argall's theory, though the term "strain" may or may not designate the particular kind of strain to which Mr. Argall ascribes a particular kind of molecular change.

I have, therefore, examined with care the statement of M. Osmond; and I find that he speaks exclusively of the two varieties of iron (*alpha* and *beta* iron) which he considers as two "molecular conditions"—not two different arrangements of the molecules—and of which he says:*

"The *alpha* variety (malleable) predominates in steels slowly cooled from red heat, and the more exclusively, as these metals approach more nearly pure iron.

"The *beta* variety (hard and brittle) is formed:

"*a.* Artificially, by the action of any mechanical pressure applied below very dark red heat and producing permanent deformation.

"*b.* Spontaneously, at a certain critical temperature not yet determined."

* *Annales des Mines, Huitième Série*, vol. xiv., 1888, p. 6.

Clearly enough M. Osmond is announcing no general law, but explaining (upon his own *alpha-beta* theory, not by any means universally accepted as yet) the familiar effects of cold-rolling and hammering upon iron and steel. He carefully excludes strains which do not produce permanent deformation, and thus implicitly contradicts Mr. Argall's hypothesis.

4. The researches upon "hysteresis," concerning which Mr. Argall quotes Dr. Barus's reference to Warburg, Ewing, Rowland and Bell, are too abstruse and too little pertinent to repay special analysis and discussion here. If they proved anything for his purpose, they would prove too much. The kind of molecular change which these writers call "hysteresis" is something which they can produce by magnetism as well as by mechanical force; it is evidenced by electrical resistance wholly or chiefly; it is not shown or asserted or believed to produce a granular structure out of a fibrous one; and it is only called a molecular change because, on the molecular theory of matter, the molecules must be somehow concerned in it. Pure and simple, it is a change in electrical resistance, which is inferred to involve a change in "chemical equilibrium," which is again inferred to be a change in molecular condition.

I can easily understand Mr. Argall's frank admission that his citations "do not support a crystallization-theory for iron;" but I will leave others to decide whether they prove "that the molecular structure of iron can change and does change under different physical conditions and at atmospheric temperatures."

With regard to Mr. Argall's question, "What is electrolysis, but the direction, by means of an electric current, of the movement of molecules in the electrolyte, to form new bodies?" I beg to say that I do not pretend to know exactly what electrolysis is, but I strongly suspect that, whatever it is, it is not *that*. I cannot conceive, however, the remotest connection between this question and the one under discussion; and will therefore abstain from introducing a purely outside and wholly theoretical issue.

But a little investigation of Mr. Argall's theory itself may not be out of place. It is, if I comprehend it:

A. That the iron of new stamp-stems has a fibrous structure.

B. That this structure is changed during use, by the effect of repeated blows and vibrations, which cause the molecules previously arranged in fibers to separate and rearrange themselves in crystals.

C. That the result of this process is shown by the granular fracture when the stamp-stem breaks.

It seems to me that any stamp-stem thus fractured in service would break at the beginning, rather than the end, of such a process. The molecules can not be expected to rearrange themselves without separating; and how they are to retain cohesion when they have once separated, so as to resist the breaking-effect of shock until they have got comfortably crystallized, is not clear. The beginnings of separation are incipient fracture; and the experiments of Wöhler and others, cited above, show that shocks producing such slight separation of particles may, by repetition, go on increasing the fracture thus begun, so that, at last, the piece breaks by the dissolution of its original, not of a secondary, structure. This conception involves no molecular theory whatever. It rests on the established fact that iron is made up of joined and cemented particles, which can be pulled apart; and that, when they are sufficiently pulled apart, the iron breaks. Such a conception explains all the phenomena thus far adduced, and it is scarcely necessary to set up an auxiliary and imaginary theory that the particles first separate, then reunite, and then break apart again, under strains which tended to fracture all the time.

The whole question of the fibrous structure of wrought-iron and its supposed relation to strength, has received much new light within recent years, especially in connection with the attempt at Avesta to produce fibrous soft steel in the Little-Bessemer process, by casting some slag with the steel. The peculiar lamination caused in puddled iron by the presence of intermingled cinder was thus reproduced in steel for the benefit of prejudiced consumers; but it was not shown that this structure gave increased strength. However, I will not now pursue that part of the subject.

Let us now examine the testimony of practical experience, adduced by Mr. Argall "apart from abstract theory."

The opinion quoted from Commander L. A. Beardslee, U. S. N., that the fracture of the 5-inch connecting-bar of the Washington Navy Yard testing-machine was "an unmistakable instance of crystallization," might be construed as an assertion that this crystallization was unmistakably due to repeated shocks. Since the statement quoted is part of the report of a committee of which Commander Beardslee was chairman, and was apparently concurred in by the other members, namely, Gen. Q. A. Gillmore, A. L. Holley, William Sooy Smith and David Smith (all experts of recognized ability), the precise language employed is worthy of careful consideration. It will be found in the *Report of the United States*

Board for Testing Iron and Steel, Part I., Washington, 1878, pp. 181, 182:

"The question as to whether crystallization can be produced in iron by stress, or by repetition of stress with alternations of rest, or by vibration, has been very much discussed, and very opposite views are entertained by experts; therefore it was considered that any data which might be gathered during our tests, bearing upon this point, would possess a value.

"We have met with but one unmistakable instance of crystallization which was probably produced by alternations of severe stress, recoils and rest.

"The connecting-rod of the chain-prover was 5 inches in diameter, had been in use for forty years, and had, during this period, been frequently subjected to stress up to 250,000 pounds, with recoils produced by rupture of test-pieces.

"It was carefully made in the anchor-shop, being hammered from the best quality of wrought-iron scrap; it is not probable that any section of it, if broken when first made, would have displayed crystalline structure, but while we were testing, it parted one day at less than 200,000 pounds stress, and the surface of the fractured ends showed well-defined crystallizations, the facets being large and bright as mica; the ends having become injured by rust, the bar was again broken by impact, at a point distant over a foot from the first fracture, and the same appearance was found, which is shown in the illustration, Plate V., Fig. 1, the original of which is now in the cabinet of the Stevens Institute."

The illustration here mentioned is a heliotype, reproducing a direct photograph of full natural size; and, while I have not had the opportunity to examine the actual piece said to be at the Stevens Institute, I feel sure that the appearance of the fresh fracture is better shown in the illustration than it could possibly be shown by the piece itself after the lapse of sixteen years. At the same time, the broken piece might still yield, under proper microscopic and other examination, some important further information, although, as I shall point out, its pedigree is not good enough to justify precise conclusions.

The photographic illustration plainly shows, I think, the laminated structure due to forging. Whatever crystallization there is, is clearly subordinate to that general structure, and therefore may have existed always, as it existed at the time of fracture, together with the lamination.

The statement of the committee is, that this is "an unmistakable instance of crystallization;" but the opinion as to its cause is much more cautiously stated as merely "probable." And the degree of this probability is carefully indicated by a statement of all the data upon which the committee's opinion is based. The facts personally known to the committee, or verifiable by it beyond reasonable doubt, are, that the piece had been in service for forty years; that it had

been frequently under stress up to 250,000 pounds; and that it broke under less than 200,000 pounds. A fact presumably less certainly established, is that it was carefully made, about 1838, by hammering from the best wrought-iron scrap. The committee infers that "it is not probable that any section of it, if broken when first made, would have displayed crystalline structure." And this is the only reason for supposing that such a structure has been since induced.

In weighing the force of this conclusion, it must be remembered, first, that wrought-iron has a crystalline structure to begin with, and that this structure can be made clearly visible by cold fracture produced in a certain way; so that, in fact, what the committee means is, that it is not probable that the piece of iron in question, if broken by continued increasing tension, when it was first made, would have failed to show the fibrous fracture due to the elongation of the crystals under such tension. Such an elongation in mass implies that the adhesions of the individual grains in mass is sufficient to resist, for a time, their separation in mass. That a sudden shock or strain might produce separation with little or no elongation is to be expected according to familiar mechanical principles.

Again, the illustration given by the committee represents a fracture *under impact*, which would have been likely to be crystalline in any event. But, considering the character of the observers, we may safely accept their assurance that this fracture presented the same appearance as that produced by tension. The committee's statement, then, is substantially that, after forty years of service, a piece of iron, broken by tensile strain smaller than that which it had previously endured without breaking, showed a tension-fracture exactly like its impact-fracture, whereas, if broken when first made, the tension-fracture would *probably* have been more fibrous.

Even this *probably* is open to somewhat damaging inquiry. For the committee does not say, and evidently does not know, what heat-treatment this piece of iron received when it was forged forty years before, or whether, during these forty years, it was ever heated, straightened, annealed, or otherwise subjected to heat-treatment. Yet such treatment, as is well-known, might induce a crystalline structure both coarser and less firmly cemented than would have existed without it. It is to this unquestionable fact that Mr. Howe refers,* when he says, in discussing the present case, and also that of the 20 foot porter-bar at the Morgan Iron Works, cited by Mr. Argall:

* *Metallurgy of Steel*, p. 199.

"Now I find nothing here which indicates strongly that any change in crystallization occurs under vibration or shock. The cases of the Washington testing-machine and of the Morgan Iron Works porter-bar may well be due to over-heating under manufacture."

We have, then, as equally "probable," the hypothesis that the crystalline structure, ultimately exhibited upon fracture, had existed in the iron ever since its last heat-treatment;* and the only remaining question is, Why should the iron break under a smaller strain than it had previously sustained without breaking?

The answer to this question is given by Wöhler's experiments, and may be summed up in popular phraseology by the statement that repeated stresses, no one of which is originally sufficient to produce fracture in mass, may, when they individually surpass the limit of elasticity of the weakest elements of the mass, gradually loosen (not transform) the existing structure, and thus, by their cumulative effect, ultimately produce visible mass-rupture. This is a fact; and it offers a sufficient explanation of all the facts thus far observed with scientific precision.

The theory which it suggests may be, either that the loosening of structure is gradual and uniform, so that, at a given moment during the process, the cohesion of all the granular or crystalline elements under strain has been equally diminished; or that it is progressive, like the breaking of a wire-cable, wire by wire, so that the final visible mass-fracture is simply the cumulative result of incipient fractures, or minute separations of structural units, which have left fewer and fewer coherent units to endure strain. To my mind, the appearance of all tension-fractures, indicating, as it does, that the strain upon the mass is not equally sustained by all parts of the section of fracture (*i.e.*, that some parts elongate more than others before breaking), favors the second of these theories, which is, moreover, made plausible by what we now know concerning the unequal internal strains produced (especially by heat-treatment) in manufacture. But it is not necessary to maintain either theory. The true explanation of the phenomenon may involve them both; and neither the phenomenon nor its theoretical explanation involves any process of re-crystallization under shock at ordinary temperatures.

Under careful analysis, therefore, the instance presented by the

* It may be added, that what little is said concerning the method of manufacture favors this suggestion. Making a bar by welding with the hammer, "out of the best wrought-iron scrap," is a process peculiarly liable to produce, by local overheating, the capacity of granular fracture.

U. S. Board (which is, in my judgment, the strongest that Mr. Argall has adduced) amounts only to a guarded opinion, based upon an incomplete statement of facts, which permits a different explanation.

The declarations of Fairbairn and Greenwood, quoted by Mr. Argall, are simply reiterations of the traditional belief, unsupported by fresh experiment. Like many similar passages in the text-books, they have merely the force of the earlier opinions of which they are echoes.

Rankin's statement that "iron ought to be as little as possible exposed to sharp blows and rattling vibrations," is not only consistent with the theory of breakage without "crystallization," but immediately follows the intimation of Rankin's doubt of the earlier theory, and a report of experiments made by him on railway-axles, which do not confirm the notion of crystallization by vibration.

The only question here at issue is, Does the vibration to which stamp-stems are subjected in practice change the structure of the iron of which they are composed? It is not, "Do stamp-stems break after continued use?" Nor is it, "Do they show a granular fracture when they break?" A thousand instances of such breakage and fracture will prove nothing. But any one of the following suggested tests would prove a good deal:

I.—Let a stamp-stem which has been running for a long time without breaking be taken down and examined as to fracture and structure. This has never been done, so far as I know.

II.—To make the result of I. conclusive, let a comparison be made between such a stem and one made at the same time from the same metal, but not used. This has never been done, so far as I know.

III.—Let a stem which has broken in service be examined as to its structure at other points than that of fracture.

IV.—Let such a stem be tested to ascertain whether, *at any point in it*, it is not possible to produce at will either granular or fibrous fracture by simply varying the means and method of fracture. This has never been done, so far as I know. Nor has Mr. Argall's claim, that such a granular fracture is very different from the "crystallized" fracture, ever been supported by the actual production and comparison of the two.

V.—Let any stem, new or old, used or unused, be tested as in IV. This has been done often with bars of iron or steel, and it has been proved that a granular or a fibrous fracture can be thus produced at will. But if there is anything peculiar about stamp-stems,

the experiment would show it. It has never been performed upon a stamp-stem, so far as I know.

It is such evidence as this that would convince doubters, and prove the crystallization-theory. That theory is now a fable, because such evidence in its support is wholly lacking. And it will never cease to *have been* a fable, because it was framed and held without evidence. It may, indeed, cease to *be* one, and become an acknowledged fact—when the necessary evidence is forthcoming, but not before.

E. E. Olcott, New York City: I am sorry that I have not followed carefully the various contributions that have been made on this subject. I have been much surprised to hear the statements of Dr. Raymond, and regret to be obliged to quote my own experience in opposition to them; but I am very strongly of the opinion, from numerous observations, that some change occurs in iron as a consequence of frequent vibration. The place where I have seen examples of it so often is in the breaking-off of the stamp-stems that has been alluded to. It is no uncommon thing to see around stamp-mills, even now, a number of short ends of the stems, broken off just above the boss or stamp-head, the crystalline faces on which are as distinct as in a broken pig of cast-iron. I have also seen these stems, after having been used, first at one end and then at the other, cut in two in the blacksmith's shop, at places away from the boss, where they show a perfectly fibrous structure. Now, I think that the stamp-mill mortar is an ideal apparatus for causing great vibration at one particular point; and I should say that the numerous occurrences of the iron breaking in that way and the frequent observation of crystallization furnish, perhaps, a stronger argument in favor of the phenomenon than the experiments made by imitating for a short time the strains that the stamp stems sustain. It is a subject that I will try and investigate a little further. As I say, I have no wish to oppose my observation, perhaps prejudiced by the practical mill-men, to the eminent authorities that have been quoted.*

William Kent, Passaic, N. J.: I have been reading up on this subject for nearly twenty years. I saw the porter-bar mentioned by Dr. Raymond in 1875, shortly after it was broken, and everybody who saw it then thought it was an unmistakable evidence of crystallization. In earlier times the belief in crystallization was almost

* See Mr. Olcott's later communication to the Secretary on page 833.

universal. Afterwards people began to differ on the subject, and there were opinions expressed on both sides. In 1879, in a paper "On an Apparatus for Testing the Resistance of Metals to Repeated Shocks" (*Trans.*, viii., 76), I think I was a little on the fence, though rather a little over on the side of the crystallization-theory. Now, I have read whatever I have come across on that subject in the last fifteen years, including Dr. Raymond's paper, which I received the other day, and I am still not satisfied and am strongly inclined to believe with the gentleman (Mr. Olcott) who has just spoken. There is no higher authority than Bauschinger, who has rec ntly died; and Prof. A. Ledebur quotes him as saying: "Strains of iron and steel repeated frequently, millions of times, bring about no change of structure."

That statement seems to me most astonishing when it is well known that you can take a bar of iron or steel and, after it has been subjected for years to shock and vibration, to all appearances there is no change in the piece of metal; yet, if you test it, you will find that it is more brittle; and some day that piece breaks. You may not find the evidence of cubical crystallization, but it always breaks with that appearance which we call crystallization. Now, can we say that that piece has been standing all this shock and vibration and has finally broken without molecular disintegration having taken place until just before it broke? Can we say that that piece has experienced no change of structure? I remember that at one of our meetings we were shown some pig-iron that was very hard to break; it took about four hundred blows of a sledge to break it, and yet finally it did break with a single blow. The same way with wrought-iron and steel. It resists shock a long time, then breaks. Something has happened to it. It may not be crystallization. I do not hold that iron once fibrous becomes crystallized; but to say that there is no molecular change is, I think, going too far. The statements made about Wöhler's experiments proving anything about crystallization do not, in my opinion, prove anything at all; because in his experiments there were repeated steady loads placed on gently, and they were not shocks at all. In the paper that I presented fifteen years ago, and to which I have referred, I pointed out that no scientific experiments on the resistance of steel to shock had been carried on, and gave a design for a machine to test this point. I am very sorry that it has not as yet been built. If it were built, I think it would settle this matter of crystallization. The statements

made during the last ten to fifteen years about what has been proved, or alleged to be proved, amount to nothing; and I claim that the statements that no crystallization happens are all theories and not deductions from actual experiments. If we can get a translation of Bauschinger's experiments, it would be very important; but I do not think he would go so far as to say that there was no molecular change. He says that no change of structure took place; but the expression "no change of structure" is commonly broadened so as to cover every possible kind of molecular change. It may be all right to say that crystallization does not happen, or to say that there is no crystallization which has been determined by the naked eye or by the microscope, or that nothing happens so far as we can see; but as long as pieces of iron, after long service, do break with ordinary loads when they are apparently just as strong as when they were new, we must believe that during this long service something happened to the iron which weakens it, which something is equally dangerous whether we call it crystallization, change of structure, molecular change or molecular disintegration.

JOHN WILKES, Charlotte, N. C.: As a builder and user of stamp-mills for the last twenty-five years, I have to differ in regard to the effect stated here. I do not go into any theories at all, because I have none; but my experience has been that a change certainly takes place in a stamp-rod near the boss or head. Some time ago the custom was to upset or cut in two after the ends broke off, and weld together in the middle. Those stamp-rods we found broke again in the middle, away from the weld, where the iron had been worked in the forge-fire. Now, stamp-users in my section of the country only change the ends. The ends break clearly. When millers were not careful in regard to their work, the stamp-rods broke in a very short time, sometimes in six months; but with greater care, now, the life of the stem is about two years and a half.

VICE-PRESIDENT J. F. HOLLOWAY, New York City (in the Chair): By changing the ends you mean reversing the rods and putting the head on the other end?

MR. WILKES: Yes, sir. Without an exception, all broken ends show a granular crystallized appearance. Some of them I have seen show a granular structure as large and as perfect as in Scotch pig. That they did present a granular effect is without doubt, and it continued above the break, as these rods, when upset, did not break in the weld, but within a foot, more or less, of the portion of the iron

which had been again worked in the forge. During this discussion, and since the paper was read in Chicago, I have taken pains to examine rods which have been sent to us to be turned over. Within the last three weeks, rods have been sent to us that had been in use three years. One of them had quite large crystals, such as you would see in Scotch pig-iron. It was my intention to bring one of those stamp-ends here, as it would have shown the members of the Institute that the fact remains, whatever the theory may be. If there is any place where this question of vibration can be brought up and looked into, it is in a stamp-mill. Think of a weight of some 750 to 800 pounds dropping eighty times a minute for days and months. If granulation or crystallization would take place anywhere, it certainly would be there. I am not a theorist myself. I accept the explosion of the crystallization-theory. At the same time, in a stamp-rod, the fact of granulation or crystallization does exist without any doubt. Within the last year we have begun to substitute steel instead of iron, but with what effect I am not as yet able to say. We find that in mills that are properly taken care of, so that the vibration is reduced as much as possible, rods have a longer term of life. Packing the end of the rod with cloth before it is driven into the boss-head will increase the life of the rod. Scientific men, to whom I listen with a very great deal of interest, may be able to tell whether steel will stand this vibration better than iron or not. We have found that the use of steel in other parts of the stamp-mill where the vibration comes has proved advantageous.

Mr. Holloway: I would like to ask Mr. Wilkes whether it has ever been the practice of people using stamps to take these stamp-rods out at intervals and reheat them in a wood-fire?

Mr. Wilkes: I do not know of anything of the kind having been done. As a user of stamp-rods, as well as a maker, I question whether a wood-fire would do away with this granulation, which certainly does exist after some years' use.

Dr. Raymond: Is there any notable tendency on the part of stamps in the South—with which section of the country Mr. Wilkes is more particularly acquainted—to break just under the tappets?

Mr. Wilkes: I do not know of any breakage of that kind. The breakage is just in the edge of the box or the head.

William R. Webster, Philadelphia, Pa.: I would like to ask Mr. Wilkes if the boss on the rod is put on by upsetting?

Mr. Wilkes: No; it is only where a rod is broken that we upset it. We use the best refined iron. The rods are turned up, and

then tapered in the lathe to the taper of the head, the end of the rod being made a little smaller than the rest of it. Both ends are tapered. The mortice in the head is tapered, and the stamp-rod is driven in; it only requires one or two blows to make it tight. In all the fractures, I have never observed one that extended out of the level above the boss-head an eighth of an inch. It is as if it had been cut off with a knife.

Mr. Holloway: It is the practice of lumbermen, I know, to take their log-chains and throw them on a log fire and heat them up at regular intervals; and it has been found from experience that the life of a log-chain is very much prolonged by doing that.

Mr. Wilkes: I should think that might be a good thing to do. At the same time, the use of a log-chain is very different from the vibrations of a stamp-rod. I know of no place where the vibration is so great as in a stamp-rod, particularly in a mill that is not taken care of properly.

Albert R. Ledoux, New York City: It may be interesting to put on record an experiment which I made that confirms very strongly the statements in the authorities, that even the best wrought-iron will break under certain circumstances with the fracture of cast-iron. A few years ago a steamship broke her shaft just at the point where the crank-arm joined the shaft. It was forged on. Under the Admiralty law, the case having come up in the United States Court, it was held that if that shaft broke from a flaw within it, although the owner of the vessel could not possibly have ascertained that the flaw was there, yet he was liable for the loss of the cargo—which happened to be fruits from the West Indies. If, on the other hand, the break was caused by the stress of weather, then the loss would fall on the insurance company. It was, therefore, an important matter to show why the shaft broke. I was an expert witness in the case, and had opposed to me an eminent marine engineer. He examined the fracture of the shaft, and testified that it was undoubtedly due to the poor quality of the iron; that the fracture was granulated; and that the granulation was not due to age, but to the fact that the shaft was poor in the beginning. I simply placed in evidence Kirkaldy's work, and called attention to the fact that there might have been a sudden strain upon the shaft, and, if so, according to Kirkaldy, it might break with crystalline fracture. In order to test this theory of Kirkaldy's, I went to the Rogers locomotive works and asked them to give me one of their best wrought-iron car-axles. This axle we broke on an anvil by means of a steam-

hammer, bending it back and forth until it was broken with fibers as long as your finger. It was a new axle. We then laid one-half of the axle, with the fibrous end, on the anvil, and tapped it gently until we had straightened it. Then the piece was placed between two rather narrow supports, and the full force of the hammer was allowed to strike it with a blow of many tons. It suddenly broke, and I had a piece six inches long, one end of which was beautifully fibrous and the other end showed coarse crystals. That piece we took into court and established our case.

R. P. Rothwell, New York City: I think there can be no question among those who have used iron and steel under such conditions as have been described that there is a molecular change under certain circumstances. Whether that change be due altogether to shock or not is, perhaps, an open question. At least, the same effect seems to be obtainable by different means. At one time I was using a very soft steel wire rope. It was too soft for ordinary use. We were using it on a slope, and the lower end of it would become highly crystalline. Periodically we would have to cut off six or eight feet of it. The wire before its crystallization (and I call it crystallization for convenience) was extremely tough. You could twist it round and round many times without breaking it; and when it did finally break it showed the finest silky texture. But where it broke at the end of the rope in use, it appeared under a magnifying-glass as if the carbon of the steel had collected into flakes of graphite running across the structure of the steel, and it had broken through these graphite structures. We attributed this effect to the acid water of the mine. It was a coal mine, and the rope would get wet at the foot of the slope. Now, a stamp-stem is sprinkled constantly with water, and that water is very likely to be more or less acid. It may be admitted freshly, yet still it is almost always in contact with pyrites in the ore. I merely suggest whether there may not possibly be some such action as that which we observed with the steel wire rope.

Mr. Holloway: Is it not true that at the bottom of the slope there would be a little slack at the end of the rope, and that in taking up the slack the tension would come suddenly on that end of the rope?

Mr. Rothwell: Yes, to a certain extent. It would not be sudden, however. With a wire rope 500 or 600 feet in length there is so much spring that you would get no sudden jar—nothing that you could fairly look to as a cause for crystallization in the wire.

Mr. Wilkes: There is no question in my mind but that mine-water will have an effect in the way Mr. Rothwell described. In the South, however, miners do not use mine-water in their stamp-mills, if it is possible to get surface-water, on account of its effect on amalgamation. Therefore, the water we use could not have much effect on the iron.

Joseph C. Platt, Waterford, N. Y.: This discussion brings up some reminiscences of the time when I was a young man and employed in puddling. I remember distinctly breaking puddling-tools, the ends of which were about seventh-eighths of an inch in diameter, with a hammer weighing not over four pounds, when striking the tool a blow only sufficient to jar off the cinders. It was a blow which would not have probably driven a ten-penny nail half an inch into a plank, and yet it broke the bar in a manner that would indicate that something was wrong with the iron. These handles were used for probably twenty-five years. The end that was put into the furnace was, of course, renewed frequently. When a tool became hot it was pulled out and another put in, and it was simply subjected to the jar of the puddle-bar on the front-plate under the notch. It was a very common occurrence for the handle to break off under a blow such as I have stated. It seemed to me a very strange thing that iron which had lasted for years would suddenly break in that manner.

A Member: It was at blue heat, probably.

Mr. Platt: No; that end of the tool did not get very hot. That was the cold end.

W. F. Durfee, West Brighton, Staten Island, N. Y.: In a lecture delivered May 11, 1887, at Annapolis, Md., before the United States Naval Institute (See *Trans.*, U. S. Naval Institute, vol. xiii., No. 3, pages 369–376), I discussed this question at considerable length, and the following statement, condensed from that lecture, expresses my view to-day as completely as it did eight years ago. The portions omitted in this abridgement were in the nature of explanatory repetitions and illustrations, adapted to a popular audience, but not necessary here:

Wrought-iron is really a mechanical mixture consisting, at its best, of clusters of crystals (which may with propriety be regarded as compound crystals) of iron, separated from each other by films or threads of cinder, as the unavoidable result of the process of manufacture. When the puddler's balls are squeezed or hammered, for the purpose of expelling the cinder and welding the granules or crystals of iron into a homogeneous mass, the attempt is never wholly successful; for the

cinder, as the metal cools, quickly becomes pasty and flows with difficulty, so that portions of it, inclosed in the interior cavities of the ball, are simply flattened out or elongated, but not removed. Hence the bloom is a complete mass of granules or crystals of iron, separated from each other by films or strings of cinder of very irregular dimensions.

By crystals of iron I mean ultimate units of that metal bounded by well-defined planes, whose intersections always form salient angles. A number of such crystals may cohere and form an aggregation, and such aggregations, or compound crystals, vary in size, and are often spoken of as single crystals, just as we speak of crystals of galena or calc-spar, when, as a matter of fact, the ultimate crystal of each of these substances remains undiscovered, and as undiscoverable as the boundaries of space.

In forging a bar it is the usual practice to turn it about its axis through an angle of 90° between the blows (or series of blows) of the hammer, and in rolling a bar it is commonly turned through the same angle between passes through the rolls. Consequently, when a bloom is rolled or forged into a bar the metal is acted upon in two directions, at right-angles to each other, and its compound crystals will be compressed in directions normal to the exterior surface of the bar and at the same time extended in the direction of its length. Thus the ends of adjacent crystals are forced toward each other, and the intervening cinder, endeavoring to escape, is compelled to move at right angles to the axis of the bar and to unite with the films or threads of cinder which have become established in parallel lines of least resistance along the flanks of the compound crystals and at right angles to the direction of the force upon the bar.

The direct consequence of the elongation of the compound crystals and the effort of the intervening cinder to escape in the direction of least resistance is the establishment of that structural peculiarity in the resulting bar known as "fiber," which is a conspicuous feature of wrought-iron not found in any other variety of ferruginous material. When any of the films or threads of cinder in a bar of wrought-iron are so large as to be distinctly visible on its surface to the unassisted eye they are called "sand-seams" or "cinder-cracks."

If its compound crystals are merely pure iron, the bar can be readily bent cold without fracture, and, if pulled asunder by a gradually augmented force, its fibrous texture is at once evident; but in case the compound crystals contain in chemical combination some substances, such as phosphorus or silicon, which tend to diminish both the cohesive attraction between crystals and the mutual attraction of the compound crystals, then the bar cannot be easily bent cold without rupture, and exhibits, when broken, a so-called "crystalline fracture." Notwithstanding this appearance, however, the mechanical structure of the bar is the same as before, that is to say, the cinder and the elongated compound crystals are still arranged in lines parallel with the axis of the bar, although it is quite probable that the average length of the compound crystals may not be much less than in the case of a bar of purer iron.

Whenever a bloom is subjected to a force of compression always acting perpendicularly to the same plane, as is the case when it is rolled into a sheet or plate, both its compound crystals and the accompanying cinder are flattened and extended parallel with that plane, and the resulting sheet or plate has more of a laminated than a fibrous structure, being built up of a number of leaves or strata of iron, separated from each other by films of cinder, which, when unduly thick at any point, cause defects in the plate that are called "blisters."

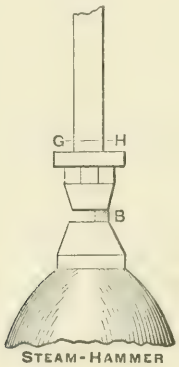
Can a bar of wrought-iron of a pronounced fibrous structure be ruptured so as to

exhibit a crystalline fracture? I answer, Yes, in two ways: first, by a sudden application of a force of extension commonly called a "jerk;" second, by a prolonged repetition of a force of compression, sometimes called a "jar."

The first method of rupture may be said to consist of a transverse separation of the compound crystals of the bar, as distinguished from a sliding of their interlocking flanks upon each other, as is the case when the rupture presents a fibrous appearance. I have often seen crystalline fractures produced in truly fibrous iron. In the manufacture of iron rails (now nearly an extinct industry) it was always considered desirable that they should be of a hard and crystalline texture as to their heads, but soft and fibrous in their flanges; but, however perfectly this distribution of metal was made, it was always possible to break a rail so as to show a crystalline fracture in its flange. This was accomplished by making a slight nick across the flange (to determine the point of fracture) and placing the rail (flange down) in the straightening-press, on supports placed a short distance on either side of the nick and then putting in the gag "heavy," just over it; the result was almost always a crystalline fracture in the flange; in short, the elongated compound crystals were jerked asunder. But if the points supporting the rail were placed further apart, and the rail given an opportunity to yield considerably between them, then, if the gag was put in "light" a number of times in succession, the fracture of the flange would be sure to exhibit a fibrous texture, due to the fact that sufficient time had been given to break up the films of cinder along the flanks of the compound crystals and destroy their transverse cohesion, thus permitting them to slide apart and exhibit the appearance of disrupted fibers.

We are indebted to a not uncommon accident, to which the hammer-bars of a peculiar type of steam-hammer are liable, for an excellent illustration of the second method of producing a crystalline fracture in fibrous iron, the result of the repeated action of a percussive force of compression. In Fig. 1 the bar of such a steam-

FIG. 1.



hammer is represented. As has been stated, there exist, in a bar of fibrous iron, films of cinder between the ends of its elongated compound crystals. These, from the nature of their formative process, cannot possibly be of uniform thickness. This, considered in connection with the fact that the greatest force of the percussive action per unit of area of any cross-section of the hammer-bar is exerted upon a section made by a plane cutting the bar at right angles immediately above its head, justifies the belief that at or near this point fracture would be most likely to occur. It is also evident that the percussive action of the hammer would have more de-

structive effect upon thick than upon thin films of cinder; while, at the same time, the force of cohesion between the ends of adjacent compound crystals will be diminished in some inverse proportion to the thickness of the films of cinder between them. It therefore seems exceedingly probable that the fracture due to continued percussion will take place, if not in the plane above named, yet in one very near to it, in which the cinder-films chance to be of greater thickness than those in that plane; and, as a matter of fact, fractures in such bars are usually within a few inches of the point where the bar enters its head, as at G—H, Fig. 1.

The particular point in the circumference of such a hammer-bar where the imminent fracture first appears is often determined by the manual peculiarity of the "hammer-man." A left-handed man will incline his work to the left, and a man who is right-handed will be likely to use the right side of the anvil more than the left. In the latter case, the work B, Fig. 1, will tend (whenever it is in the position shown) to produce a tensile strain at the point G, which, as the work is shifted to the center or occasionally to the left side of the anvil, becomes a compressive strain. We should, therefore, expect (as is, in fact, the case), that the initial manifestation of the fracture would be found at that point, and that it would gradually extend towards H, until the bar was finally "jarred" asunder. The separation would take place through films of cinder between the ends of the elongated compound crystals of the bar, thus exposing those ends, and exhibiting what is called a crystalline fracture.

The belief in the so-called crystallization of wrought-iron, as the result of prolonged use, is, I think, altogether a mistake; and I am clearly of the opinion that the crystallization observed in the case of any particular fracture existed, just as we see it, at the time the metal was given the shape in which it was ruptured. After a bar of distinctly fibrous wrought-iron has been subjected to multitudes of sudden jerks of extension or jars of percussive compression, the cinder in some cross-section of it (in which this impurity is slightly thicker than elsewhere) gets broken up; cohesion is destroyed, and the bar breaks with a crystalline fracture.

I have had a specimen prepared for the purpose of making the foregoing explanation of the apparent crystallization of fibrous iron more evident. It is a short piece of a square bar of wrought-iron. One end is decidedly crystalline in its fracture, showing distinctly that the bar was originally built up of five flat bars. The other end is, for more than one-half of its area, as decidedly fibrous as wrought-iron can well be; and this end would have been uniformly fibrous in appearance had the workman who made the specimen exercised the requisite care. Thus, in a sample not over two inches in length, we have an instance of a fracture which most observers would call very bad, and another which as certainly would be called good.

It is a well-known fact that wrought-iron is improved in strength by repeated working. This may be accounted for thus: In the initial heating and shaping of the metal, its crystals were left with a comparatively thick film of cinder between them; but, by each successive reworking, the crystals of metal are driven into closer order, some of the intervening cinder is expelled, and what remains is very much reduced in thickness, so that the cohesive attraction (whatever that may be) between these crystals, having less space to act through, acts with augmented intensity. It is well to remember, when we speak of "less space" in a matter of this kind, that we are dealing with a very small quantity indeed—one that is a near neighbor to the infinitesimal.

W. H. Shockley, San Francisco, Cal.: With regard to the use of mine-water in the battery, the custom on the Pacific coast is the

same as Mr. Wilkes states it to be in North Carolina. Mine-water is not used in the battery when it can be avoided.

I do not think the water causes the stamps to break, as suggested by Mr. Rothwell; for they last longer in a wet-crushing mill than they do in a dry-crushing one, where no water at all is used.

From my own observation, I do not think the vibration causes the stamp-stems to crystallize and hence to break. The chief strain on a stamp-stem is a bending-strain, caused by pieces of rock tilting the stamp when struck by the circumference of the shoe. This gives a strain, nearly all the effect of which is concentrated at the place where the stem enters the boss; and it usually breaks at this place. I have noticed on shafts that have been broken flattened places on the fractured surface, showing that the bending caused enough motion to wear the surfaces smooth.

The pieces of metal mentioned by Dr. Ledoux and Mr. Durfee prove conclusively that the appearance of the fracture does not show what the internal structure of the metal was before it was broken.

If the vibration causes stamp-stems to crystallize and break, it certainly requires a very long time to produce that effect; for I have known stamps to be in use for four years, dropping 95 times per minute throughout that period, which would give something over 200,000,000 blows.

As a matter of interest, however, I remark that all the blacksmiths and manual workers of iron with whom I have talked believe that iron will crystallize under shock.

Mr. Webster: Some years ago, an inspector of bridge-material, after making thorough tests of double-refined iron bars for eye-bars, was so well satisfied with the tension-, bending- and nicking-tests that he made a special report to the rolling-mill, saying that it was the best material he had ever inspected. After these bars had been manufactured and shipped to the mill-site, a test of the full-sized bars was received, which showed very poor results, the bars having been broken through the head and in the neck with bad crystalline fractures and low ultimate strength. The bars were all condemned and taken out of the structure. A thorough investigation was made of this material by nicking-tests, starting in the center of the bar and going towards the end; and in all cases good results were obtained from the body of the bar, and crystalline fractures in the neck. Bending-tests without nicking showed the same difference. In many cases the bars did not bend 10 degrees in the neck, but even at the first stroke or two of the small hydraulic jack that we were

using, the sharp, snapping sounds were heard and the material gave way all at once. The crystals were very large. Additional tension-tests of the full-sized bars were made, and some of them broke in the neck with as low an ultimate strength as 42,000 pounds per square inch, the fractures being all crystalline. Had these bars been in use several years, when this trouble was discovered, it no doubt would have been cited as another instance of crystallization of the material, caused by the vibration.

I cite this to show the importance of knowing the heat-treatment to which iron has been subjected before we attempt to theorize on the change of structure due to vibration.

In 1884, Mr. Peck, superintendent of bridges for the Missouri Pacific Railroad Company, called my attention to the fracture of some eye-bars, taken from the wreck of one of their bridges, which had been knocked down by a derailed train. These bars, he claimed, were made from good material by one of the leading bridge companies of the country, and yet they broke off short like pot-metal. Upon thoroughly investigating, we found that the bars had broken through the neck, with a coarse, crystalline fracture. I called his attention to the trouble often caused in that portion of the bar in the course of manufacture; and he embodied in his new specifications a clause which called for "eye-bars to withstand bending to a curve of 90 degrees in the neck." This test was carried out by subjecting to a welding-heat a piece of the bar about 16 inches long, allowing it to cool slowly without putting work upon it, and then bending it under a press. Several lots of material were condemned as not meeting this test.

In 1881, while we were making bending-tests of double-refined bar-iron under a small hydraulic press, the work was interrupted after several pieces had been bent about 170 degrees over a 2-inch round. Twelve hours afterwards, these pieces were put on end under the hydraulic press and we attempted to close them down further. Much to our surprise, they broke off short, the fracture being 100 per cent. granular. At first it was thought that the cold might have had something to do with this, as it was in the winter season and the pieces had been left out overnight; but upon repeating the experiment and keeping the pieces indoors all night, at a temperature of about 70° Fahr., we got the same short fractures as obtained before. The same bars, when broken in the ordinary way, that is, without any interruption of the test, gave fibrous fractures and were satisfactory in every respect. This experiment was re-

peated on different sizes and makes of iron; and sometimes the fracture was changed and sometimes not. (I refer to the fractures as granular, as they were entirely different from the crystalline fractures cited above as being produced by the heating of the bars.) It would be interesting to follow up a set of experiments on this line and carefully note all the conditions, including chemical composition, in order to get at the cause of this apparent change of structure.

I believe I have still a piece of one of these bars, about four inches long, one end of which is entirely granular and the other end fibrous.

Dr. Raymond: It will be remembered that this controversy originated as a side-issue, in connection with a casual observation (*Trans.*, xxiii., 143) in Mr. Rickard's paper on "The Limitations of the Gold Stamp-Mill." The discussion having taken a wide range, and aroused special interest, it has been deemed advisable to publish this continuation in the present volume under a new title, more precisely indicating its subject.

This discussion illustrates forcibly the importance of attaching definite meaning to the terms employed in describing observed facts. "Molecular change" and similar phrases—even "change of structure"—may be (and, I fancy, have been, in this discussion) employed as signifying no more than incipient fracture, or the progressive separation of the units of structure in the line of stress, or the gradual diminution of tensile strength under repeated stresses. Strictly speaking, not one of these phenomena necessarily involves *molecular* change, such as is involved in the rearrangement of the molecules, to form crystals. That they do indicate, in a certain sense, a structural change, is not denied. But this change may be the same in kind as that produced by any kind of fracture. When any two continuous elements of structure are pulled apart, whether gently or violently, gradually or suddenly, there is a change of structure, if we choose to call it so. But it is useless to confound that change with one that is supposed to take place prior to any rupture between the elements. When Mr. Kent speaks of "molecular disintegration," I understand him to mean a loosening of the existing structure, not the formation of a new one; and, in that sense, I conceive that he is stating exactly the position assumed by modern investigators, who fail to find any proof of a radical change of structure preceding fracture.

Mr. Kent does "not hold that iron once fibrous becomes crystallized," yet declares that "the statements that no crystallization hap-

pens are all theories." I must repeat my protest on the latter point. The advocates of the crystallization-theory have no right to call simple disbelief in this proposition a "theory." It is incumbent on them to prove their position; they cannot demand that doubters should prove a negative. As to the only theory here under discussion, it is perhaps not fairly represented by the proposition that "iron once fibrous becomes crystallized." If that be the theory, then it suffers under a double lack of proof; for there is no evidence that any iron is fibrous prior to rupture. We produce a fibrous or a non-fibrous fracture at will, according to the method of breaking.

Mr. Durfee's explanation of the process of fracture in wrought-iron seems to me to satisfy the observed facts, although I do not think that the presence of films of cinder between the elements of structure is absolutely necessary to an explanation. Planes of smaller cohesion might suffice. What Mr. Durfee has pointed out concerning the breaking of steam-hammers is, to my mind, pertinent and conclusive; and I deem it highly significant that he has directly observed, in such cases, incipient fracture.

It seems to me also significant that Mr. Wilkes's stamp-stems break only just above the head and not under the tappet, while Mr. Austin reports that in western mills the stamps break in both places. I am inclined to infer that the North Carolina arrangement of tappets and cams is superior; and I may go further and say that possibly some better connection between stem and head might reduce the amount of breakage in both types of mills. I venture to believe that if a stamp were composed, for instance, of one solid cylinder of iron, of equal diameter throughout, there would be no sign of "crystallization" in it if it ran fifty years. In other words, I think there is no proof of an inevitable destruction of the material, by the operation of a universal law, which cannot be largely prevented by strengthening the parts now exposed, without special protection, to nicking- and bending-strains.

In this connection, I would call attention to two very able and thorough articles by Mr. Paul Kreuzpointner, of Altoona, Pa., entitled, "Do Iron and Steel Crystallize in Service?" and published in the *Iron Age* of July 5th and September 27th, 1894. Mr. Kreuzpointner is the accomplished assistant of Dr. Dudley in the Altoona laboratory of the Pennsylvania Railroad Co. His discussion of this subject ought to convince any one who still inclines to the "crystallization-theory" of the baseless and untenable character of that

theory. I will quote but one sentence from his second article, which gives a new reason for disputing the traditional error. He says:

"It would hardly be worth while to take the old superstition about the crystallization of iron under shock seriously at this late day, if it were not for the fact that this superstition is being transferred to steel. This is really a misfortune to the constructing engineer who may happen to believe in it, and to the consumer of steel in general."

Mr. Wilkes: Referring to what Dr. Raymond has said concerning breakage under the tappet, I have no doubt that the best shapes for cams and tappets should be used, so that, when the lift begins, the blow may be as light as possible, and the friction between cam and tappet during the whole lift may be as small as possible. This shape we have secured in our practice by adopting true curves at first, and modifying them as observation of their behavior in actual work suggested. While we were using iron, we succeeded in this way in reducing vibration, wear and tear to a minimum. Since we have adopted steel for the parts referred to, a great further reduction in wear and breakage has been secured, as the result, in my opinion, of the retention of the original form of the cams and tappets, and consequently, the more certain keeping of the stem in its proper place during the lift. This permits a fairer blow, and more effective work. The stamp-mill is often regarded as a rough machine that can be taken care of by anybody. But it needs, like any other machine, to be kept in order, if it is to do good work. Suitable care bestowed upon it will effect improved results as important as those to be got from any other kind of machinery used about a mine. A properly-constructed and properly-handled stamp-mill is, by reason of its simplicity and its economy in metal consumed per ton crushed, still the favorite appliance for reducing ores for amalgamation and concentration.

Mr. Olcott (later communication to the Secretary): The result of a little study on the subject shows the weight of scientific argument to be against the crystallization of iron from shock or vibration at ordinary temperature. I have read the two able articles on the subject by Mr. Kreuzpointner, to which Dr. Raymond has called attention. The salient points of these papers, as affecting the stamp-stem discussion, seem to be:

1. That the crystalline appearance on the fracture is caused by the manner of breaking. That is, where fibers are broken transversely they show granular or crystalline faces, but where pulled apart lon-

gitudinally, the same iron shows a fibrous structure. In other words the stamp-stem may have been weakened and finally broken off by successive shocks, and short kinks or bends, operating transversely, as the result of striking uneven surfaces in the mortar, etc.

2. The iron in a bar may be crystalline at one point, but fibrous at another.

3. Iron may have been crystalline at the point tested, but assumed a fibrous appearance at the tensile fracture, due to the flow of metals.

4. Mr. Kreuzpointner not only gives his own opinions, but quotes eminent German authorities in support of the idea that changes in the component elements of iron are necessary for changes in its crystallization, and that these changes cannot occur at low temperatures.

5. The results of Dr. Wedding's researches are given to show, also, that repeated stresses cannot produce crystallization.

While, therefore, there is a strong weight of argument against the crystallization of iron in service, Wöhler and Spangenberg agree that alternate and intermittent stresses tend to deteriorate and fatigue metals; and Mr. Kreuzpointner says:

"If we consider how, with insufficient dimensions and impaired cohesion, sudden shock will produce sudden fracture, then we have all the elements necessary to produce the well-known crystalline appearance of the fractured surfaces.

"The fractures will thus appear crystalline, even if the iron were ever so fibrous, because of the suddenness of rupture which did not allow the metal time enough to flow, giving, consequently, a clear transverse break of the fibers, which, as already explained, are nothing but elongated crystals, the transverse sections of which are the measure of their sizes."

Wöhler declares, as the result of his experiments, that "the members of structures which are subject to alternating strains, pulling and pushing, or bending and twisting, ought to be made larger in the proportion of 9 to 5."

Pieces of iron, planed, polished, and etched, are said to give "undoubted evidence of the crystalline conditions existing before the iron was ever subjected to any strain."

The foregoing seems to establish that, though there may be the weakening of stamp-stems by repeated shocks, which finally may cause them to break suddenly, thereby showing the crystalline faces of the iron to great advantage, there has been no enlargement in service of such crystalline faces in the iron.

H. M. Howe, Boston, Mass. (communication to the Secretary): Will Dr. Raymond let me modify the statement, which he gives,

Trans., xxiii., 560, of my position in regard to the crystallization theory of rupture under repeated stress and vibration? My argument on page 196, *et seq.*, of my *Metallurgy of Steel*, was that, though it was quite conceivable on *a priori* grounds that vibration might make iron crystallize, yet there was no evidence that it ever does. My summing up was that we have "every reason to believe that the granulation and crystallization of iron under vibration and shock is a myth."

We seem to be at cross-purposes with Mr. Argall. He seems to think that people have denied that iron under certain sets of conditions, some of which include shock and vibration, breaks with a crystalline fracture: whereas, so far as I know, nobody has ever denied this. It is not the occurrence of a crystalline fracture but its explanation that is in dispute. I suppose that he must have fallen into this confusion; for I see no other way of accounting for his setting forth the undisputed crystalline fracture of stamp-stems in such a way as to imply that it answers the question at issue.

Let me try to sum up briefly the condition of our knowledge. Repetitions of stress, wholly unaccompanied by vibration and shock, are well known to induce some kind of deterioration which eventually breaks iron. Vibration and shock, unaccompanied by great stress, or at least by prolonged repetition of considerable stress, have never, so far as I know, been known to break it. This points to repetition of stress, and not to the vibration and shock which only in certain cases accompany or cause it, as the real cause of such breakage.

Examination of the fragments of pieces thus broken by repeated stress, even when accompanied by vibration and shock, has indicated that the injury was local;* and careful microscopic examination of the fragments close to the fracture has detected no crystalline change, but at most a shattering and incipient separation of the pre-existing particles, grains or crystals, whichever you call them.† All the evidence has been thus against the theory that vibration caused even a local crystallization.

The crystallization-theory thus was a discredited one. Fresh evidence might indeed rehabilitate it. But I fail to see that Mr. Argall

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* Baker, *Trans. Am. Soc. Mech. Eng.*, viii., p. 163, 1887. Howe, *The Metallurgy of Steel*, p. 199, Column 1. Sorby, *Journ. Iron and Steel Inst.*, 1887, i., p. 265.

† Martens, *Stahl und Eisen*, vii., p. 238, 1887. Sorby, *Journ. Iron and Steel Inst.*, 1887, i., p. 265.

has given us the faintest ray of evidence or of reasoning in favor of that theory.

We know that iron, if nicked on one side and bent backwards, yields a fibrous fracture, but that the same bar, if nicked all around and broken with a sharp blow, yields a crystalline one. The two different modes of causing rupture induce it to follow different paths, and yield different fractures; for the fracture is nothing more than the path of rupture. In this case nobody supposed that nicking all around and breaking with a single sharp blow has crystallized the iron; it has simply developed a new path for rupture. Thus a crystalline fracture is shown to be no proof, but at most only a suggestion, of crystallization. The planes along which the rupture of the nicked bar travelled existed before rupture followed them, just as the cleavages in a feldspar crystal exist before I cleave the crystal with my knife, and as the image exists in the exposed but undeveloped photographic plate.

Mr. Argall vainly attempts to escape from the fact that "iron when fractured suddenly presents invariably a crystalline appearance: when fractured slowly its appearance is invariably fibrous," by his unqualified assertion that "In the first case the fibers are not given time to stretch, but are broken off at right angles to their longer axis, whence the apparent fine crystallization; while, in the latter case, actual crystals are developed in the iron, some reaching as much as 0.25 inches in diameter."

Let us see how true this theory is. First, so far as our present evidence goes, there probably are no fibers in iron such as Mr. Argall supposes, prior to rupture. Its particles apparently are nearly equiaxed.

Next, when a crystalline fracture forms in suddenly breaking iron, its faces are not, as Mr. Argall asserts, at right angles to the imaginary fibers, or to the axes of the fibers which would actually have formed during fiber-favoring rupture. They are in general approximately at an angle of 45° with those axes.

Finally, it is not the suddenness of breakage, as such, that gives us a crystalline instead of a fibrous fracture; for in certain extremely rapid breakages, as for instance when a bar is torn apart longitudinally by an explosion of gun-cotton, we get invariably a silky fibrous fracture.*

The simple truth is, that each new mode of causing rupture seems

* Maitland, "The Treatment of Gun-Steel," *Proc. Inst. Civ. Eng.*, lxxxix., pp. 120, 121, 1887.

to direct it along a special peculiar path, and causes a special fracture. The fracture thus depends jointly on the properties of the material broken, and the conditions under which breakage occurs. Why rupture follows this or that special path under special conditions, is for the elastician and mathematician to determine with great care. Even for them the question is no easy one; and it certainly cannot be brushed aside off-hand or answered at random by those who run.

With these facts before us, shall we wonder if the special set of conditions under which breakage occurs in stamp-stems directs rupture along still a new special path, and thus yields a special kind of fracture? Is this special kind of fracture really any stronger evidence of crystallization than the other kind of crystalline fracture which we had long known that we could cause by nicking all round?

The defenders of any discarded theory, of this one as of the corpuscular theory of light, need not trouble themselves to show that their theory is conceivable; that it does not violate "any law of modern physics or of the molecular theory of matter." What we need is evidence which this theory explains, and which other theories cannot explain. We have no room for theories which are simply conceivable or even possible. We want those which are probable through evidence. But evidence, like the fracture of stamp-stems, which accords equally well with either theory, really helps the accredited theory but does not help the discredited one.

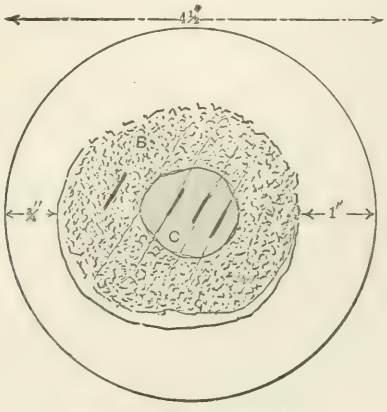
If Mr. Argall or Mr. Wilkes will send me a piece of a broken stamp-stem containing the fracture, I will gladly try to procure some evidence which will count, whether it be for or against the crystallization-theory.

It seems to me that the chief teaching of this discussion is care in the use of words. Had Mr. Argall contented himself with saying (*Trans.*, xxiii., p. 557) not "vibration under all conditions will crystallize iron," an assertion certainly wholly unjustified and probably very far from the truth, but "severe shock will eventually weaken or destroy iron," he would have asserted all that was necessary for his purpose. By going beyond this, and needlessly asserting that *all vibrations* injure iron, and by specifying that the particular way in which they injure it is by causing a crystalline change within it, he gave criticism a most pressing invitation.

His calling those whose opinions he attacks "dogmatic theorists" seems unfortunate. If by theorists he means those who habitually

study the causes of phenomena, or "theories," he simply says that their habits should qualify them to form trustworthy opinions as to the cause of this phenomenon. If he means that they are ignorant of the conditions under which metals fail in practice, he is simply mistaken. And as to dogmatism, those whom he attacks have not *denied*, but *questioned* and *doubted* crystallization by vibration; while he positively asserted at first that *vibration under all conditions* will crystallize iron; and his later modification merely limits the proposition to certain conditions, without changing its character as a positive assertion. It is bad enough for the skeptic to be excommunicated, but to be called *dogmatic* to boot, and by the Pope at that, would be rather bewildering.

Dr. Raymond: Since the foregoing discussion took place, I have received from Mr. Argall, in a private letter, the following statement, which seems to me worthy of preservation in the record as a pertinent observation. He writes that on the 24th of February last, he



Fracture of Railway-Axle.

was delayed for some time near Hill City, South Dakota, by reason of the fracture of an axle under the tender of Burlington engine No. 256: "The axle broke off close to the wheel; an old and rusty crack, varying in depth from three-quarters to one inch, ran completely round the journal; next came coarsely crystalline iron, while in the center the iron was beautifully fibrous, and showed the bars from which the axle had been forged. These, by the way, as indicated by heavy lines in the drawing, were not properly welded."

The accompanying figure, made from a pencil-sketch in Mr. Argall's letter, illustrates his statement. I will only observe, as to the

conclusions to be drawn from this case, that the facts seem to me consistent with the theory of progressive fracture, and with the well-known relation between the nature of the stress causing fracture and the appearance of the fracture-surface.

The indications of imperfect welding observed by Mr. Argall may fairly be taken as evidence of improper heat-treatment for the process of forging; and this, as has been emphasized in the present discussion, is a source of crystalline structure (or, more precisely, of that condition which yields a crystalline or granular fracture under circumstances in which a fibrous fracture would otherwise be expected). The existence of the old crack round the outside seems to indicate that this part of the mass was in such a condition as to break without such elongation as might have held the whole axle together, until a fibrous fracture of the whole had been effected. In other words, improper heat-treatment may have over-heated the outside and under-heated the center of the forging, so that the former became "crystalline," while the latter, not hot enough to weld perfectly, retained the capacity of elongation before fracture, which is called "fibrous structure."

On this hypothesis, the axle, if broken at any time after manufacture, would have shown on the surfaces of fracture a difference of quality between the outside and the inside. But it should not be forgotten that such a fracture would not fairly represent the process of repeated shock and stress undergone by the axle in practice. Even if the material were uniform throughout, the peculiar nature of the stresses to which it was subjected might well develop differences in the successive fractures of different concentric parts. Recent experiments have proved the somewhat surprising fact that locomotive wheels advance, not in constant contact with the rails, but by a series of jumps. If I remember correctly, these experiments were confined to driving-wheels; but it seems to me that the same proposition must be true in some degree of all railway-wheels, especially those which are nearest to the drivers, and thus receive most directly the effect of the successive jumps of the latter. We have to consider, in that case, the effect of transverse blows, repeated at the rate of 1000 to 2000 times per minute. Considering this rate of rapidity, and the weight supported by a railway-wheel, I think I am justified in saying that the test is more severe than that to which stamp-mill practice subjects the stem of a stamp. But the effect of this series of blows is doubtless somewhat different. Each shock exerts a tensile strain upon the lower, and a corresponding strain of compression

upon the upper, half of the axle. It is obvious that, by virtue of the revolution of the axle, every part of the circumference experiences these strains in rapid alternation, and that every part of the interior experiences them in degree dependent upon distance from the neutral axis. On the assumption of the complete homogeneity of the axle as to structure, condition and internal strains due to heat-treatment, it would still be natural to expect that the outer portions (under stresses not sufficient to rupture the whole mass practically at once) would break not only first, but with the smallest amount of elongation, and that the central portion, breaking last, would show the greatest elongation before fracture, because it would have been exposed to gradually increasing stresses, as the progressive fracture of the outer concentric portions increased the intensity of stress upon those remaining. Another point deserves consideration, namely, that up to a certain stage in such progressive fracture, both bending and elongation of the outer layer are resisted by the rest of the mass, a condition which diminishes with the decreasing diameter of the unbroken central portion.

If it be supposed that the axle, by reason of its heat-treatment in manufacture, or for any other reason (such as different quality of its original parts), was not homogeneous in the respects mentioned above, the differences in its fractured surfaces might be increased. The instance cited by Mr. Argall, therefore, while it may be consistent with the notion that the railway-axle in question was once wholly fibrous, as at C, and had become, in use, crystalline at B, before its fracture, does not require or prove that theory.

R. A. Hadfield, Sheffield, England (communication to the Secretary): I have long entertained the idea that many of the so-called fractures by vibration were really due to previous, and often careless, heat-treatment. I can say, after personally handling a very large number of specimens, that I have never yet found a case which could not be satisfactorily explained when the previous heat-treatment could be traced.

F. Osmond, Paris, France (translation of a communication to the Secretary): Having read the discussion of this subject as printed thus far,* I take occasion to say that I am fully in accord with Dr. Raymond's view. I know of no fact which demonstrates the crystallization of iron by vibration; and all that I do know is opposed to that opinion. The aspect of the fracture depends upon the original quality of the iron and the mode of rupture.

* Up to page 824.—R. W. R.

As to the formation of *beta*-iron by shocks and vibrations, that is another question. As Dr. Raymond has correctly pointed out, it is only in the case of permanent deformations that the production of *beta*-iron can be seriously argued. It appears to be, however, not impossible that the elastic limit may be exceeded *without apparent deformations* under the action of vibratory forces which operate at each point for an extremely short time only. But this is a mere hypothesis. If it is well-founded, it could be verified by determining the coercive forces of the iron before service and after rupture. The production of *beta*-iron would be indicated by an increase in permanent magnetism. The truth is, we know at present almost nothing as to the transmission of mechanical waves.

EXPERIMENTS FOR DETERMINING THE REFRACTORINESS OF FIRE-CLAYS.

Discussion of the Paper of Messrs. Hofman and Demond. (See p. 42.)

(Virginia Beach Meeting, February, 1894.)

PROF. DR. H. SEGER and MR. E. CRAMER, *Chemisches Laboratorium für Thonindustrie*, Berlin, Prussia (communication to the President)*: We have learned, with much interest, from the pamphlet sent to us, of the work of Messrs. Hofman and Demond, particularly as we observe from their experiments that these gentlemen recognize the usefulness of the Seger cones, and that this method of determining temperatures in the furnaces of the ceramic industry and in the tests of refractory clays is valued, not only in all parts of Germany, but also in other countries. Permit us to say, with reference to the crucibles employed in tests for refractoriness, that we have used of late, besides the magnesia crucibles, also those made of Zettlitz kaolin, with the addition of alumina, *previously calcined*, with regard to which we beg to refer to our article in the *Thonindustrie-Zeitung* of December 9, 1893.†

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* Translated by the Secretary.

† SECRETARY'S NOTE.—The following is a translation of an extract from the article referred to:

"As the material of the crucibles for these experiments, a highly burned *chamotte* is used, consisting of equal parts of alumina and Zettlitz kaolin, with a certain additional amount of the latter, added raw, to make the mass mould properly. For crucible-stands we use, on economical grounds, a refractory *chamotte*, which does

So far as the testing of clays alone is concerned, these crucibles are to be preferred to those of magnesia, on account of their nearer approach to clay in composition. We prepare them by mixing equal weights of well-mixed Zettlitz kaolin and finely-ground calcined alumina, and calcining the compound in the hottest feldspar-porcelain fire of the Royal Berlin porcelain works, at the melting-point of Seger cone No. 30. The burnt mass is then crushed to pin-head size, an equal amount of crude Zettlitz kaolin is added, and the crucibles are moulded.

The arrangement of the furnace and the conduct of the experiments described in this paper have interested us greatly, and we shall not fail to call attention to them in the *Thonindustrie-Zeitung*.

Prof. HOFMAN: It is almost a year since the experiments described in our paper were made. During that time I have followed them up with an extended course of tests on the principal fire-clays of the United States, using both the indirect silica-lime method and the direct Seger method, with the kaolin-alumina crucibles referred to above. I am now trying to work out an indirect method for determining the fusibility of low-grade clays with the aid of Prof. Seger's apparatus for high-grade clays. The experiments have so far given very satisfactory results, and, when completed, the intention is to present them in a paper before the February meeting of the Institute in 1895.

ORE-WASHER AT LONGDALE, VIRGINIA.

Discussion of the Paper of Mr. Johnson. (See p. 34.)

(Virginia Beach Meeting, February, 1894.)

JOHN S. KENNEDY, Chambersburg, Pa. (communication to the Secretary): The washing-plant described by Mr. Johnson is a good illustration of the advantages derived from a well-designed and care-

not melt below the fusing-point of the Seger cone No. 35. We have abandoned the use of the crucibles formerly described by us, and consisting of magnesite and chromite, lined with alumina, because, notwithstanding the protecting layer of alumina, the experimental cones always showed, at high temperatures, coatings due to the material of the crucibles. The tests were mostly enameled or colored red with chromic oxide. The crucibles of material containing alumina which we now employ have shown, thus far, no influence upon the substances tested. They are so refractory that the Seger cone No. 36 (Rakonitz clay-slate) can be entirely melted down without the crucibles showing any sign of fusing."

The authors announce that they will furnish such crucibles, and the Deville furnaces used in the experiments, at cost.—R. W. R.

fully built ore-washer. In some instances that have come under my observation, companies have spared no expense to secure a modern furnace-plant, but the ore-washers were badly located, inadequate to the requirements of the furnace and crudely designed. Mr. Johnson calls attention to the failure of the cone-washer. This form of washer has the advantages of being easily moved and set up, and operated by small power. When an ore is composed largely of lump, or where the gangue is of a sandy nature, or as an adjunct to a washing-plant to give the ore a final rinsing, this form of washer is of value. But the single- or double-cylinder washer is the only one that has stood the test of time and practical experience. Where the ore is associated with clay, and where, as in many instances, the clay is of a greasy, tenacious character, locally known as "buck-fat," the cone-washer tends to "ball" the clay and carry it out with the ore. The cutting-action of the cylinder-blades and the friction of the pieces of ore, are necessary for clean washing.

Mr. Johnson's paper will re-open among mining men the old discussion as to the relative merits of the single- and the double-cylinder washer; and if this question can be settled, a vexatious controversy will be ended. In the single-cylinder washer, the ore is passed once under the cylinder and then propelled by the blades, while in the double-cylinder type, it is carried forward between the two cylinders.

There is an elasticity in the washer constructed with wooden box and cylinder, which is wanting in those made of cast-iron. When the ore is not crushed and sized, broken blades frequently result from too rigid construction.

The character of the ore regulates such questions as speed, elevation and water-supply. In general, a double log-washer should run at from 18 to 24 revolutions per minute, should have from three-quarters to one inch of elevation to every foot of length, and should be supplied with at least 150 gallons of water per minute.

As an instance showing that it is not every one who can build an ore-washer, I may cite a plant that I once saw, which was put up by a would-be engineer. The washer was unprovided with mud-ports, had only 8 inches elevation, and was geared to run 45 revolutions at the minimum engine-speed. It is needless to say that these conditions had to be changed before that plant became a success.

Mr. Johnson describes screen-plates in the waste-troughs, to retain any ore that may pass out through the mud-ports. Where the ore is mined from open-cut workings, roots and leaves are often carried

into the washers, clogging the mud-ports and screens. In such cases I use a vertical box or pocket, fastened to the side of the ore-washer and enclosing the mud-port. This box is about 2 feet long vertically, and 15 by 10 inches in section. The waste-trough, which is 8 by 8 inches in section, is taken out of this box about 4 inches from the bottom, thus forming a pocket which will retain any pieces of ore passing through the port. On the outside of this box, and above its water-level, is cut a door about 10 inches square, which is conveniently made of a piece of board, covered with $\frac{1}{8}$-inch sheet-gum, which acts as a hinge. Through this opening a bar can be introduced to loosen any obstruction in the mud-port, and any ore which is found in the pocket can be removed.

The universal joint, which allows freedom in elevation, is an improvement over the bevel-pinion in common use. The revolving screens I have found to be expensive in first cost and also in maintenance. It will be found cheaper to use a screen made from a flat perforated steel sheet, inclined at a small angle from the horizontal. It can be framed by any carpenter and quickly replaced when worn. It should be swung from two eye-bolts in the washer-heading and given a bumping-motion by a rod connecting it to a stud in one of the cylinder-journals.

The process of washing should include crushing and jigging. All lump-ore too large to pass through the hopper-grating should be crushed. A cleaner and more uniform product is obtained by crushing and washing the lump-ore, than by handling it separately. The sizing and jigging of the washed ore is necessary in many cases. It has occurred to me, that the fines could be concentrated in a vertical washer, similar to the Robinson coal-washer. The lighter particles of quartz and slate would be carried over what is now the coal-discharge, while the heavier pieces of ore would be saved in what is now the waste-pocket.

The amount of hygroscopic moisture after washing varies from 10 to 15 per cent., in addition to the combined water. This is a serious item where freight is taken into consideration. Moreover, the furnace-man is embarrassed by changes in the burden, caused by the continually varying amount of water in the washed ore. A dry ore would permit greater accuracy in burdening, and give increased economy in the furnace. When the furnaces are located near the mines, the washers are placed, in some instances, in the stock-house; and calcination should be cheaply effected in kilns by utilizing a portion of the furnace-gases. Care must be taken to avoid too high

a temperature, or the ore might be "louped." I would suggest, as a simple method of drying, that the ore be passed through a cast-iron trough by means of a spiral conveyor, the trough to be heated from below to a temperature sufficient to expel all the hygroscopic moisture, and probably some of the combined water. A cheap fuel, such as coke-braize, mixed with a small proportion of bituminous "slack," could be used in a step-grate, the combustion being assisted by means of forced draft from a steam-air jet.

Mr. Johnson: I have read with much interest the remarks of Mr. Kennedy, and while agreeing with him in the main, am compelled to dissent on one or two points.

Mr. Kennedy objects to the revolving screens and advocates the use of a single inclined perforated plate, with a bumping motion. This arrangement is not nearly so good, in my judgment, as the revolving screen, because it does not wash the ore so thoroughly. In speaking of cone-washers, Mr. Kennedy says: they are useful to give a final rinsing to the ore, after it has been passed through a log-washer. That is exactly what the revolving screen does.

In passing through these screens, the ore is turned over and over in a thin sheet, constantly exposed to a strong jet of water; but in the case of a perforated sheet with a bumping motion, there is much more sliding than rolling, and nothing like so thorough an exposure to the cleansing action of the water.

Nor is the first cost of revolving screens very great. Any blacksmith can put together a properly designed screen in a comparatively short time; and we find that they last exceedingly well, the average duration of service with us being one year, during which time some 15,000 tons of ore passes over each screen.

Mr. Kennedy's arrangement of screens for saving the ore carried out at the tail-end of the washer, does not seem to me as good as the one in use at Longdale. If I understand his plan correctly, each washer must have the box and screen described by him, the situation of which must necessarily be at the rear end of the cylinder, and near the driving-machinery. At this place, in our plan, one screen serves all four washers. It is built on the outside of the washer-frame, entirely away from the driving-machinery, and presents a large surface for screening and shoveling-over. It may be remarked here, that a large amount of bark and chips from the mine-timber, which finds its way into the ore in underground workings, is subsequently caught on this screen.

THE RE-WORKING OF ANTHRACITE CULM-BANKS.

Discussion of the Paper of Mr. Sheafer. (See p. 364.)

(Virginia Beach Meeting, February, 1894.)

In answer to inquiries from members, Mr. Sheafer said that the culm-banks of which his paper gave the shipments were of about the average quality of the banks in the Mahanoy region of the Schuylkill field, made prior to 1881 or 1882, when the practice of preparing buckwheat coal was introduced; but that in a very old bank, made about 1849, no coal above pea-size had been found. Apparently it was then the practice to ship everything.

In reply to an inquiry concerning the utilization of the fine coal-dust, Mr. Sheafer said that it was not utilized in connection with the washing described in his paper, but was carried away in the water as waste; that during Mr. Corbin's presidency the Reading Company built at Mahanoy City an extensive plant for making artificial fuel, but difficulty was encountered, due to the circumstance that the small coal from different collieries varied in its combustible quality and also in the amount of pitch required to be mixed with it, and he believed they concluded that from some varieties of the coal-dust a very good fuel could be made, but at a cost entirely too great for successful competition with cheap coal. Of course, the anthracite dust was not so well suited to this manufacture as bituminous slack.

R. W. Raymond, New York City: I believe that was the practical outcome of the enterprise of a member of the Institute, Mr. E. F. Loiseau, now deceased. It was found, I think, as an additional drawback, that the manufacture of artificial compressed fuel from anthracite dust could not be successfully practiced with material so impure as that of the culm-banks. Consequently, the manufacture could be based on that source of supply only when accompanied with a preparatory washing. The most favorable location for it would be at large yards where the waste from the handling of coal, which is comparatively pure material, could be thus treated.

I believe the manufacture of compressed fuel from bituminous coal was at one time a profitable business in Nova Scotia, but was destroyed by the indirect effect of the general introduction of water-gas for illuminating purposes. The result of that change on the

part of the gas-works was to make coal-tar so scarce and dear as to preclude its use in the manufacture of coal-dust briquettes.

We have heard of the process of filling old colliery-workings in the anthracite region with waste from the banks, which is carried into them by means of water, and I would ask Mr. Sheafer whether this material is the final waste from the washing of the banks which he has described.

Mr. Sheafer: It is not. In the instances known to me, the material thus washed into the colliery-workings has been the whole of the culm-bank, buckwheat-coal and all. We had one colliery, the workings of which are under the town of Shenandoah, that gave us continual trouble through the sinking of the surface and consequent suits for damages. The officials of the Reading Company united with the lessors to put down, with an ordinary drill, some ten bore-holes, 8 inches in diameter, and 300 or 400 feet deep, through which culm was sluiced into the mine. The vein was between 30 and 50 feet thick, and all the coal had been removed, except the pillars; some of the cavities were 70 feet high. We found that we could fill the space with culm and water, and then pump out the water, when the mass would become quite solid, and would support the roof so thoroughly that the remaining coal-pillars could be safely extracted. I think the method has been practiced in the Wilkes-Barre region also, where the material is forced down through pipes, and thus made to rise again for a certain distance, to fill the old workings. Whether it would set as well if there were no fragments of small coal in it, I do not know. The material left from culm-washing is mostly soft mud and slate, and might behave differently. Upon the culm, as we have used it for this purpose, the mine-water seems to act as a cement.

J. C. Platt, Waterford, N. Y.: With regard to the absence of coal from the old banks, to which Mr. Sheafer has referred, I think the explanation is that the breakers (or "crackers," as we used to call them) were not introduced for the preparation of coal until somewhere between 1850 and 1855. I remember that the first coal shipped from the Lackawanna basin for private use was of all sizes together, from the largest lumps down to perhaps buckwheat. There was no waste coal.

As to the "brick-fuel," I remember that the Delaware and Hudson Company undertook to make it at Rondout between 1875 and 1885, and found that it could be successfully used; but the introduction of the Wootten fire-box,* about 1876, rendered it possible to

* *Trans.*, v. 4.

burn the culm without making bricks of it; and I think that device had as much as anything else to do with the abandonment of the compressed fuel.

E. E. Russell Tratman, New York City: In Europe coal-briquettes are largely used for locomotive-fuel, more especially on the continent, and are made from bituminous coal-dust with a tar or pitch cementing-medium. In England, the Coal Brick Company controls the Lee patents for the manufacture of bricks from bituminous or anthracite coal-dust, coke, slack, etc., using a dry powder (composition apparently secret) to mix with the coal; the mixture being then made into a paste by the action of steam, and pressed into bricks. These bricks have been reported as showing excellent results in evaporative capacity, and as giving off but little smoke. English coal-briquettes are used to some extent in Mexico for locomotive-fuel. In this country, the Fuel Patents Company has been introducing what it calls "eggette" coal, made from anthracite or bituminous coal, or lignite, crushed to powder and mixed with a hot pitchy binding-material. The mass is then passed between rolls having pockets or indentations, and the finished product is in the shape of lumps about the size of hens' eggs. A plant with one set of rolls will turn out about ten tons an hour. It is not generally understood that anthracite is utilized in this process; but a plant in operation at Chicago uses a mixture of anthracite and bituminous coal. At Gayton, near Richmond, Va., a natural coke is used; at Huntington, Ark., a soft, friable bituminous coal; and at Denver, Colo., bituminous waste. These four "eggette" plants are now in operation.

TRANSMISSION OF POWER BY ELECTRICITY.

Discussion of the Paper of Mr. Leggett. (See p. 315.)

(Virginia Beach Meeting, February, 1894.)

Frederick H. Davis, Electrical Engineer of the Westinghouse Electric and Manufacturing Co., in charge of the plant at Bodie, Cal. (communication to the Secretary): I beg to offer, to complete the information conveyed in Mr. Leggett's paper, the following report of tests of the electrical efficiency of the power-transmission he has described:

Test on Generator.

	Amperes.	Volts.	Watts.
Self-excited field, current	15.8		
" " electro-motive force		60	
" " watts			948.
Separately-excited field, current	18.2		
" " electro-motive force		78	
" " watts			1419.6
Resistance of armature, 1.6618 ohms.			
C^2R loss in armature			664.72
Total loss in machine			3032.32
Load, current	20.		
" electro-motive force		3414	
" watts			68280.
Apparent electrical efficiency of generator 95.559 per cent.			

Test on Motor.

	Amperes.	Volts.	Watts.
Self-excited field, current	52		
" " electro-motive force		62.4	
" " watts			3244.8
Resistance of armature, 1.4 ohms.			
C^2R loss in armature			560.0
Total loss in machine			3804.8
Load, current	20		
" electro-motive force		3110.	
" watts			62200.
Apparent electrical efficiency of motor, 93.883 per cent.			

It will be readily seen that the foregoing figures deal simply with the apparent electrical efficiency of these two machines.

As there were no instruments available by which their mechanical losses could be determined, or by which the relative position of the current and electro-motive force phases of the load could be ascertained, these two conditions could only be judged of in a somewhat remote way.

As the generator is directly and rigidly coupled to the water-wheels, and has but two bearings, both of which are amply large, and run without heating under full load, it is fair to assume that the mechanical loss of power in this machine is very small.

The motor is running under equally favorable conditions, being connected directly to its work by a clutch.

The condition of the phase of the line-current was judged of in the following manner:

The apparent horse-power delivered to the motor, obtained from readings of the line-current and electro-motive force at the motor-station, almost exactly agrees with the mechanical horse-power required to drive the same load from an engine, the amount of work done being determined from indicator cards taken from the cylinder of the engine.

This close agreement between the work done by the engine and that apparently done by the motor on the same load, indicates that there is but very little shift or lag in the phase of the current delivered to the motor, and therefore the apparent efficiency of these machines is, for all practical purposes, the true electrical efficiency.

In these machines the mechanical losses must be very small, as there are no "belt pulls" to increase the friction, the bearings carrying nothing but the weight of the armatures. They are built to work at a full load of 120 kilowatts, and it will be noticed that this test was made at a load of but little more than one-half their rated capacity, and therefore a somewhat higher efficiency can be expected at full load.

Mr. Leggett: From the above careful tests made by Mr. Davis, it will be seen that the generator and motor attain a higher efficiency than that of 90 per cent. allowed in the table, showing the approximate efficiency of the transmission.

Taking the generator efficiency at 95.5 per cent., and that of the motor at 93.9 per cent., as determined, the final efficiency of the transmission, from the water-wheel shaft to the motor-pulley driving the mill, results as follows:

	Per cent.
In test No. 1, with 20 amperes of current, ...	77.7
" No. 2, " 21 " "	78.1
" No. 3, " 23 " "	79.0
" No. 4, " 25 " "	79.2

BAUXITE.

Discussion of the papers of Messrs. Laur and Hayes. (See pp. 234, 243.)

(Virginia Beach Meeting, February, 1894.)

A. E. Hunt, Pittsburgh, Pa.: I have heard these papers read with a great deal of pleasure. I am particularly interested, in a financial way, in the bauxites as ores of aluminum, and it is safe to prophesy that there will be many more mining engineers in the future who will be interested in these ores of aluminum, in the same way as many are

now particularly interested in the ores of iron; for certainly aluminum is commanding continually increasing attention as to its uses in the arts, and consequently as to its ores and metallurgy.

Mr. Laur's paper is specially valuable, because he is one of the earliest owners and miners of the bauxites. This mineral was practically unknown before the year 1870. Mr. Laur has done much in pushing the bauxites of the Departments of Bouches du Rhône, Var and Alpes Maritimes, France, in the same way as Messrs. George Blackwell and Alexander Sutherland have pushed the Irish bauxites upon the land of the Marquis of Downshire, in Strain, near Ballyclare, County Antrim, in Ireland.

I am glad that Mr. Laur says there are varieties of bauxite in this country which run low in both silica and iron. His theory some two years ago, when I first had the pleasure of meeting him, was, if I understood him correctly, that the only known varieties of bauxite had either a high percentage of ferric oxide (some 15 to 18 per cent.), and were red in color, or had the iron replaced largely by silica in the white, yellow and gray-colored minerals—that is, that the total iron-oxide and silica in bauxite was always at least 18 per cent. I note in his paper, however, the statement that he has found in France (what certainly exists in vast quantities in Georgia, Alabama and Arkansas, in this country) bauxite running very low in both silica and oxide of iron. There are hundreds of thousands of tons of bauxite in these three Southern states containing a total of less than 5 per cent. of silica and oxide of iron, and with about 4.5 per cent. of titanic acid. These ores will have very great commercial importance, not only in the manufacture of aluminum but also in the manufacture of alum and sulphate of alumina and as a basic (or neutral) refractory material in the form of bricks, furnace-linings, etc.

It is of special interest to note that these bauxites occur in the South comparatively near the localities of our sugar industries. In Great Britain the sulphate of alumina produced from bauxite is used to treat the sugar wastes, causing the separation of a large amount of sugar and converting the sulphate of alumina into potash-alum. This operation has been carried on with special profit abroad in the manufacture of beet-root sugar. The alum produced sells for a sufficiently higher price, as compared with the sulphate of alumina, to secure a considerable profit, and the recovery of sugar from the wastes is also of considerable amount.

In the manufacture of alumina, the pure anhydrous oxide of alu-

minum, from bauxites, the silica is the most difficult impurity to separate and leave a uniform product of good alumina. The red bauxite mentioned by Mr. Laur, coming from the Department of Var, has, I understand, proved so far the most uniform in its composition and the most economical for the manufacture of alumina, in France, where the silica and iron separated out can be sold to gas-works for "scrubbers."

Alumina is separated from bauxite by heating with soda-ash, forming aluminate of soda, which is separated by solution in hot water from the insoluble silica, oxide of iron and titanic acid. If the ore treated does not run uniform in silica and alumina and there is an excess of soda-ash added over the exact amount required to form aluminate of soda, or if the material be heated too hot, there is danger of the formation of the soluble silicate of soda, which contaminates the alumina in its after-precipitation with carbonic acid. But uniform and satisfactory ores for the manufacture of alumina are now shipped from Georgia and Alabama in large quantities to the chemical manufacturers of the North.

The importations of Irish and French bauxite during the four years ending with 1892 were as follows :

	Pounds.
1889,	28,945,647
1890,	27 503,730
1891,	17,936,500
1892,	12,804,253

The decrease in importations was met and supplied by three Southern states, Georgia producing and giving to the market, in 1892, 4,000,000 pounds, while Alabama for the same year gave 14,400,000 pounds, a total of 18,400,000 pounds supplied by two Southern states.

Another important use of the purer grade of bauxite is as a refractory material, for the lining of basic open-hearth or other furnaces where a siliceous brick is disadvantageous. Until the discovery of bauxite with only a small percentage of silica and oxide of iron, it was impossible to use the mineral to make a good refractory, for the high silica and oxide of iron combined to render it fusible when made into a brick; but many of the bauxites from Georgia and Alabama are well adapted to be used both in the linings of furnaces and in the manufacture of refractory brick. I exhibit with this statement a bauxite brick. It contains about 90.5 per cent. of alumina, about 5 of titanic acid, about 2 of silica, about 1 of oxide of iron and

1.5 to 2 of lime. The lime is added as a binding-material in the manufacture of the brick.

It is necessary, in the manufacture of these bricks, to have the bauxite very highly heated, to drive out all the water of hydration; and it is the present practice, after the bricks are made, to take out the shrinkage that there is in them by burning them in the hottest part of the brick-kiln. Then the material is re-ground and re-made into bauxite bricks, which are again very highly burned. The bricks thus made show very little further shrinkage.

As to the chemical composition of the various kinds of bauxites, I can give some information.

The interior portions of the pisolites are somewhat purer, on the average, than the exterior; the average for pure material being about as shown in the following analyses:

Interior.	Per cent.
Silica,	1.13
Sesquioxide of iron,	0.94
Exterior.	
Silica,	2.46
Sesquioxide of iron,	1.17

But while this is the case with many of them, some of the pisolites are homogeneous throughout.

The average bauxite of good quality from the Georgia district runs about as follows:

	Per cent.
Silica,	3.
Sesquioxide of iron,	1.5
Titanic acid,	4.50
Alumina,	58.67
Water of hydration,	32.33

The percentage of mechanical moisture varies according to the form of the material. The pisolites have not over one, or at most, 2 per cent. The softer portions of the bauxite material often run as high as 10 or 15 per cent. of mechanical moisture. The average rock-bauxite has from 1 to 3 per cent. of moisture in addition to the water of hydration.

In many of the purer samples, the silica and the oxide of iron fall below 1 per cent. each. In such cases, the combined water of hydration is increased by 1 or 2 per cent., and runs from 32 to 34 per cent.

Dana gives the composition of hydrates of alumina as follows:

$$\text{Diaspore, } Al_2O_3,H_2O = \left\{ \begin{array}{c} Al_2O_3, \text{ 85 per cent.} \\ H_2O, \text{ 15 per cent.} \end{array} \right\}$$

$$\text{Bauxite, essentially } Al_2O_3,2H_2O = \left\{ \begin{array}{c} Al_2O_3, \text{ 73.9 per cent.} \\ H_2O, \text{ 26.1 per cent.} \end{array} \right\}$$

Some analyses, however, give Al_2O_3,H_2O, like diaspore.

$$\text{Gibbsite, } Al_2O_3,3H_2O = \left\{ \begin{array}{c} Al_2O_3, \text{ 65.4 per cent.} \\ H_2O, \text{ 34.6 per cent.} \end{array} \right\}$$

The mineral gibbsite exists in the form of stalactites in many of the purer pockets of bauxite in the Georgia and Alabama deposits.

This gibbsite mineral as it occurs with the Georgia bauxites has about the following composition:

	Per cent.
Combined water,	33.00
Silica,	2.60
Sesquioxide of iron,	trace.
Titanic acid,	trace.
Alumina,	64.40

The water of hydration in the hydrate of alumina, as we buy it from the chemical concerns (as a triple hydrate, $Al_2O_3,3H_2O$) contains, when pure, 34.62 per cent. of water, with 65.38 per cent. of alumina.

When the water of hydration runs below 31 per cent. in the grayish and white-colored native mineral which we get from Georgia and Alabama, it is quite certain evidence to us that the percentage of silica is high.

A bauxite with only 29 per cent. of water will pretty certainly have a composition as follows:

	Per cent.
Water,	29.0
Silica,	5.5
Alumina,	60.0
Oxide of iron,	1.0
Titanic acid,	4.5

I append the analyses of several bauxites illustrating these points.

Combined water. Per cent.	Silica. Per cent.	Sesquioxide of iron. Per cent.	Titanic acid. Per cent.	Alumina. Per cent.
33.43	0.80	0.47	4.20	61.10
32.00	2.66	0.47	4.06	60.81
32.33	1.53	1.18	4.16	60.80
18.66	26.50	0.25	3.11	51.64
14.86	24.86	0.71	3.62	55.83
20.93	22.96	0.47	4.72	51.14
16.70	32.03	0.71	3.76	47.03
18.20	3.10	17.30	3.90	57.00

The fine material of bauxite usually contains more siliceous matter than the pisolites and other harder portions of the mineral It often runs 5 per cent. higher in silica than the harder portions. Often the fine material is interspersed with seams or beds of silicate of alumina (clay).

The method of analysis of bauxites, as used in commercial work by the Pittsburgh Testing Laboratory, Limited, chemists for the Pittsburgh Reduction Company, is as follows:*

Mix and fuse five-tenths of a gramme of very finely powdered bauxite with 8 grammes of powdered bisulphate of potassium. The fusion should be made in a thin-walled platinum crucible of about 400 cubic centimeters capacity; the cover of the crucible should fit well.

During the first fifteen minutes the crucible should be on a platinum wire triangle over a small flame of a Bunsen burner. The burner flame should be protected from drafts by a sheet-iron chimney, and the flame at first should just touch the crucible bottom. At intervals of five minutes remove the cover carefully and give the contents of the crucible a rotating motion, holding the crucible firmly in the tongs. At the end of fifteen minutes turn up the flame till the lower quarter of the crucible is red hot; agitate frequently as before.

In ten minutes more turn on flame full and heat for five minutes, with shaking. Cool, add 2 grammes more of bisulphate of potassium and gradually bring to a homogeneous fusion, but do not heat long enough to drive off the free sulphuric acid.

Pour out the liquid fusion into a warmed and dry platinum dish; the cake cools and does not adhere to the dish. Place together with the crucible and cover in a 200 cubic centimeter beaker. Add 150 cubic centimeters of water. Heat to 40° C., with frequent stirring, until all soluble matter is dissolved.

Silica.—Filter into two 300 cubic centimeter beakers and wash the residue. Ignite and weigh as silica. Make correction for silica if the bisulphate of potassium contained any. Also test the silica with hydrofluoric acid, and if any residue is found fuse it with a little bisulphate of potassium, dissolve in water and add it to the main solution.

The filtrate from the silica contains the titanic acid, alumina, and oxide of iron.

Titanic Acid.—Add dilute nitric acid to slight precipitation, not cleared by stirring. Add dilute (1 to 3) sulphuric acid until this precipitate just redissolves. Add four drops of concentrated sulphuric acid to the solution and dilute to 250 cubic centimeters. Saturate with sulphurous acid gas. Heat slowly to boiling, and boil gently for three-quarters of an hour. Add a little strong sulphurous acid water occasionally to keep the iron in ferrous state.

Filter through double filters and wash with hot water. Ignite and weigh titanic acid.

The filtrate is boiled until free from sulphurous acid; 2 cubic centimeters of concentrated hydrochloric acid and 2 cubic centimeters concentrated nitric acid are added and the solution boiled for fifteen minutes to thoroughly oxidize the iron. It is then diluted to 250 cubic centimeters with hot water and ammonia added in slight excess. Boil gently for five minutes and then warm for five minutes more. Long boiling gives a precipitate which retains potassium salts when washed. Filter and

* See *Mineral Resources of the United States for* 1892, Washington, 1893, p. 239.

wash thoroughly with hot water. Wash the precipitates off the filters back into the beakers, dissolve in 10 cubic centimeters concentrated hydrochloric acid and water, dilute to 250 cubic centimeters with hot water. Reprecipitate with ammonia as before. Filter on the same washed filters. Ignite (finally to highest heat of blast-lamp) and weigh as oxide of iron and alumina. Fuse with carbonate of soda, boil out with water, filter, and dissolve residue in hydrochloric acid. Titrate iron with bichromate and obtain alumina by difference from total weight of oxides of iron and alumina, calculating the contained aluminum from the oxide.

I would like to ask Dr. Hayes if he does not consider that the bauxite-deposits of Georgia and Alabama are "pockets," and that the ore occurs in these States in the same way as the manganese-ore does—altogether in large "pocket"-deposits.

Dr. Hayes: I consider them as pocket-deposits; but they are more definite in their outlines than the manganese-deposits. They are not disseminated throughout the clay, but are rather compactly segregated, though still in pockets.

There is one point in M. Laur's paper to which I would call attention, namely, the alleged deposition of bauxite at the present time in the Yellowstone National Park. I think that statement must be based upon an error. Mr. Weed, who has been at work for the Geological Survey upon the hot-springs deposits of the Park, informs me that the only compound of alumina which is deposited by the geysers is the silicate. It constitutes a chalky substance containing a considerable amount of free silica; but there is nothing which at all resembles the bauxites of the South. The pisolitic nuclei which are deposited are entirely siliceous.

R. W. Raymond, New York City: So far as negative evidence goes, I can corroborate Dr. Hayes's remarks, as in my own exploration of the Yellowstone Park, in 1871, while I observed siliceous pisolites in abundance, I noticed nothing like bauxite.

In connection with this subject, I think it appropriate to recall the circumstance that the attention of the Institute was first called to the Southern bauxites by our late fellow-member, beloved and lamented, Mr. Edward Nichols, whose paper on "An Aluminum Ore" (*Trans.*, xvi., 905), read at the Duluth Meeting, July, 1887, describes a deposit in Floyd county, Georgia.

THE DETERMINATION OF PHOSPHORUS IN COAL AND COKE.

Discussion of the Paper of Mr. Lychenheim. (See p. 66.)

(Virginia Beach Meeting, February, 1894.)

GEORGE L. NORRIS, Melrose, Mass. (communication to the Secretary): About a year ago I had occasion to make a large number of determinations of phosphorus in anthracite coal, and found the following method highly convenient and accurate. It resembles very much the one followed by Mr. Lychenheim.

Five grammes of powdered coal were weighed into a platinum milk-capsule, about two inches in diameter and one inch deep, and burned over a Bunsen burner. After the volatile matter had been driven off, the burning was hastened by a stream of oxygen. The ash was then moistened with water; about 25 c.c. of strong hydrochloric acid and 2 to 5 c.c. of hydrofluoric acid were added; and the mass was heated to solution. It was then transferred to a small flask, concentrated nitric acid was added, and the remaining hydrochloric and hydrofluoric acid were evaporated off; after which the determination was completed as usual in the Emmerton method. I found that the addition of a little phosphorus-free iron chloride or sulphate was useful as an indicator in neutralizing with ammonia before the precipitation with molybdate solution. My experience was, that, as Mr. Lychenheim says, most of the phosphorus is soluble in hydrochloric acid; but that by using hydrofluoric acid I was sure of getting all the phosphorus, and had no troublesome residue in my solution.

IRON-ORES OF EAST TEXAS.

Postscript to the Paper of Mr. Kennedy. (See p. 258.)

(Virginia Beach Meeting, February, 1894.)

SINCE this paper was written, some changes have taken place, materially reducing the cost of iron-making as estimated in the text. These are chiefly reductions in the prices of ore and fuel. The charcoal used is now made in bee-hive ovens, and the price is con-

sequently 4 cents instead of 6 cents per bushel, which alone effects a saving of over $2 per ton of iron. The cost of the ore also has been slightly reduced, so that the estimate for a ton of iron would be $9.50 instead of $12.01, as given in the paper. This expense would be distributed as follows:

2¼ tons of ore,	$2 05
110 bushels of charcoal,	4 40
½ ton of limestone,	75
Labor and salaries,	1 50
Interest,	30
Repairs and incidentals,	50
	$9 50

Our chief trouble now is transportation; and that, I think, will soon be remedied, or at least greatly ameliorated, so far as the New Birmingham district is concerned. The State is building an outlet to Palestine, and we have now under consideration a new road, for which plans, profiles, and other details have been partially prepared, and the right of way has been secured, and which will traverse the whole region from Alexandria, La., to Fort Worth, Tex., passing through New Birmingham and Palestine. This road, when finished, will give us, to the east, an outlet on the Red river, which is navigable; and on the west it will connect us with the great railway systems of Texas. The freight-charge on fluxes, as well as on pig-iron, will be materially lessened.

Experiments to test the coking-qualities of the Thurber coal are in progress. In the event of a successful result, we may be able to obtain coke from these mines at a reasonable rate.

THE TORSIONAL THEORY OF JOINTS.

Discussion of the Paper of Mr. Becker. (See p. 130.)

(Virginia Beach Meeting, February, 1894.)

PRESIDENT HOWE: It is, of course, not easy to discuss off-hand the paper which Mr. Becker has presented with so much lucidity. I will only make one remark, which is outside of the line of his argument, and concerns merely a passing allusion in his paper. Mr. Becker speaks of the fact that glass breaks with a flat or conchoidal fracture, and that steel shows a rough or "cupped" surface of frac-

ture. This difference we would naturally ascribe to the great difference in the other physical properties of the two substances. Yet I have seen copper-steel which did break in a perfectly flat fracture, or apparently so. It seemed to be accurately perpendicular; a smooth, mirror-like fracture, such as mineralogists called "splendent." Yet the physical properties of this steel did not differ from those of common steel, or resemble those of glass in the way which this fracture suggests.

R. W. Raymond, New York City: The general conclusions reached by Mr. Becker seem to me to be fairly demonstrated. His paper does not contain any precise definition of what we call joints; and possibly such a definition ought to be offered as the basis of any complete theory of the subject. Without venturing to supply such a definition here, I infer from Mr. Becker's paper, and in accordance with our general usage of the term, that joints are to be distinguished on the one hand, from cleavage-planes, which we may, perhaps, consider as potential rather than actual partings, and, on the other hand, from fissures on a larger scale, such as become the receptacles of mineral veins. And I understand his view to be, that jointing is neither produced by tensile stress nor by pure torsion, while direct, more or less oblique, pressure will account for the observed phenomena. At the same time, he recognizes the inevitable complexity of the stresses involved.

This conclusion may have an important bearing on the subject of larger rock-faults. At the Washington meeting of February, 1882, I presented a paper on "Hoefer's Method of Determining Faults in Mineral Veins," based upon the author's essay,* which appeared in volume xxix. of the Austrian *Zeitschrift*. The peculiarity of Prof. Hoefer's method is, that it provides for the case of a relative movement of the two walls of a fault which is not rectilinear, but to some extent rotatory; that is, in which a partial revolution of the mass on one side of the fault has taken place around an axis normal to the plane of the faulting-fissure. The evidence of such a motion is found in changes of dip and strike, produced by the fault, in the fissure faulted. I wrote to Prof. Hoefer at that time, telling him that this phenomenon had not been recognized in our mining districts to any great extent, and asking whether such movements, distinct from rectilinear ones, had been frequently observed by him. His reply was:

* *Trans.*, x., 456.

"Circular movements, combined with movements in straight lines, are very frequent in our faults; in fact, I do not doubt at all that a continued careful study will show them to be the rule. Whether simple revolutions often occur, is very difficult to decide from the observations thus far available." *

It is my present belief, that the circular movements have been, as a rule, not only accompanied by straight-line movements, but that the latter have been predominant; and it seems to me most reasonable to suppose that rotation has been caused, not by the direction of the forces which caused rupture, but by the resistance encountered in the course of a rectilinear slide. It is, indeed, almost inevitable that such a movement would not be wholly rectilinear, but that the mass would be turned more or less around points of greater friction. In other words, the seeming effects of torsion might be produced after the occurrence of a rupture in which torsion had taken no part.

In this view the causes of rupture on the large scale would be entirely analogous to those observed on the small scale in joints. This is a familiar principle to those who have worked upon rocks with the microscope, or have studied in hand-specimens the features exhibited in rock-masses. We find in all sizes the phenomena of faults, contortions, cavities, segregations, etc.; and we are almost irresistibly led to the conclusion that the microscopic is but the image of the macroscopic, and *vice versa*. But, in accordance with this principle, it may be (indeed, it ought to be) true that the small ruptures are sometimes produced, as are larger faults, by tensile stress. In fact, I think such minute tension-ruptures are frequently to be observed, but they do not generally, if, indeed, they ever do, exhibit the smoothness and regularity of joints.

Mr. Becker: I may remark that faults supposed to be produced by torsion are said to be so common in certain French mining districts that the miners have a special name for them. They call them hinge-faults, which is, I think, a very good name.

The term joint I regard as used simply for convenience to designate those partings in rocks on which the throw is not apparent without close observation. Excepting in the amount of throw, they are not distinguishable from the paraclastic ruptures on which mineral veins form. Even on joints ores sometimes occur, *e.g.*, the "paints" of the quicksilver mines. Joints also pass over into cleavage. This structure sometimes consists of closely grouped joints of microscopic throw, and sometimes of mere deformation not

* For a single and uncertain instance of possibly simple circular movement, adduced by Prof. Hoefer, see my paper above cited, *Trans.*, x., 463.

carried quite far enough to induce rupture. Cleavage and faulting are equally orogenic disturbances.

C. R. Boyd, Wytheville, Va.: In examining a silver-mining property at Conrad Hill, six miles southeast of Lexington, Davidson county, North Carolina, several years ago, I found two series of fissures crossing each other, one set being mainly filled with carbonate of iron and magnesia, the other with iron and copper pyrites, carrying gold and silver. The individual fissures of each series were not more than 15 or 20 feet apart. The carbonate-veins had a north and south strike, and a dip of 30° west. The pyrites veins ran east of north and west of south, dipping 40° west of north. The angle between the two series was about the same as that which the mountain ranges of that section make with the true meridian. That both series could have resulted from simple tangential strains or thrusts would be difficult to prove, and that they could both be fissures of contraction I hardly believe. Possibly torsion may have caused them; but it is more probable, in my judgment, that they resulted from disturbances proceeding from separate *foci*, and not necessarily simultaneous.

Mr. Becker: The angle depends on the amount of distortion which has preceded rupture. If a substance is brittle, like glass or cast-iron, and yields to rupture before it has been deformed to any considerable extent, the fissures will cross at right-angles. If, on the other hand, the character of the mass or its confined position (as is often the case with rocks) prevents rupture from occurring until deformation has reached an extreme limit, the angle between the direction of the force and that of fracture may be even 50 or 60 degrees, and large angles seem always to mean great preliminary deformation.

The most usual conditions of rock fracture involve the simultaneous formation of two sets of fissures; but when the resistances in all directions perpendicular to the line of force are substantially uniform, four sets of rupture may form, each being at 45° or more to the line of force, and all four sets will be slickensided. A single system of parallel faults involves the action of a "rotational" stress (which is, of course, utterly different from a torsional stress); and a solitary fault arises as an extreme, and in my experience rare, case of a rotational stress. I am inclined to believe that when two or three or four systems of joints or fissures intersect a rock mass they were, as a rule, formed simultaneously. When a rock is once shattered a fresh force meets, in general, with very unequal resistance in differ-

ent directions, and will cause disturbance on the old fissures or brecciate the whole mass rather than induce a new, regular system of intersecting partings.

THE INACCURACY OF THE COMMERCIAL ASSAY FOR SILVER.

Discussion of the paper of Mr. Stetefeldt. (See p. 530.)

(Bridgeport Meeting, October, 1894.)

Prof. H. O. Hofman, Massachusetts Institute of Technology, Boston, Mass. (communication to the Secretary): When Mr. Stetefeldt quotes me as saying that "silver-assays are uniformly made by scorification," he has evidently not understood that I was referring solely to the practice of silver-lead smelting-works. Everybody knows that silver-*mills* do not employ the scorification-assay for regular work; and it is equally certain that in very few, if any, silver-lead smelting-works will the crucible-assay be found in use for determining silver. The second part of the quotation, "as it is adapted to all kinds of ores and gives excellent results unless organic matter is present," although, of course, also intended in a restricted sense, might very well be extended to a general one. Mr. Furman, in his *Manual of Practical Assaying*,* says, on page 123: "We believe that the Colorado practice is preferable, as most ores will yield higher silver-results by scorification." The fact that the scorification-assay is the accepted one in Colorado is strong presumptive evidence for its accuracy, as exactness would nowhere else be more jealously demanded.

Mr. Stetefeldt cites Albert Arents† as a supporter of his preference for the crucible-assay, although I had already done so in favor of scorification. Looking up again the reference, I find that I was justified in quoting him, as, while Mr. Arents advocates‡ the crucible-assay for low-grade gold ores, not on account of the greater accuracy of its results, but because it is quicker and less expensive, he condemns it§ for the assay of silver-ores, and prefers the scorifier for rich gold-ores, and for coppery ores, and in general for ores containing both silver and gold. As I, like Mr. Stetefeldt, have kept no detailed

* John Wiley & Sons, New York, 1893.

† *Berg. und Hüttenmännische Zeitung*, 1867, p. 85.

‡ *Loc. cit.*, column 1.

§ *Op. cit.*, p. 86, column 2, foot-note.

record of my own experience, I may quote such authorities as Fournet,* Richter and Hübner,† and Görz,‡ in support of my view.

Mr. Stetefeldt maintains that dry silver-ores containing antimonial and arsenical sulphides ought always to be assayed by the crucible-method rather than by scorification. But in the crucible, especially with arsenical ores, we are very liable to obtain a speise-button; and while this does not, as a rule, run high in silver, we are not justified in neglecting it. We have to make a separate assay to recover the precious metal it may contain, and cannot but have considerable loss whichever way we may attack it. True, the bulk of the silver of the ore has been collected in the lead-button; but will the silver recovered by crucible-work on ore, combined with that obtained from the speise by any kind of work, be greater in amount than that which the scorification-assay yields? It does not seem probable.

Tables I. and II., given by Mr. Stetefeldt, put the crucible-assay in a very favorable light, but would have been more convincing if they had shown in detail that the losses were not sufficiently large to detract materially from the results. In Table III., however, the silver recovered from the cupel is given separately from that of the slag; and there I find that the amount absorbed by the cupel is excessive. At the government silver-lead smelting- and refining-works of Lautenthal§, Prussia, this loss was ascertained in my laboratory with comparatively pure low-grade base bullion, containing 99.4 per cent. lead and 43.75 ounces silver per ton, to be 3.5 per cent. of the silver. In Table III. it is seen to range from 6 to 22 per cent. As the assays of Tables I. and III. were made in the same mills, A and B, and Mr. Stetefeldt himself does not claim that their assayer is an expert in the scorification-method, while cupelling crucible-buttons belongs to his regular work, is it not possible that the difference of 2.3 ounces in favor of the crucible may be due to his inexperience?

It is to be hoped that the figures presented by Mr. Stetefeldt may induce some of our silver-lead smelters to take up the question of scorification- *versus* crucible assay, and let the public have the benefit of their work.

Mr. Stetefeldt (communication to the Secretary): In quoting

* *Berg. und Hüttenmännische Zeitung*, 1867, pp. 102, 122.

† *Zeitschr. für das B., H., u. Salinen-Wesen in dem Preussischen Staate*, 1873, vol. xxi., Abthl. B, p. 118, and 1876, vol. xxiv., Abthl. B, p. 497.

‡ *Berg. und Hüttenmännische Zeitung*, 1886, p. 441.

§ *Zeitschr. für das B., H., u. Salinen-Wesen*, 1880, vol. xxviii., B, p. 295.

the article by Mr. Arents in the *Berg. und Hüttenmännische Zeitung*, in favor of the crucible-assay *versus* scorification, I must say that I have not read the same, because I failed to obtain a copy of the *Zeitung*. My information was derived from conversation with Mr. Arents himself. Addressing him recently on the subject, I received the following reply:

"In answer to your communication of October 17th, I will say that my article in the *Berg- und Hüttenmännische Zeitung*, referred to by you and Prof. Hofman, was primarily intended to recommend to my colleagues in Germany the use of the crucible-assay in certain instances in place of scorification.

"While I would consider it labor lost to test, for example, the galenas of the Hartz for their silver by any other method but scorification, I did consider it folly not to use the crucible-assay, but persist in endless repetitions of unsatisfactory scorification tests, for the determination of the precious metals of the ruby silver-ores of Andreasberg and of certain foreign dry ores with little gold and some rich silver minerals, then frequently bought and treated in Germaany.

"My opinion has not suffered any change since. Docile ores and products which upon heating do not decrepitate or sputter, which are of uniform composition and not of too low grade, are eminently fit to be tested by scorification with attending commercially correct results; but where these conditions do not prevail, where loss by effervescence or volatilization will take place in heating, where a large quantity of material will represent a more accurate sample or a more satisfactory quantity of precious metal for weighing purposes, then the crucible assay is preferable to scorification on account of its more reliable results."

Mr. Arents also agrees with me that the general acceptance of the scorification-assay in the smelting-works of Colorado is *not* "strong presumptive evidence for its accuracy." The smelter's business is to make money, not accurate assays. His principal profit in buying dry silver-ores is derived, not from the low charges for reduction, but from the difference between the actual contents of precious metals in the ores and that shown by the commercial scorification-assay.

I give on the following page a supplement to Table I., as suggested by Prof. Hofman. For Table II., I have no such detailed statement on hand; and since in this case the commercial and corrected assays by the two methods differ only slightly, it would not be of much interest.

From the fact that in Table I. the corrected crucible-assay is in most cases considerably higher than the corresponding scorification-assay, we may draw the conclusion that in scorification the loss of silver by volatilization was greater; and this, I claim, is always the case with dry silver-ores containing arsenic and antimony.

SUPPLEMENT TO TABLE I.—*Silver in Slag and Cupel.*

Description of Ore.	Method of Assaying.	Silver in Slag.	Silver in Cupel.
		Ounces.	Ounces.
Raw Ore, No. 1	Scorification.	0.5	1.5
	Crucible.	0.9	1.6
Raw Ore, No. 2	Scorification.	0.6	1.3
	Crucible.	0.9	1.5
Raw Ore, No. 3	Scorification.	0.8	0.8
	Crucible.	0.9	1.0
Mixture of Raw Ore, Nos. 1, 2 and 3	Scorification.	0.5	4.8
	Crucible.	1.8	3.6
Mixture of Roasted Ore, Nos. 1, 2 and 3	Scorification.	0.4	4.8
	Crucible.	0.1	4.0

NOTE.—Why so much more silver was absorbed by the cupel in assaying the mixtures of raw and roasted ore than in the separate assays of raw ores Nos. 1, 2, and 3, I cannot explain; I simply give the results as handed to me.

The percentage of arsenic in dry silver-ores is generally so slight that there is no danger of obtaining a speise-button in the crucible-assay, as Prof. Hofman suggests; in fact, during my long practice of assaying such ores at Austin, Nev., I have never observed this to occur if sufficient litharge was used in the flux.

PROF. H. O. HOFMAN, Boston, Mass. (communication to the Secretary): The losses that occur in assaying gold- and silver-bearing materials are now being investigated by practical assayers, and the final results will be made public in due time. Meanwhile, it seems rather unprofitable for those not directly engaged in the work to continue the discussion further, unless they can furnish data from their own previous experience which may help to clear up the matter. I have no such data in my possession.

Mr. Stetefeldt says that "the smelter's business is to make money, not accurate assays," and that therefore the general acceptance of the scorification-assay by Colorado smelters is not strong presumptive evidence of its accuracy, as I had claimed. Mr. Arents is also quoted by Mr. Stetefeldt as holding the same opinion. This may have a plausible sound, but will scarcely bear closer investigation. Take, for instance, Denver and Pueblo, with their smelting- and sampling-works. They contain five of the largest silver-lead smelting-works

of the country, competing for the same class of ores. If crucible-work were so superior to that by scorification, would not all assays be made in the crucible, that the superintendents might know accurately how much silver and gold they were buying, whatever they might be inclined to deduct for the supposed inferiority of the scorification-assay? But such is not the case. Any attempt to take unfair advantage of the miner by means of the lower scorification result could not succeed, as the sampling-works stand in the way, whose business is to do the best for their clients, the miners. They would demand the crucible-assay for silver (as they do to-day for gold) if its results were really so much higher. But we see the scorifier accepted as the standard by both parties, and I submit that this fact *is* "strong presumptive evidence for its accuracy."

H. Van F. Furman, Denver, Colo. (communication to the Secretary): Mr. Stetefeldt's statement that "the smelter's business is to make money, not accurate assays," is somewhat misleading. That the smelter's business is to make money, none will deny; but that accurate assays, at least within certain rather narrow limits, are a necessity to the smelter, is well known to any one who has been engaged in the business, and should be apparent to all.

As I have stated elsewhere (*Manual of Practical Assaying*, p. 123), the scorification-assay has been quite generally adopted in Colorado for the determination of silver, for the reason that it apparently gives *higher* and *more uniform* results on the majority of Colorado ores. For the determination of gold, the crucible-method is now quite generally adopted in Colorado.

I can hardly agree with Mr. Stetefeldt that the smelter's principal profit on dry silver-ores is derived from "the difference between the actual contents of precious metals in the ores and that shown by the commercial scorification-assay." That there is a loss of silver in the assay is well known to all smelters. That there is a greater loss in smelting is also well known. The fact that our smelters only pay for 95 per cent. of the silver, as shown by commercial assay, is an evidence. Were it possible to determine the actual amounts of silver present by commercial methods, I have no doubt that our smelters would gladly adopt such methods, but of course accompanied with a higher deduction for loss in smelting.

Whatever the former practice may have been, it is not usual at present in Colorado to sell ore exclusively on the smelters' assays. The practice for a number of years past has been to check the smelters' assay with another, made by an outside assayer. In case

of difference, a third sample is sent to some outside assayer, who is selected as an umpire, and his assay is regarded as final.

In buying ores, the object generally aimed at is to pay for such amounts of gold and silver as will finally be obtained after the ores are smelted and the bullion refined. The precious-metal value of the ore is usually calculated on this basis, and a deduction is made from the value thus obtained for profit and cost of treatment.*

A UNIFORM METHOD FOR THE ASSAY OF COPPER-MATERIALS FOR GOLD AND SILVER.

Discussion of the Paper of Dr. Ledoux. (See p. 575).

(Bridgeport Meeting, October, 1894.)

R. W. Raymond, New York City: As was announced in the Secretary's note, appended to the pamphlet edition of Dr. Ledoux's paper, I have undertaken to superintend the preparation of the samples required by his plan, and the collation for publication of the results reported. Promises of cordial co-operation have been received already from a number of leading assayers;† and it is to be expected that the data thus collected, and the discussion to be based upon them, will shed much light upon the important question raised in this paper. Although a detailed discussion of methods on the present occasion might be deemed premature, I am sure that suggestions on the subject from members engaged in assaying would be timely and valuable as a preliminary to later and more detailed criticism.

Herbert G. Torrey (U. S. Assay Office), New York City: I may say in relation to the losses in the furnace that I have made a number of experiments, and on my little blank form years ago I put down a place for the losses in the cupel. It met with a good deal of opposition among the smelters, but I claimed that it was my duty to give the full amount of the gold and silver present, and that it

* See article by the writer on "Purchasing Silver-, Gold- and Lead-Ores," *School of Mines Quarterly*, vol. xv., No. 1.

† The number was subsequently increased to over 20, and the samples were distributed in accordance with the plan of Dr. Ledoux. It is hoped that the results can be received, printed and distributed in time for thorough discussion at the next meeting of the Institute.

was their duty to make the commercial allowance. This was accepted by one leading smelter. Many times I allowed from 20 up to as high as 50 ounces on the assay for the loss in the muffle; and the fact that the smelter allowed it showed that it must have been correct. The question came up, I think, in connection with comparative assays with Mr. Pitkin on some very close work that we were doing together; and my attention was drawn to the loss of gold, not so much in the muffle as in the manipulation of decanting and washing. I think I have also had comparative experiments with the Balbach establishment on the same head. I think this is a question all assayers should look into very carefully. After the work is all done in the furnace and in the cupellation, losses may occur from a little carelessness in washing away some particles of the gold at the time of parting. I think that is a place where we are most apt to make mistakes. The time that the matrass is allowed to stand, and the adroitness with which the decantation is performed, may make a difference of 10 per cent. without our knowing it. I think that point might be included in this experiment with great advantage, namely, the exact method which should be followed in recovering very small particles of gold from the nitric acid in the matrass and the best method to be used in decanting.

E. D. PETERS, JR., Dorchester, Mass.: I should like to ask Dr. Ledoux if he could suggest the form of a pyrometer for measuring the temperature of a muffle during assaying. It has been a good many years since I have had occasion to do any assaying myself. In the West that used to be a great trouble. In present practice there is probably very little loss by volatilization in a commercial assay, because the temperature is so carefully watched, especially in the West, where the assayers of works deal with numerous samples at once. But I do not see why a simple form of pyrometer might not be used. I suppose it would be practicable to have something like a thermostatic bar that would automatically turn the gas off and on, and I would ask Dr. Ledoux if he knows of anything in that line.

MR. TORREY: I meant to say, in mentioning the deduction for losses in the cupel, that they were based upon numerous experiments to determine such losses. By making a comparative assay and finding about what the material contained, and then making up a proof and putting it between the duplicate samples, and then allowing for the losses in the cupel, I found that I could get very nearly the proper results. In answer to Dr. Peters's question regarding a pyrometer, I would say that Mr. Charles Taylor, one of my assistants

has invented and put in operation a pyrometer that has worked admirably. It turns off the gas automatically; a platinum rod going entirely through the muffle and being attached to a little screw, which is regulated in the morning with a very delicate thread, to take up any error of the previous day, and set the pyrometer at a given point which will stand for one day. During that day the heat of the furnace can be regulated at will; and the next morning the rod is set again by a slight variation of the screw. A uniform heat can thus be held for the day. This regulator has been in use for several years at the assay office, and it has worked very nicely. It was described in the *Engineering and Mining Journal*, August 28, 1886.

H. Van F. Furman, Denver, Colo.: I have read Dr. Ledoux's paper with considerable interest, and consider his suggestions as a step in the right direction. I shall take pleasure in becoming one of the many who will doubtless volunteer to test the samples.

Our iron and steel chemists have done much valuable work towards the adoption of standard methods of analysis, using the *Transactions* as a medium for their discussions. It would appear that considerable benefit might result if our gold and silver assayers would follow in their footsteps. As I have pointed out in my paper on "Losses of Gold and Silver in the Fire-Assay," presented at this meeting (*ante*. p. 735), there is too great a lack of uniformity in the methods for assaying gold and silver. Dr. Ledoux has mentioned the difference in methods of assaying copper products as adopted in the East and West. The same is true in different sections of the country as regards ore-assaying.

On the Pacific coast, the crucible-method is almost universally employed for both gold and silver. In Colorado, the scorification-method is almost universally employed for the determination of silver, while both methods are used for gold. Of late the crucible-method has come largely into vogue, and with good reason. Several years' experience in the West, as assayer and buyer and seller of ores and metallurgical products, has satisfied me that there is no disposition in this country, as there is elsewhere, according to Dr. Ledoux, to demand either *high* or *low* assays. What is required is an assay which represents the actual amount of gold and silver in the sample within reasonably close commercial limits.

Lucius Pitkin, New York City (communication to the Secretary): The question considered in Dr. Ledoux's paper is one of much interest and importance, not only as affecting assay-returns in gold-

and silver-ores proper, but also in that important group of copper-products in which gold or silver, or both, are present.

F. P. Dewey has published, in the *Journal of the American Chemical Society*, an interesting table of comparative figures as between "commercial assay" and the assay corrected by the appropriate amount for slag-loss and cupel-absorption. A glance at these figures (obtained on silver-ores and concentrates) shows the wide difference in values resulting from such a correction.

The experience of my own laboratory, in copper-silver products, bears out the figures already adduced. A bar of copper, carrying 91.62 ounces of silver by "commercial assay," was raised to 93.22 ounces by correction for slag- and cupel-absorption. Similarly, a copper matte carrying 181.73 ounces of silver by "commercial assay," was raised to 183.77 ounces by the corrections. On high-grade material, a 5500-ounce concentrate gave a slag- and cupel-recovery of 103.20 ounces. A recovery of 141 ounces was effected on 7500 ounces of material.

In view of such figures, which themselves do not take into account volatilization-losses, it is certainly proper that some standard agreement, as to what the "commercial assay" shall, or shall not, include, be definitely formulated. Concurrence among those to whom these interests are chiefly confided, is becoming daily more necessary; and such work in common, as may be necessary to establish standard processes and conditions, should, and probably will, be heartily engaged in.

Charles Kirchhoff, New York City (communication to the Secretary): The following appraiser's decision, just published, emphasizes the importance of the adoption of standard methods for determining the metallic contents of ores:

"Before the U. S. General Appraisers, at New York, September 10, 1894.

"In the matter of the protest 22,454 *b*-3491, of F. A. Hartman, against the decision of the collector of customs at San Francisco, as to the rate and amount of duties chargeable on certain silver-ore containing lead, process used in making assay, imported per *St. Paul*, December 20, 1893.—Opinion by Wilkinson, *General Appraiser*.

"The merchandise is silver-ore containing lead. It was assessed with duty upon assays made by the 'dry process,' and is claimed to be dutiable according to assay by the 'wet process.'

"The decision of the collector appears to have been based on Synopsis 10,037. This ruling, as well as that in G. A. 1992, related, however, to the ascertainment of the copper content.

"The common commercial and correct test for lead is by the 'wet process,' and we sustain the claim that this is the proper mode of assay.

"For purposes of comparison, the appraiser made an assay by the wet test, showing 20.13 and 19.8 lead, respectively, for the two lots. In the absence of evidence to controvert these figures, the collector is instructed to use them as a basis for the reliquidation of the entry."

This official declaration that the "common commercial and correct test for lead is by the wet process," will, I fancy, prove a surprise to American assayers generally. I have been laboring all along under the impression that the fire-assay was the commercial standard for lead.

DR. LEDOUX: With regard to Dr. Peters's inquiry, I would say that, of course, a pyrometer could hardly be applicable in a furnace where coal or coke was used as a fuel, as it would be impossible to alter the temperature quickly; but there would be no difficulty at all, in connection with a gas-furnace, in having the gas turned on or off automatically according as the temperature fell below or rose above a certain point. Even then, however, the assayer having more than one cupel in the furnace would find a different temperature at the mouth of the muffle from that existing further back.

With regard to the samples to be distributed for the comparison that I have proposed, I may say that so far as the mattes are concerned, I do not think any assayer need fear that there will be any variation among them, because they can be ground very fine and intimately mixed. But when it comes to bar-copper, carrying large amounts of gold and silver, there is not only great variation among the pigs and in the individual borings, but among the different parts of the same pig. Dr. Torrey knows how difficult such sampling is in his experience, and I must say that it is almost enough to make a man turn gray when a buyer, in Europe, accepts 100 tons of gold and silver-bearing bars in New York and puts himself absolutely in the hands of the American assayer, and the seller in the West does the same thing, and the assayer finds that he gets different results by boring in different parts of the same bar. Assayers have arrived at a certain agreement as to where the samples shall be bored, so as to get a fair average of the silver contained in lead bullion, but there is no such rule with regard to similar copper bars. Sometimes it is difficult to take fair samples by dipping. Of course, you can get from a molten mass, well-stirred or in action, a fair average sample by having a certain number of dipped samples; but, unfortunately, dipped samples often run too high, and the assay shows more gold and silver than the refiner can get out of the material. One reason for this is the fact that when we dip into the

molten mass and fill the ladle full, the material carrying the dross runs over the sides, and the result is that we are getting a cleaner metal for our assay than the product will give when it is bored. I have found a difference of two per cent. between the dipped sample and the best drilled samples. In the preparation of these drilled samples which Dr. Raymond will send out, we shall bore them as small as possible and mix them as well as possible, but even then there will probably be certain variations.

I want to add one more word, and that is, that I hope not only the assayers, but also those who employ them, will take an interest in this matter. I recall an incident in my early experience which will show how necessary it is for public assayers to have the hearty co-operation of their employers. We were representing some parties selling very rich silver ore to a certain smelter, and we sampled the material jointly with the smelter and assayed it, but in no instance did we get as much within five ounces per ton as the smelter himself returned, although we used every precaution. I was exceedingly annoyed about it and very much worried. One day, while I was talking to the smelter, he said, in a burst of confidence: "You need not be worried about that at all. I will tell you in confidence, that we add five ounces of silver to the assay because we do not like to have you public assayers coming into our works and bothering us, and therefore we add five ounces of silver to the assay. We are getting from $20 to $30 a ton for refining this material, and can very well afford to pay for four or five ounces of silver extra rather than encourage the representatives of the shippers to come loafing around our works." Of course, this statement being given to me in confidence, I could not report what the trouble was, and I could not conscientiously add five ounces of silver to my assay report so as to equal the smelter's assay; and the result was we lost a job.

EARLY DAYS OF THE IRON MANUFACTURE.

Remarks upon the Presidential Address of Mr. Fritz. (See p. 594.)

(Bridgeport Meeting, October, 1894.)

E. C. Pechin, Cleveland, O.: Will you indulge me a moment, Mr. President, to narrate an incident which is recalled by your admirable address?

For many years Mr. Hughes Oliphant operated a small charcoal-furnace and mill at what is now Fairchance, in Fayette county, Pa., at the foot of the Allegheny mountains.

He told me that early in the century he ran for eighteen months, and in that time saw only $10 in money. I said to him: "How under heaven did you manage?" He replied: "We made our iron into nails, rods, and kettles; hauled them 12 miles over to Brownsville on the Monongahela river; loaded them into flat-boats; and floated them down the Ohio, swapping our wares for whiskey and rum. At New Orleans we exchanged these for sugar and molasses, which we sent by sea to Baltimore, and there we swapped again for groceries and dry goods, which we hauled in Conestoga wagons over the mountains, 300 miles, to our furnace."

In these days this would appear to be making iron under difficulties. It is a noteworthy fact that in those good times there was "free and unlimited" coinage of silver dollars.

ALUMINUM-BRONZE.

Discussion of the Paper of Dr. Waldo. (See p. 525.)

(Virginia Beach Meeting, February, 1894.)

President Howe: It is not so clear to me that the facts which Dr. Waldo brings forward really argue that the nature of the combination between copper and aluminum differs from that of the combination between copper and zinc or copper and tin.

As regards electric conductivity, I suppose that the fact, which he thinks points to a special intimate kind of union between aluminum and copper, is that the line in his Fig. 1 is neither straight nor uniformly curved, but that its curvature varies rapidly, not to say abruptly. But we find just such variation in like curves for the copper-zinc and copper-tin alloys. Thus, a slight addition of zinc or tin to copper causes a change in color wholly out of proportion to the quantity added; and, as we add zinc or tin to copper, the tenacity rises very greatly, only to fall off abruptly with further additions of either of these metals.

Dr. Waldo: My curve related only to electrical conductivity.

The President: If irregularity in one physical property argues

chemical combination, why, one would think that irregularities in other properties would argue in the same direction.*

The evolution of heat which Dr. Waldo mentions is striking; but I think that we should be cautious in basing on this fact the conclusion that there is a difference in the nature of combination until careful experiments have shown whether heat is or is not evolved when tin or zinc is added to copper. Much heat might well be evolved without attracting attention.

Finally, as regards the homogeneousness of copper when alloyed with but little aluminum, can Dr. Waldo fill out his argument by showing that segregation necessarily occurs in the alloys of copper with only a little zinc or tin?

I raise these points because, while I cannot pretend to anything comparable with the familiarity with the subject which Dr. Waldo has, yet, looking at it in a general way, I thought that I saw a very strong family likeness between aluminum bronze and the brasses and true bronzes.

I fear that the steel people have hardly yet agreed as to the nature of steel. Many of us have been studying the question, and for many years. There is still much that no man can explain. Our acknowledged ignorance, in spite of all the work that has been done, will, I hope, be a warning to those who are developing the very promising alloy which Dr. Waldo here treats to use great caution in making assertions as to its chemical nature. The whole subject of the nature of alloys is a difficult one, in which it is easier to assert than to know.

Turning to the use of aluminum-bronze, I see some fields where it could be extremely valuable. Its resistance to corrosion has, of course, the greatest value. In its combination of ductility with tensile strength, it seems, in certain instances, to rival the better qualities of steel; that is, there are cases in which the combination of ductility and tensile strength of aluminum-bronze is as good as that of very excellent steel. On the other hand, it has one peculiarity which should give it a value in one direction and should limit its applicability in another direction. Namely, so far as I have seen,

* Later Communication to the Secretary.—On refreshing my memory, I find that, as we add tin to copper, the electric conductivity falls off in the same abrupt way that Dr. Waldo shows in Fig. 1 for the copper-aluminum alloys. The change of curvature may be less abrupt in the former than in the latter, but the curves do not seem to differ in their general shape in such a way as to argue any difference in kind between the combinations which underlie them.—H. M. H.

the modulus of elasticity is very low—about 15,000,000 pounds, if my memory serves me right; whereas, that of steel and iron lies between 27,000,000 and 30,000,000—about double. Later experiments may show some way of getting a higher modulus for the aluminum-bronze.

Dr. Waldo: All that the President says is very interesting. With regard to the tin-copper alloys, I do not suppose there is much doubt that they involve a chemical combination up to 1 or 2 per cent. of tin. Beyond that, it is a mechanical mixture. With regard to zinc also, there is probably a chemical combination up to a certain small proportion.

As respects the evolution of heat when the constituent metals are fused together, the phenomenon is far more strongly marked in the case of aluminum-bronze than in that of any of the other white-metal alloys, and indicates a much greater amount and intensity of chemical action.

Segregation, of course, may not occur in the tin- or zinc-alloys; but the fact that it does occasionally occur indicates the mechanical nature of the union for the larger part of the metal concerned. In aluminum-bronze there is no segregation.

The difficulty with the whole set of curves of electrical conductivity for all alloys is, that this property is much affected by the presence of impurities which ordinary analyses do not reveal. An impurity sufficient to stain the metal to the electrical vibration might not be shown even by a precision-analysis.

I do not think the modulus of elasticity has been determined for the true aluminum-bronze, $AlCu_4$. The nomenclature has been so vague that any aluminum-copper alloy has received that name, and the reported results of physical tests are consequently worthless without analyses of the particular material tested. Both hardness and elastic limit can be greatly varied by comparatively small variations in the percentage of aluminum. Without being able, as yet, to formulate any law in connection with these proportions, we have obtained some remarkable results in our experiments. For instance, we furnished to the government a sample firing-pin for the Springfield rifle. The life of an ordinary firing-pin was 11,000 to 13,000 blows struck upon the pin by the hammer of the gun. Our pin was tested up to 127,000 blows, and was then taken out in apparently the same condition as when it was introduced into the breech-mechanism. In that case, the aluminum-bronze employed had a high elastic limit, and was so hard that it could be with difficulty faced

by filing. It is possible, by carrying the proportion of aluminum a little beyond that of the formula $AlCu_4$, to make a bronze of any requisite hardness, up to that of hard steel.

I have been asked, with regard to such articles as the souvenir spoons of aluminum-bronze, presented to members at this meeting, whether they are absolutely non-corrodible. Of course, that cannot be claimed for any metal or metallic alloy as against the action of strong mineral acids. As regards atmospheric agencies and the organic acids contained in fruit and other foods, I have found these spoons, which I have used for several years in my own house, to be unaffected, remaining uniformly bright. But what I have said concerning the effect of very slight variations in the proportion of aluminum upon the physical qualities of the metal, seems to be true also concerning the matter of tarnishing. I have observed that while my spoons have not tarnished, some of those manufactured from other casts of metal change color slightly by simply lying unused. This tarnish, however, is removed at once by simple rubbing, and is in no way more significant than the tarnish which silver-ware assumes.

No doubt the effect is due in both cases to the action of the sulphuretted hydrogen and sulphurous acid present in the atmosphere of our dwellings, especially where coal is burned as fuel or coal-gas is used for illumination. It seems that even this small degree of sensitiveness to atmospheric influences may be removed by absolute precision in the proportions of the combination. But it would be extremely difficult to bring the manufacture to such perfection as to guarantee that precision; and it is therefore probable that if spoons of aluminum-bronze come into general use, people will have to clean them just as they do silver or silver-plate.

The difficulty of securing absolute precision of proportions affects us in the production of much more important articles than spoons. The purity of both the aluminum and the copper is, as I have observed in my paper, of the utmost consequence. For aluminum, the highest percentage of purity that we have been able to reach commercially is 99.5 per cent. No doubt my friend Mr. Hunt may claim as high as 99.8. The difference is a reasonable discrepancy, depending upon the method of analysis. But fractional percentages of foreign admixtures may have great influence upon physical qualities in aluminum-bronze as well as in steel.

This is specially true, as I have already observed, of electrical conductivity. If a single drop of black ink be put into a gallon of

distilled water, the percentage of impurity thus added is very small; but the effect upon the electrical properties of the gallon of water is relatively very great. I ventured the statement before the Institute of Electrical Engineers that there is, as yet, no evidence that the electrical conductivity of pure aluminum is not as great, bulk for bulk, as that of copper.

J. C. Platt, Waterford, N. Y.: I had occasion, a few years ago, to look into the question of the "Delta metal," with reference to its possible introduction into the United States; but I found, after some inquiries, that the "Tobin bronze," made in Connecticut, seemed to be so nearly like the Delta that it was not worth while to enter into a commercial competition, with possible ruinous consequences to the manufacturers of both. I found the literature of the subject both meager and unsatisfactory. The statements made concerning these alloys were generally brief and sweeping, but not backed up by any careful investigations. I hope that Dr. Waldo will be able to tell us wherein the aluminum-bronze, in its different proportions, is superior to these cheaper alloys. Notwithstanding the recent reduction in the price of aluminum, it is still much dearer than iron, the alloy of which, with copper, appears to be the secret of the strength of the Tobin bronze.

A. E. Hunt, Pittsburgh, Pa.: Upon the question of Mr. Platt, concerning the relative advantages of Delta metal and aluminum-bronze, perhaps a side-light may be thrown by the fact that the Delta Metal Company of France has purchased the Hall aluminum-patent for that country, with the idea of making aluminum-bronze, to replace, in whole or in part, their Delta metal. It is fair to suppose that for some purposes and in some particulars, at least, they consider the aluminum-bronze to be superior. Otherwise, they could scarcely, in view of its somewhat higher cost, either fear it as a competitor, or attempt to introduce it as a substitute, for their present product.

To my mind, the advantage of aluminum-bronze over any other metal or metallic compound lies in its combination of adequate strength with exceptional incorrodibility. The preference of the government engineers for the aluminum-bronze bolts exhibited here by Dr. Waldo is not due to the belief that they would be better than steel bolts at the beginning, but to the fact that the latter would be liable to corrosion under the conditions of use, while the former would be comparatively unaffected after the lapse of years.

DR. WALDO: There seems to be an under-current of feeling that there is a relationship between aluminum-bronze and the alloys of copper with other metals. All I can say is, that if they are relations, they are "poor relations," and not to be recognized in the aristocracy of the aluminum-bronzes. The employment of manganese, iron, etc., for such alloys gives, in my judgment, a treacherous and non-homogeneous product as compared with aluminum-bronze. I do not recognize the Tobin bronze, or metallic compounds of that class, as belonging in the same category with aluminum-bronze. As I said to the Secretary, when I offered my paper to the Institute, the manufacture of aluminum-bronze is still in its infancy, and I can present concerning it nothing more than a preliminary report of progress. Nevertheless I believe, and have attempted to show in my paper, that we have to deal, in this manufacture, with a product differing in kind from the ordinary alloys of copper, and presenting closer analogies with steel than, for instance, with brasses or bronzes. The great effects produced by minute variations of proportion in the constituents, and by minute proportions of foreign ingredients, are among these analogies.

In this connection, lest a previous remark should be misunderstood, I wish to pay a high, but deserved, tribute to the progress made by the Pittsburgh Reduction Company in the commercial production of pure aluminum. The managers and experts of that company were quick to see the relation between the purity of their metal and its successful employment in the arts; and I know of no case in metallurgical history where improvement in the quality of the metal produced has been so rapid and satisfactory.

THE NICKEL-MINE AT LANCASTER GAP, PA., AND THE PYRRHOTITE DEPOSITS AT ANTHONY'S NOSE ON THE HUDSON.

Discussion of the Paper of Prof. Kemp. (See p. 620.)

(Bridgeport Meeting, October, 1894.)

E. E. OLCOTT, New York City: Prof. Kemp's valuable description of the Lancaster Gap mine is in line with many other able contributions on the origin of mineral deposits that the Institute has lately brought forward. The splendid contribution that Prof.

Posepny gave us last year, coupled with the discussion that it has elicited, adds much to the literature on the subject, and should have an important bearing even on economic mining considerations.

It is a trite saying that "a mining engineer cannot see further into the ground than anybody else;" yet though, physically speaking, this is correct, we certainly must make the result of geological research help our judgment in opening up mineral deposits. If our professional opinions are to be based only on the actual ore that is in sight, they become of little value. Although it would be manifestly imprudent to count positively on ore not uncovered, we must make the knowledge of similar formations and the results of observation stand us in good stead.

The valuable opinions and generalizations given by Prof. Kemp regarding the Lancaster Gap and the Anthony's Nose mines will help others in the field in further observations, and in collecting additional information.

While it is somewhat startling to have the immediate successor of Prof. Newberry ascribe an igneous origin to even some of the pyritous deposits, we cannot but see some weight in the arguments adduced. Certainly the writings of Prof. Vogt, referred to by Prof. Kemp, as well as his notes on the pyrites deposits of Norway and Germany, published in the *Zeitschrift für Praktische Geologie* for February, April, and May of this year, deserve most respectful consideration. Some of the types described seem to have exemplifications in this country; but though I firmly believe that igneous intrusions, in the neighborhood of mineral deposits, have had a most important influence in their formation, I believe that their agency has usually been exerted through gaseous or aqueous action, rather than by what Prof. Kemp calls magmatic differentiation. Not having examined the Lancaster Gap mine, I do not wish to express positive opinions; but, judging from Prof. Kemp's description, it seems strange that the enriched nickeliferous ore is only at certain places on the walls, and especially that the ore decreased in richness as depth was obtained, while the converse would seem probable under the magma-theory.

I cannot see how a fused mass, even though it might have been as fluid as water, could have remained in this state long enough to permit the segregation of the molten sulphides from the associated basic igneous rock. I look upon the changes in mineral deposits as much more gradual than this theory would seem to imply, and I believe with Emmons and Posepny rather in aqueous deposition

and secondary concentration even as an explanation of large sulphide deposits.

The important bearing of the generalization, deduced by Prof. Kemp and Prof. Vogt, that the basic igneous rocks are favorable to nickeliferous pyrrhotite, while acidic gneisses are unfavorable, should, I think, be emphasized.

Prof. Kemp, after illustrating a case of basic igneous gangue-rock, describes the Anthony's Nose mine as a lenticular mass lying in acidic gneiss, calling attention to the fact that it has not yielded paying percentages of nickel. Vogt makes a similar comparison, and says (paragraph *d*, page 180, *Zeitschrift für Praktische Geologie*, 1894):

"It has been shown above that the Norwegian pyrite deposits, from Vigsnäs 59½° to Sulitelma 67¼° North latitude, lie in immediate connection with a certain basic rock (saussurite-gabbro) containing 45 to 50 per cent. of silica, while at Rammelsberg, as well as in the Huelva district, it may be questionable whether the country-rock has been only moderately basic or slightly acid.

"Perhaps the ores originated by pneumatolytic (or pneumatohydatogenic) process in Norway from a basic, and in Spain from an acid eruptive magma. . . . . On the one side, therefore, we find represented pyrite-deposits connected with gabbro; on the other, copper-ore deposits connected with granite."

The cross-section of the Lancaster Gap mine is not unlike the cross-section of the Shaw gold-mine in Eldorado county, Cal., a sketch of which, by Mr. Leo von Rosenberg, is shown in the accompanying figure. This Shaw ore-body is apparently a dike, enriched along its contact with the slate country-rock.

Dr. H. W. Turner, of the United States Geological Survey, who examined the rock-specimens of the Shaw mine, says:

"The ore-bearing lode represented by the white and greenish-gray rock appears to have been introduced as a dike into the clay slates. The rock of this dike is composed of plagioclase feldspars, and would be called a porphyry by most miners. Originally, it may have been a soda-lime feldspar."

Such a soda-lime feldspar would be a basic igneous rock.

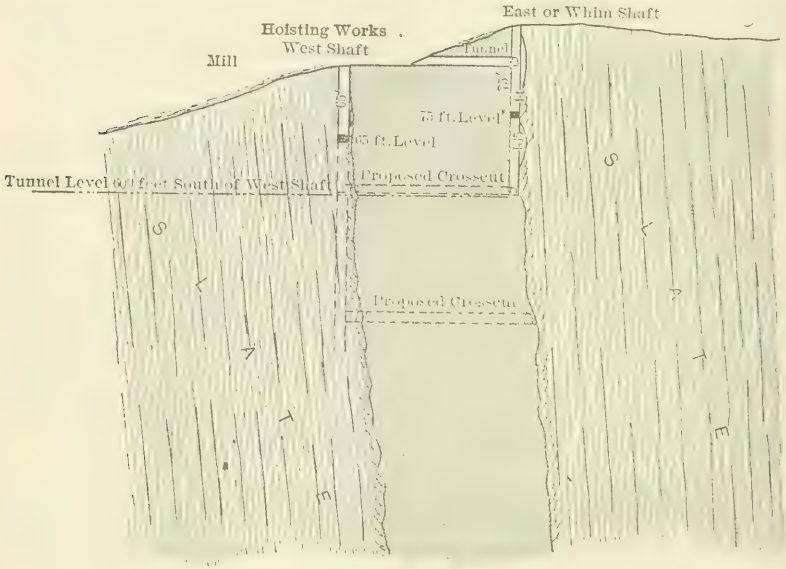
This mine has produced pay-ore on each wall for a thickness of from 7 to 12 feet. The Shaw dike is continuous for 2 miles with a thickness of from 100 to 150 feet; but, outside of the hanging- and foot-wall streaks just mentioned, it has not been found remunerative up to the present time, though the whole mass carries some gold.

Local enrichments of parts of copper-veins have often been commented on. An instance is recalled in the case of the Ore Knob

copper-mine of North Carolina, where, after passing through the gossan, oxidized ore containing much beautiful red oxide (cuprite) and assaying 16 per cent. in copper was common, while in depth the undecomposed pyritous ore never yielded more than 4 per cent. of copper.

Both of these examples certainly show the results of circulating water; and they are mentioned merely to raise the question whether the Lancaster Gap mine may not have been another instance of the same kind.

R. W. RAYMOND, New York City: Prof. Kemp's description of the mine at Anthony's Nose has specially interested me, because I had occasion nearly thirty years ago, as a member of the firm of Adelberg and Raymond, to examine that deposit, to assist in certain



Cross Section of the
SHAW MINE, ELDORADO CO., CAL.
Scale, 150 feet = 1 inch.

experiments performed upon the ore and to join in the advice given to the Hudson River Copper Company, which then owned the property.

The experiments to which I refer consisted in the "kernel-roasting" of a considerable quantity of the ore (which was pyrrhotite, with very small percentages of copper and nickel) with the view of ascertaining whether the copper and nickel could be sufficiently concentrated in the "kernels" resulting from such roasting

to be made commercially available. As I recollect the matter, some degree of concentration was effected; but the product was not valuable enough to be profitably treated, particularly in view of the circumstance that the sulphur burned in the heap-roasting went to waste.

My partner, Dr. Justus Adelberg, was a thorough and brilliant technological chemist, and had been in charge of large chemical works in England. It was his constant hope to establish in the United States those great technical industries, the basis of which would be furnished by the domestic production of cheap sulphuric acid.

At that time all the sulphuric acid produced here was made from Sicilian sulphur, and he saw in this extensive deposit of pyrrhotite the source of a raw material which might take the place of the imported article. Under his advice, the attempt was made to introduce this material, in the hope that its residual value for copper and nickel, added to its original value as a sulphur-ore, might give it a commercial footing. I do not remember the exact history of the enterprise; but I presume it was a failure. American manufacturers of sulphuric acid were not prepared to utilize the possible value of the residuum of roasting and were not tempted to abandon the use of native sulphur. Shipments were made to England; but no doubt the river and ocean freights consumed the margin of profit. A small experimental furnace on Staten Island made some sulphuric acid from the ore, and, finally, the chemical works on the Hudson, to which Prof. Kemp has alluded, employed it for a considerable period. I need hardly say that pyrrhotite is inferior to pyrite as a commercial source of sulphuric acid, and that the extensive manufacture which now exists employs, in the main, better materials than that of the mine at Anthony's Nose. It was, in fact, the hope that the mine would grow, in depth, richer in copper (if not also in nickel), which led to the attempt to make it pay, as a sulphur-mine, the expense of further development.

The success and fame of the mine at Lancaster Gap undoubtedly stimulated this hope as to nickel, and the developments in the Ducktown mines of Tennessee encouraged a similar expectation as to copper. It was supposed that a general analogy subsisted among all the lenticular deposits of pyrrhotite in crystalline rocks, and that, while only a few of them might prove valuable for mining, there was a chance that any one of them might be among that few. Such a distinction as Prof. Kemp has indicated in the character of

the country-rock was either not recognized or not deemed to have an economical significance. I regard his generalization on that head as a contribution of the highest value to mining geology. And it will be equally valuable, whether his theory be or be not accepted—*i.e*, whether the difference in the country-rock, which he shows to exist between the two classes of pyrrhotite-deposits, be or be not accompanied with a radical difference in the mode of their formation. I cannot say that I am entirely prepared to accept the notion of a concentration from the fused magmas for the case of the deposits in or adjoining basic eruptives; but it must be confessed that Prof. Kemp has made a strong presentation of it, and is supported with important analogies and parallels by the work of Vogt in the Scandinavian peninsula.

Prof. Kemp: Mr. Olcott and Dr. Raymond have correctly appreciated, as the essential point urged in my paper, the contrasted geological associations of the profitable and unprofitable deposits of nickel-ores. The mode of their origin is of secondary importance. Nevertheless, the facts adduced in the paper have a bearing upon that question, and do not stand alone, in tending to show that the widespread prejudice, heretofore existing among American geologists, against the notion that igneous rocks may be the direct sources of economically valuable ores, is not in all cases warranted. With regard to the few ores specially associated with such rocks (namely, titaniferous magnetite, chromite and nickeliferous pyrrhotite with chalcopyrite) the practically unbroken series of known occurrences, from metallic meteorites to igneous rocks with basic segregations, is striking and significant, even though the chemistry of fused and cooling magmas be not fully understood.

In my paper I omitted from the list of authors referred to (*ante*, p. 630), the name of Mr. W. H. Merritt, of Toronto, Canada, who, in the discussion of Mr. Emmons's paper on the "Geological Distribution of the Useful Metals in the United States" (*ante*, p. 755), while admitting that magmatic concentration is not an unreasonable hypothesis for some of the ore-bodies, cites evidences of the secondary origin of others, especially those rich in copper.

To the last foot-note on page 629, I wish to add a reference to the paper of Alfred Harker, "Carrock Fell, a Study in the Variation of Igneous Rock-Masses," *Quart. Jour. Geol. Soc.*, 1894, p. 311, and especially p. 326.

MAGNESIA AND SULPHUR IN BLAST-FURNACE CINDER.

Discussion of the Paper of Mr. Firmstone. (See p. 498.)

(Bridgeport Meeting, October, 1894).

E. K. Landis, Philadelphia, Pa. (communication to the Secretary): Mr. Firmstone's paper is of great interest; but he has unfortunately omitted to state the most important point, viz., the percentage of sulphur in the two materials he compares; and this omission vitiates the conclusions which he draws.

Comparing by grade, instead of silicon-contents (as in his Table III.), the iron made using limestone with that made using dolomite, we find for gray-forge the average of four analyses under limestone to be, Si, 0.644; S, 0.089; while one analysis under dolomite gives Si, 0.508; S, 0.070.

For No. 2 iron, the limestone flux gives, as an average of 19 analyses, Si, 1.107; S, 0.051; while the average of 4 analyses made with dolomite flux gives Si, 0.630; S, 0.056.

It will be observed, that no No. 1 pig is given under limestone, while 14 analyses appear under dolomite, averaging Si, 0.950; S, 0.028.

As nothing is said about changed conditions, it is fair to presume that the ore and fuel, as well as the blast-temperature (which is stated to have been uniform at about 750° F.) remained practically the same; and since the sulphur in the two fluxes is not given, it is impossible to know whether the lower sulphur for equal silicon-contents is due to the use of dolomite or to other conditions.

The use of dolomite would produce a more fusible cinder, and the furnace would be hotter than when using limestone; therefore, we might expect to see a grayer iron. The amount of No. 1 iron made with dolomite seems to confirm this supposition; and, under these conditions, we might expect more of the sulphur to be volatilized, and to pass off in the gases.

The easier fusibility of the cinder would also permit more rapid driving; and this should reduce the amount of silicon and sulphur in the iron, as the amount of reduced metalloids in the pig is directly proportional to the length of time it remains in the reducing-zone.

The writer has seen " glazy iron," containing 7 per cent. of silicon, produced from easily-reducible hematite ores, using pure limestone as flux, and with a cinder so basic that it would hardly run out of the furnace. The infusibility of the cinder prevented the furnace from being driven rapidly enough to remove the iron from the reducing-zone before this large amount of silicon had been reduced and had combined with the metal. The intention was, to make low-silicon mill-iron, and the furnace should have produced 100 tons per 24 hours; instead of which it only made about 35 tons, having the above composition.

The amount of alumina stated by Mr. Firmstone is surprising, the writer never having seen a cinder running so low.

Ledebur, in his *Eisenhüttenkunde*, gives a method of fluxing ores which has been used by the writer for many years, and found extremely convenient and satisfactory. He calls attention to the fact that, to produce the most fusible cinder, the ratio of alumina to lime plus magnesia should approximate 1 : 3, or 1 : 4; and suggests that the mixture be arranged to fulfill this condition. The above-mentioned ratio, in Mr. Firmstone's limestone-slag, is 1 : 9.63, and in his dolomite-slag 1 : 13.66, showing that the fusibility of the slag could be largely increased, and considerable economy thereby effected. The use of dolomite for limestone is a step in this direction; for the stoichiometrical equivalent of his dolomite for a singulo-silicate is 6.04, and that of his limestone 9.03. In other words, 6 tons of the dolomite are equal to 9 tons of the limestone for fluxing purposes.*

This matter of the relative value of different fluxes, though very important in these days of close competition, has been almost entirely neglected by furnace-men.

The following table gives some analyses of limestones and dolomites, with the equivalents worked out to show the comparative fluxing values. No. 1 is taken as 1000; therefore, 1000 pounds of No. 1 are equal in fluxing value to 1099 pounds of No. 3, or 1304 pounds of No. 5, etc. D is theoretical dolomite, and L is pure limestone. It is easy to see the economy of using the former. Nos. 3 and 4 are regularly used at a large steel-plant in eastern Pennsylvania, and No. 4 at a furnace in the Lehigh district. Analyses were furnished by the owners. The equivalents are given for both singulo- and bi-

* For the manner of calculating the stoichiometrical equivalent, the reader is referred to Ledebur's *Eisenhüttenkunde*, or the translation by the writer, published in the *Journal of the U. S. Assn. of Charcoal-Iron Workers*, vol. viii., No 3.

Relative Fluxing-Values of Limestones and Dolomites.

	1	2	3	4	5	6	7	8	D.	L.
Silica	0.68	2.105	2.20	3.20	6.16	8.305	1.710	4.00	.....	.....
Oxide of Iron and Alumina	1.675	1.010	2.10	1.55	1.43	3.555	1.455	1.305	.....	.....
Carb. of Lime	55.17	55.235	95.95	94.28	50.34	54.09	90.37	81.50	54.282	100.00
Carb. of Magnesia	41.066	41.514	2.415	1.512	40.57	33.51	6.846	13.856	45.718	.....
Phosphorus	0.018	0.025	0.011	0.021	.....	0.004	0.039	0.018	.....	.....
Sulphur	trace	0.026	0.164	0.020	.....	trace	trace	trace	.....	.....
Stoichiometric Equivalent ... ... { SS	5.82	6.21	6.40	6.95	7.59	8.15	6.44	7.06	5.717	6.246
Stoichiometric Equivalent ... ... { BS	5.76	6.00	6.17	6.56	6.76	6.90	6.26	6.57	5.717	6.246
Comparative value	1000	1067	1099	1194	1304	1400	1106	1213	982	1073

SS = singulo-silicate; BS = bi-silicate. Data:

O in silica, ... 53.3 per cent.
O in alumina, ... 46.6 per cent.
O in lime, ... 28.57 per cent.
O in magnesia, ... 40.0 per cent.
Lime in $CaOCO_2$, ... 56.0 per cent.
MgO in $MgOCO_2$, ... 47.62 per cent.

silicates, although the former only are used in coke and anthracite practice, while the latter concern the charcoal-iron producers.

MR. FIRMSTONE (communication to the Secretary): The sulphur in the dolomite, as shown by several analyses made during the time covered by the iron-analyses, varied from 0.025 per cent. to 0.104 per cent. (iron of October 25th). I have none of the limestone for September, but one in August shows 0.125 per cent., and those made in the preceding months give the same.

Through inadvertence this was not stated in the paper as it should have been, but the point was not overlooked in writing it; and the following figures would seem to show that the sulphur in the flux, even at the highest, was not sufficient to affect the general conclusion drawn.

The materials used in making the iron of October 25 contained the following quantities of sulphur per 100 parts of pig-iron made:

In the coke,	1.300
In the dolomite,	.135
In the ore,	.059
	1.494

The sulphur in the No. 1 iron made was (as stated in Table II.) 0.025 per cent., being 1.67 per cent. of the total sulphur filled into the furnace.

For the iron of November 6, we have per 100 of iron:

In the coke,	1.661
In the dolomite,	.056
In the ore,	.090
	1.807

Sulphur in the iron (No. 1), as per Table II., 0.025 per cent., or 1.38 per cent. of the total sulphur present.

As above stated, I have not the figures for a like statement for the time covered by the analyses in Table I., but one made in August shows per 100 of pig-iron:

In the coke,	1.387
In the limestone,	0.143
In the ore,	0.010
	1.540

Sulphur in the iron (No. 1) 0.024, or 1.56 per cent. of the whole. This iron contained 1.77 per cent. of silicon. It appears from the

above, that at the highest, the sulphur from the flux is less than one-tenth of the whole; and even if the sulphur in the pig be drawn from the three constituents of the charge in equal proportions, the difference between no sulphur in the flux and the greatest amount shown above, would only affect the percentage of sulphur in the pig in the third decimal place.

But the figures for November 6th show that an increase in the total sulphur of more than double the greatest amount shown in the flux was without effect on the sulphur in the iron.

It is hardly necessary to point out that (considering the known relation of the lime to sulphur) a given variation in the sulphur contained in the flux is far less likely to appear in the iron than an equal variation in that brought in by the ore or fuel.

There was no known or probable variation in the ore or fuel; and the driving for five weeks on limestone was 791 charges; for the five following weeks on dolomite, 765 charges; therefore, practically the same.

Since my paper was written, I have found at a works in the Lehigh region a specimen of cinder of the following composition:

	Proximate per cent.	Ultimate per cent.	
SiO_2,	35.26	35.26	
Al_2O_3,	10.20	13.83	
CaO,	24.35	24.35	
MgO,	17.79	19.23	
FeO,	1.65	1.65	
CaS,	3.10	3.10	(S = 1.38)
SiO_2,	0.37	0.37	
Spinel,	5.07	.....	
K_2O,	1.29	1.29	
Na_2O,	0.90	0.90	
	99.98	99.98	

The spinel contained:

Al_2O_3,	70.78
MgO,	27.91

The furnace-manager, a very competent man, informs me that it was probably made when running on gray-forge iron, and that he had never noticed any want of fluidity or tendency to lose hearth-room when making similar cinder. This cinder being intermediate between the Andover and Stanhope samples, so far as percentage of spinel is concerned, and not showing any tendency to choke up the hearth, it seems doubtful whether the spinel has any direct influence

in that direction; and the tacit assumption, made in my paper, that such is the effect of the spinel, must be set aside pending further evidence.

The above, and all the original analyses in the paper, excepting those of the Longdale cinder, were made by Mr. P. W. Shimer, Easton, Pa.

Mr. Landis (communication to the Secretary): The figures given by Mr. Firmstone show the dolomite to average 0.0645 per cent. sulphur, while the limestone averages 0.125 sulphur, or nearly twice as much. Under these circumstances, lower sulphur was to be expected.

But there is still another reason, apparently overlooked by Mr. Firmstone, viz., the oxygen-ratios of the slags. In the dolomite slag the ratio of basic to acid oxygen is 1 : 1.01, and in the limestone slag 1 : 1.17. The basicity of the slags is therefore approximately in the ratio of 1.16 : 1, the dolomite slag being more basic than the limestone slag. This would tend to reduce the percentage of sulphur in the pig-iron, and would probably more than equal the difference of affinity for sulphur between the lime-alumina silicate and the lime-magnesia-alumina silicate. From a careful study of these conditions, Mr. Firmstone's results go to confirm those of Prof. Ledebur instead of conflicting with them.

With regard to the formation of spinel, it would seem to be a result of too basic a cinder.

The change produced by the formation of spinel leaves a less basic cinder, which is probably more fusible than the original one, and the temperature would also be raised by the heat given out when the spinel solidified, according to the laws of thermodynamics. The formation of spinel seems to follow the law of formation of chemical precipitates.

Upon substituting dolomite with less sulphur, for limestone, and thus forming a more basic slag, the resulting pig-iron should have been lower in sulphur than before, as is shown by the above considerations, and it is difficult to see how any other result could have been expected.

R. W. Raymond, New York City: The experiments of Prof. Ledebur, to which Mr. Firmstone refers, were made by melting, in crucibles lined with charcoal, measured quantities of sulphurous pig-iron and slags of different composition. Inferences drawn from such experiments as to the effect of lime, magnesia, etc., in the blast-furnace charge, upon the proportion of sulphur in the resultant pig-

iron, are open to the criticism that the conditions of the experiment do not represent those of the blast-furnace. The tacit supposition appears to be, that the hearth of the blast-furnace represents the crucible of the experiments, and that in the hearth, as in the experimental crucible, there is a reaction between the metallic bath containing sulphur and the materials of the slag. Evidently this can be true, if at all, only as regards that proportion of the sulphur in the total charge which has been actually taken up in the zone of fusion by the melting iron. That this proportion is not dependent only upon the total sulphur in the charge, need scarcely be pointed out. The elimination of sulphur in the upper zone of the furnace by oxidation, and the formation of calcium or magnesium sulphide in the reducing zone, prior to the carbonization and fusion of the reduced iron, are factors not represented in Prof. Ledebur's experiment; and it is quite conceivable that a dolomite flux may operate throughout the whole blast-furnace process so as to bring about a final result differing from that of his crucible-fusion. I understand Mr. Firmstone to mean this, when he says that "Ledebur's experiments seem conclusive as to the exact point he investigated:" this exact point being the effect of slags already formed upon sulphurous pig-iron already formed, when they are melted together in the presence of abundant carbon. And the acceptance of these experiments as conclusive on that point does not exclude the possibility that Prof. Ledebur's sweeping deduction as to the use of dolomite flux in the blast-furnace may be untenable, by reason of other conditions which he did not take into account.

Prof. Ledebur's own statement of the matter, made twelve years later than his experiments, is found in his *Handbuch der Eisenhüttenkunde* (1893), page 291, from which I translate as follows:

"Fortunately the behavior of lime towards sulphur-bearing iron gives the iron-master a means for removing the sulphur from the iron. When sulphurous iron is melted with lime and coal, there are formed metallic iron (pig-iron) and calcium sulphide, which, being insoluble in the iron bath, is taken up by the cinder. Perhaps this process is represented by the formula, $FeS + CaO + C = Fe + CaS + CO$. It is not necessary that the lime should be present in a free state; basic slags rich in lime have the same effect, but the more strongly, the greater their contents of lime. According to R. Åkerman (*Kaernthner Zeitschrift*, 1876, p. 160), one and the same ore, smelted successively with 15 per cent. of quartz, with 5 per cent. of lime, and with 20 per cent. of lime, gave pig-irons with 0.09, 0.04, and 0.01 per cent. of sulphur respectively. In several experiments of my own, a specially prepared pig-iron, containing 2.33 per cent. of sulphur, was melted at one time with a singulosilicate, and at another time with a bisilicate slag. Both slags contained, besides lime, some alumina, and the quantity of slag was in each case double that

of the iron. In the resulting product, the singulosilicate slag contained 1.445 per cent. of sulphur to 0.079 per cent. in the iron; the bisilicate, 0.681 per cent. to 0.357 per cent. in the iron.

"Protoxide of manganese in the slag is likewise favorable to the elimination of sulphur.

"The desulphurizing effect of magnesia is smaller. Magnesium has but a slight affinity for sulphur; nevertheless, a basic slag containing magnesia likewise takes up sulphur from the iron. When, in my experiments above mentioned, the lime in the slags was replaced by a chemically equivalent quantity of magnesia, the singulosilicate slag showed 1.069 per cent. of sulphur to 0.260 per cent. in the corresponding iron, and the bisilicate 0.290 per cent. to 0.391 in the iron.

"Alumina in the slag has no direct influence upon the contents in sulphur.

. . . .

"High temperature favors in these cases the taking up of sulphur by the slags, and consequently the production of pig-iron low in sulphur."

It will be seen that, in this later restatement of his views, Prof. Ledebur does not explicitly declare the unconditional superiority of limestone over dolomite as a flux for the production of low sulphur pig. Yet perhaps that opinion may be inferred from his language.

Mr. Firmstone cites the well-known practice of the Lehigh Valley and other regions to show that there is no difficulty in making, with dolomite, iron low in sulphur, and adduces analyses proving that "under certain circumstances, at least," the sulphur in the pig has been reduced by substituting dolomite for limestone containing about 5 per cent. of magnesia. These statements do not contradict Prof. Ledebur's results; they only tend to modify the sweeping generalization as to dolomite which has been based thereon. Mr. Firmstone refers to two recent utterances out of many which he might have quoted, and the unqualified character of these expressions certainly justifies his protest.

Thus, Sir Lowthian Bell says* that the use of dolomite as a flux is "a great disadvantage, because lime acts not merely as a flux but as a means of removing sulphur; an office magnesia is incapable of discharging."

Mr. Stead† declares that lime is a much more powerful desulphurizer than magnesia; that magnesian limestone has never been used successfully in smelting Cleveland iron, the sulphur having been increased in the pig whenever it has been tried; that it has given better results in the manufacture of Bessemer iron, though even in that department, after trial in one or two cases, it has been aban-

* *The Iron and Steel Inst. in America in* 1890, p. 16.

† *Jour. I. and S. Inst.*, 1892, No. ii., p. 242.

doned for limestone, and that "it is clear that by far the most potent agencies at work in a blast-furnace for the removal of sulphur are a sufficiently elevated temperature accompanied by a sufficient quantity of basic silicate of lime."

Mr. Stead cites also from H. M. Howe* a passing allusion to "the Illinois blast-furnace practice," in which the substitution of calcite for dolomite materially diminished the sulphur in the pig, as showing that lime is a more efficient desulphurizer than magnesia. But Mr. Howe, elsewhere on the same page, distinctly asserts that magnesia also removes sulphur, and nowhere goes to the length of declaring with Sir Lowthian Bell that magnesia is incapable of doing this, or with Mr. Stead that "*by far the most potent agencies*" are high temperature and plenty of basic silicate of lime.

As against such sweeping assertions, Mr. Firmstone shows that under certain circumstances magnesia does the work even better than lime. What those circumstances are, he fully sets forth without discussing them in detail; and Mr. Landis's suggestions, if accepted as both correct and sufficient, do not at all invalidate Mr. Firmstone's position, or justify the unqualified proposition against which he protests.

But I do not think Mr. Landis's suggestions cover the case. He thinks the results adduced by Mr. Firmstone are to be explained:

1. By the fact of the presence of 0.125 per cent. of sulphur in the dolomite, and only 0.0645 per cent. in the limestone. I do not believe that this difference would have any considerable (if indeed a traceable) effect on the resulting pig.

2. By the fact that the dolomite slag was slightly more basic than that of the limestone. This would unquestionably have some effect, but it seems to me highly doubtful whether the very small difference in oxygen-ratio here disclosed could account for the effect actually exhibited. But if Mr. Landis is correct, it follows that the basicity is a factor so much more potent than the lime that a small change in basicity is sufficient to counter-balance all the difference between the affinities of lime and magnesia for sulphur.

In other words, on Mr. Landis's own showing, the sweeping condemnation of dolomite expressed by various authorities should be modified so as to declare that if the dolomite be slightly less sulphurous, and the dolomite-slag slightly more basic, than the lime and

‡ *Metallurgy of Steel*, p. 51.

its slag, dolomite may be used, under conditions otherwise the same, to produce pig-iron surprisingly low in sulphur. Such a statement would express the facts; and I think Mr. Landis will confess that it would not "confirm," but, on the contrary, essentially alter, the sweeping generalization which Mr. Firmstone has modestly ventured to question. I wish that Mr. Landis, or some other observer equally competent, would take up this subject for comprehensive discussion, determining, if possible, the quantitative value of the different factors involved, such as the basicity of the slags, their temperature of fusion and specific heat, the percentage of sulphur in ore, flux, and fuel respectively,* the furnace-reactions prior to fusion, the reactions in the hearth, and finally (but, perhaps, when all is said, not chiefly), the direct affinity for sulphur possessed by calcium, magnesium, etc.

COAL-DUST IN MINE-EXPLOSIONS.

Discussion of the Paper of Mr. Glenn on "Mine-Explosions Generated by Grahamite Dust." (See p. 195.)

(Virginia Beach Meeting, February, 1894.)

E. E. Russell Tratman, New York City (communication to the Secretary): Mr. Glenn's paper brings up again the question of the part played by coal-dust in colliery-explosions. In the discussion of Sir Frederick Abel's paper on "Accidents in Mines," read before the Institution of Civil Engineers,† much was said about the dangerous properties of coal-dust, and several instances were given of the successful use of a spray of water, water and compressed air, or exhaust-steam from an engine, to keep the mine sufficiently damp to prevent the formation of dust clouds. Methods of preventing shots from blowing out; of using water cartridges to prevent flame; and of using lime cartridges, wedges and other means of breaking down the coal were also discussed. Mr. Emerson Bainbridge referred to the explosive termed "roburite" (ammonium nitrate and

* Nothing is more probable *a priori*, or according to experience, than that sulphur behaves differently in these three relations, or, in other words, that the behavior of two blast-furnace charges cannot be predicated upon their total sulphur without regard to its original distribution and combinations.

† *Proc. I. of C. E.*, 1887 and 1888, vol. xc., p. 160, and vol. xci., p. 36.

well purified chlorinated dinitro-benzine) which produced no flame and could be used with safety in fiery mines. It is, however, asserted to give out very noxious fumes. The very complete report of the British Commission on Accidents in Mines,* made in 1886, discusses the dust question very fully, and gives details of numerous experiments, from which it was concluded that while, in the total absence of fire-damp, coal-dust does exhibit some tendency to become inflamed or to carry or propagate flame, the tendency is of a limited nature, and very different from the property of carrying or propagating flame which even comparatively non-sensitive dusts possess in the presence of a very small quantity of fire-damp. The proportion of fire-damp required to bring dust readily into operation as an explosive, when thickly suspended in the air, borders upon and is even sometimes below the smallest amount (about 2 per cent.) which can be detected in the atmosphere of a mine, by the most practiced observer, with the use of the Davy lamp. The report says that the most decisive of the results of numerous experiments were not of a nature to warrant the conclusion that flame could be carried along to a very great distance by coal-dust in the complete absence of fire-damp. On the other hand, in the summary of their report the commissioners made the following remarks:

"Unless the coal-dust which exists in the immediate vicinity of a blown-out powder shot is dry, very finely divided and of a very highly inflammable character, the propagation of flame from the shot by the raised dust will only take place to a comparatively limited extent if the atmosphere in which the dust is raised be entirely free from fire-damp. It is, however, well established that even when the air is quite free from fire-damp, an exceptionally inflammable coal-dust in a very finely divided and dry condition, and existing in abundance in the immediate vicinity of a blown-out shot, may, when raised by the shot, be ignited so readily and carry on the flame so rapidly that it may produce explosive effects of a similar character to those caused by a gas explosion. The flame, as it rushes along, if fed by freshly raised dust, may extend under these circumstances to very considerable distances, with results resembling, in their disastrous nature, those of explosions originating with, and mainly due to, fire-damp."

Perhaps the dust of the grahamite described by Mr. Glenn may be classed with exceptionally inflammable coal-dust. The commissioners did not appear to think the use of water in laying the dust in roadways very practicable, but did recommend the use of high explosives, in conjunction with water, for dry mine-workings liable to access of fire-damp. They also recommended careful work to

* Engineering News Publishing Co., New York, 1887.

reduce the liability to blown-out shots. In a report made by Mr. Hall, Inspector of Mines, to the Royal Commission on Explosions from Coal-Dust in Mines (England), issued in 1894, details are given of experiments made with numerous samples of coal-dust in a mine-shaft; a wrought-iron cannon being used for the gunpowder-shots. He presented the following conclusions: 1. That the flame from a blow-out gunpowder-shot in the presence of coal-dust always ignites more or less of the dust, which increases the burning and charring effect of the shot; 2. that a dusty atmosphere will explode with great violence if traversed by a flame, the explosion continuing through any length of such atmosphere and increasing in violence as it progresses; 3. that coal-dusts from certain seams are almost as sensitive to explosion as gunpowder; 4. that the higher the quality of the coal-seam, the more sensitive is the dust to explosion; 5. that a ready supply of oxygen, as by brisk ventilation, makes the dust-explosions more probable and more severe; 6. that certain high explosives are incapable of igniting or exploding coal-dust, for which reason the report recommended the total exclusion of gunpowder from coal-mines and the substitution of such high explosives.

Mr. Glenn observes that he has been unable to find records of any work done in this direction by M. de Souich, mentioned by Prof. Abel; and I therefore beg to quote the following lines from the report of the Accident Commissioners:

"Although the report of Faraday and Lyell, and subsequent writings on the subject by the former, were published in 1845, these publications appear to have remained long unknown in France; for in 1855 M. de Souich, Chief Government Mining Engineer, of the Sainte Etienne Arrondissement, when referring to an explosion which had occurred at Firminy, advanced as new the view that the deposition of crusts of a light coke upon the props was due to dust which had been swept up and transported to a distance by the violent current produced by the explosion, and which, becoming in part inflamed, had extended and prolonged the destructive effects originated by the fire-damp. On the occasion of two explosions in 1861, M. de Souich again dwelt upon his views regarding the part played by coal-dust in increasing the disastrous effects of fire-damp explosions."

The difficulty which Mr. Glenn has experienced in this instance emphasizes once more the importance of full references in all papers presented to technical societies and in technical books. Mr. Glenn says that while M. de Souich and an Austrian observer are mentioned by several writers, nobody has cited the places of their communications; and this, I notice, is true of the commission report just quoted. Much exasperating labor on the part of students might be saved by

a foot-note giving title, publisher, volume, date and such other references as are necessary to locate the publication. This, it will be noticed, Mr. Glenn has done. In books, I think, this defect is more frequently found than in the proceedings of technical societies.

G. C. Hewitt, Washington, D. C. (communication to the Secretary): With reference to the influence of dust in coal-mine explosions, I wish to suggest, as the result of considerable experience with extremely inflammable and other coals in Wyoming, Utah and Colorado, that the degree of inflammability is largely a matter of the physical structure of the coal as distinguished from its chemical composition. That some coals may, under ordinary conditions, burn so rapidly as to produce an explosion, there is no question. As an instance, the finer dust from the drilling in the New Castle (Colorado) big seam, will, if dusted over the flame of a candle, carry the flame up 15 or 18 inches. This coal contains about $2\frac{1}{2}$ per cent. of water and about 38 or 40 per cent. of volatile matter.

The Castle Gate (Utah) coal, containing about $1\frac{1}{2}$ per cent. of water, is also extremely inflammable; and the Almy (Wyoming) coal, containing 12 per cent. of water is nearly as much so; while the Rock Springs (Wyoming) No. 1 coal, containing $7\frac{1}{2}$ per cent. of water, is not sufficiently inflammable to be explosive.

The use of giant instead of black powder will, with the above coals, under ordinary conditions, avoid the probability of explosion; and I would suggest that Settle's "water-cartridge," in use in Staffordshire, England, would avoid the possibility of explosion with any coal that I am aware of.

R. W. Raymond, New York City: In this connection, mention should be made of papers in our *Transactions*, giving valuable information upon this subject. I refer particularly to the paper of Messrs. Bramwell, Buck and Williams on "The Pocahontas Mine-Explosion" (xiii., 237), and that of Mr. Hutchinson, "Notes on Coal-Dust in Colliery Explosions" (xiii., 253). Both of these were presented at the Chicago meeting of the Institute, in May, 1884. The latter gives a good summary of the prior English literature of the subject.

Probably the most thorough experimental investigation of the relation between coal-dust and colliery-explosions which has ever been made was that of the Royal Prussian Fire-Damp Commission, which will be found in vol. iv. of the appendix to the general report of that commission.* Although this final and formal publica-

* *Hauptbericht der Preussischen Schlagwetter Commission*, with Appendix in Five Volumes, and Atlas, Berlin, 1887.

tion was not made until 1887, the essential results of the coal dust experiments had been reached by the end of 1884, and were made public in 1885.*

These results are much more definite and quantitative than those announced by the British Commission to which Mr. Tratman refers, and the experiments made were more precise in method than any which have come to my notice. In consequence of the recommendations of the commission, shot-firing was prohibited altogether in mines controlled by the Prussian government, where safety-lamps were used. The commission declared that black powder and all other slow explosives ought to be prohibited in collieries containing fire-damp, and that even dynamite and other high explosives, though permissible in certain cases, ought not to be used where accumulations of fire-damp are possible in sufficient quantity to give a clearly perceptible blue cap on the lamp; and that in all cases it is desirable that the air should be tested within a radius of ten yards before igniting any shot.†

Since the full report of the Prussian commission is not accessible to most of our members, I give here a part of the abstract of its results published by me in the *Engineering and Mining Journal:*

"The König mine, at Neunkirchen (Saarbrücken), was chosen as the locality for the experiments because it possesses, 120 meters below the surface, a steady, natural 'gas-blower,' which has been discharging for two years about 0.025 cubic meter of fire-damp per minute. This was conducted in pipes to a gasometer, for use in the experiments. The conditions of actual experience were imitated as closely as possible, consistently with the necessary arrangements for accurate observation and measurement. In an old dump, a gangway, 51 meters long, was constructed of elliptic rings of I-iron, lined with pine planks 5 centimeters thick, the dimensions in the clear being 1.72 meters for the major and 1.20 meters for the minor axis. One end of this gallery terminated in a mass of masonry, 3.75 meters long, 3 meters broad, and 4 meters high, strengthened with anchor-bolts, and containing seven cast-iron mortars set in the masonry, with elastic backing of soft wood and rubber. These represented bore-holes, having 35 millimeters interior diameter and 800 millimeters depth, and taking a charge of 230 grammes of powder (except the central one, which was 40 millimeters in diameter, 940 millimeters deep, and was charged with 500 grammes). They were arranged, two near the roof, two near the floor, and three in the central line of the heading represented by the masonry; and their axes were so set that those of the upper and the lower two, prolonged, would all intersect at a point in the center of the gallery 5 meters from the heading (or, in

* See *Preuss. Zeitsch.*, vol. xxxii., 1884, Abthl. B, p. 575; also an extended analysis by the writer in *Eng. and Min. Jour.*, xxxix., 221, April 4, 1885.

† I take this summary of conclusions from the *Colliery Guardian*, as quoted in the *Eng. and Min. Jour.*, xl., 216, Sept. 26, 1885.

other words, at 10 meters would strike the roof and floor respectively), while those of the middle three, prolonged, would intersect in a point in the middle of the floor 5 meters from the heading. The gallery was so built in the dump as to be buried wholly on one side and three-fourths on the other. Along the exposed upper quarter, at intervals of about 1 meter, were inserted thirty-two windows of 20-millimeter glass in cast-iron frames. There were sundry other particulars which need not be described at length: a man-hole; openings loosely stopped with wooden plugs, to serve as safety-valves; an exhauster, to accelerate the removal of after-damp; arrangements for brattice-canvas, by means of which a chamber containing 20 cubic meters could be constructed to receive and hold, 12.35 meters from the heading, the gas-mixture to be experimented upon; electrical apparatus for firing, etc. Finally, at the mouth of the gallery, in the line of its axis, a tram-way was laid, with a rising grade of 4°, upon which a mine-car was placed to give, by its motion after an explosion, a measure of the energy developed. A side-gallery at right angles to the first was afterwards added for the purpose of testing the effect of glowing coal-dust upon accumulations of fire-damp at a distance, and aside from the original ignition or explosion.

"It was found by preliminary experiments that an ordinary shot (230 grammes of powder), fired with clay tamping, gave a flame 3 to 4 meters long, but that when tamped with coal-dust the flame was 9 to 16 meters long. (For the big hole, loaded with 500 grammes, the respective lengths of flame were 5 meters and 19 meters.) The coal-dust was of medium fiery quality, as afterwards determined. This, like all the other experiments, represents only such shots as blow out, which are, of course, in practice, the most likely, if not the only ones, to cause explosions. The tamping with coal-dust was fairly assumed to give conditions at least as dangerous as those presented by a hole bored in coal.

"To test and compare the behavior of some twenty-eight different varieties of coal-dust, each was separately tried by scattering about 15 kilogrammes of it along 10 meters of the gallery, firing the charge of 230 grammes, and observing the length of the flame. This was done once with clay tamping and once with dust tamping. The maximum length of flame observed was 33 meters (from a very fine coal-dust, rich, yet not the richest of the series, in volatile matter), and the minimum was 4 to 6 meters (from a lean anthracite coal). It was found that scattering the coal-dust along the floor was practically as efficient as any other way of distributing it in the gallery, since the result of the shot was to stir it up in a cloud. Under the conditions of these experiments it appeared that, as a rule, when the flame was over 22 meters or under 12 meters long, the effect of coal-dust tamping for the shot was not important. But a dust which gave for clay tamping a flame 12 to 16 meters long showed 19 to 22 meters of flame when the shot was also tamped with it.

"The sectional area of the gallery being 1.62 square meters, it is easily calculated that 15 kilogrammes of dust scattered along 10 meters of it would give about 1 kilogramme per square meter. But in most cases only a portion, perhaps only a small portion, of the dust is actually lifted by the blast into the air, the rest being too coarse and heavy. Hence the dangerous proportion of dust must be less than 1 kilogramme per square meter. A great light is shed upon this point by Dr. Brookman's analyses of the coal-dust and of the "explosion-coke," that is, the beads and crusts of coked dust formed in the gallery after the shot. To take one instance: the coal contained 21.8 per cent. of volatile matter, the coke 13.6. Hence, 8.2 per cent. had been expelled. Now, if the whole 15 kilogrammes of coal-dust had been thus

coked, 1.23 kilogrammes of gas would have been developed. The analyses show that this gas was almost wholly hydrogen and hydrocarbons. It would therefore occupy (at freezing-point and atmospheric pressure), say, 2 cubic meters space, and if generated in 10 meters of the gallery (or 16.2 cubic meters space), would constitute about 12 per cent. by volume of the local atmosphere. But this is more than is required for the most explosive mixture. It is evident, therefore, that, under some circumstances, coal-dust alone, ignited by a blown-out shot and instantaneously coked, might give rise to an actual explosion.

"According to these experiments, however, the coal-dust undoubtedly plays a double part. First, it becomes glowing hot in the glowing gas-current from the shot. The ordinary yellow flame from the powder is colored dark red by it, and is essentially lengthened, because the particles of coal-dust continue to glow as they are blown forward. Other powdered substances might do the same. Prof. Abel, for instance, showed that magnesia would. Anthracite dust comes nearest to confining itself to this comparatively innocent rôle. But it is certain that the more intense phenomena exhibited by other coals are due to the combustion of the gas generated by the glowing dust itself. And this combustion, again, may be continuous and comparatively quiet, or it may (for instance, if a quantity of the gas has been generated and is suddenly mixed with air) become an explosion. Both processes, and irregular forms of combustion to be classed, as it were, between them, were observed in these experiments. Fortunately the former is the usual one.

"Very significant were the results obtained by scattering some kinds of coal-dust over 20, 30, or 40 meters instead of 10. Of two of the most dangerous, it was evident that, once kindled by a blown-out shot, the flame would go as far as there was any dust, and would burn with detonation. Other coal-dusts were found incapable of giving a lengthened flame, no matter how far beyond the 10 meters they were strewed.

"The chemical composition of the coal plays, of course, an important part; yet it is impossible to say positively that coals containing less, for instance, than 16 to 20 per cent. of volatile matter are safe. It is, however, true that the coals richest in gas show the most intense phenomena of ignition up to a certain point. This point, in the analyses given, seems to be about 22 per cent. A coal containing 27 per cent. of volatile matter showed less than half as much increase of flame-length as one containing 22 per cent. The latter percentage (or say from 16 to 24 per cent.) characterizes a class of coals which make much, and very fine dust; and this fineness has no doubt much to do with the behavior of the material. Finally, it has been proved that the rapidity with which a coal yields its gas is not in exact proportion to the amount it contains. The richest coals do not lose their volatile constituents most quickly when heated; and it is evident that this readiness to part with volatile constituents must be of supreme influence in the case under consideration.

"So much for the experiments upon coal-dust alone. These bear most directly upon the disputed questions involved. But no adequate comprehension of the real dangers to be feared in practice can be acquired without a study of the equally careful experiments with fire-damp, and with fire-damp in the presence of coal-dust. The latter invariably showed an increase of intensity in all observed phenomena. Thus, to take a single case, 15 kilogrammes of coal-dust, strewed along 10 meters of the gallery, and ignited with a clay-tamped shot, gave the following results as the percentage of fire-damp in the atmosphere was increased:

Fire-damp. Per cent.	Length of flame. Meters.	Force, as shown by movement of mine-car at mouth, on 4° up-grade. Meters.
0,	15.0	0.70
1,	15.0	0.90
2,	15.5	1.00
3,	19.5	1.25
4,	24.5	2.30
5, explosive	35.0	3.30
6, explosive	41.0	over 14.00

"It is therefore not improbable that dusts which are safe in the absence of fire-damp may be rendered dangerous in its presence.

"Among the latest experiments mentioned in this report were those conducted by means of the branch gallery, in which a confined explosive gas-mixture was exploded by the secondary effect of an explosion, produced in the main gallery, of a mixture containing only 5 per cent. of fire-damp, and hence not explosive *per se*, but rendered so by the presence of coal-dust. In this case, both the first explosion and the communication of it to the second mixture in the side gallery were the work of the coal-dust.

"The following points are considered to have been established:

"1. The presence of coal-dust, with or without small quantities of fire-damp, always increases the length of the flame from a blown-out shot.

"2. When fire-damp is wholly absent, this prolongation of flame is usually limited, not exceeding 6 to 15 meters for most varieties of dust when clay-tamping is used, and when the sides of the bore-hole do not themselves yield coal-dust and gas, or 9 to 21 meters when dust is used for tamping, or is produced by the hole itself. But there are coal-dusts which, once ignited by a shot, burn on spontaneously, giving flame far beyond the locality strewed with the dust, and sometimes actually producing explosions, where no fire-damp is traced.

"3. Small proportions of fire-damp render the combustion more intense. Fire-damp up to 3 per cent. moderately lengthens the flame of the dusts giving but a short flame without it. But 4 per cent. and upward of fire-damp prolongs such flames indefinitely; while the most dangerous dusts, even in small quantity and in an atmosphere containing less than 3 per cent. of fire damp, give real explosions.

"4. Secondary explosions may be caused by coal-dust, in the manner described above."

Continued Discussion.

(Bridgeport Meeting, October, 1894.)

R. W. Raymond, New York City: In connection with this subject, I would call attention to a book just published,* the author of which, Mr. Donald M. D. Stuart, is a mining engineer of Bristol, England. It contains a thorough discussion of the explosion which

* *Coal Dust as an Explosive Agent, as shown by an Examination of the Cumerton Explosion.* By Donald M. D. Stuart, F.G.S., Mining and Civil Engineer. New York, Spon and Chamberlain, 1894. Small 4to., 103 pp.; 7 large plates.

occurred in November, 1893, at the Camerton collieries in Somersetshire, and possesses special interest and value, not only by reason of its exhaustive character, but also because the explosion referred to occurred in a mine free from fire-damp, and is presumed to have been due to coal-dust alone. The accident afforded, therefore, a peculiarly favorable opportunity for studying the phenomena of coal-dust explosion.

I shall not undertake here to describe or discuss in detail the circumstances of this case, or Mr. Stuart's elaborate and ingenious argument in support of the theory he proposes; but I believe a brief summary of his method and conclusions will be interesting to members of the Institute, and will induce many to procure the book itself for more careful study.

The two Camerton collieries are situated about seven miles from Bath, and have been worked extensively, on six separate coal-seams, for a hundred years. The faulted geological structure of the field, the subsidence of large areas of roof, and the extensive goaves resulting from the operations of a century, combine to furnish the most favorable conditions for the collection of fire-damp, if it were ever generated in the coal or rocks; but this gas has never been discovered, either before or since the explosion of last November. The Camerton collieries, like all the others of the Radstock district, have been, and still are, operated with naked lights exclusively. These facts seem to preclude the hypothesis that the explosion was due to fire-damp; and this conclusion is confirmed by the fact that no carbonic acid was detected in the gas produced by the explosion. Certain phenomena, observed in the workings afterwards, pointed also in the same direction.

By a thorough analysis of all the observed facts, Mr. Stuart reaches the conclusion, that there was a series of ten successive explosions, which originated in a shot fired with gunpowder, and were propagated 279 yards with the air-current and 1278 yards against it. For the initiation and propagation of these phenomena, he proposes the explanation, that the heat developed by the explosion of the gunpowder was sufficient to distil the coal-dust in the immediate neighborhood, and even to dissociate the hydrocarbons thus set free, leaving free hydrogen as an explosive gas, and depositing carbon-dust; and that the hydrogen, coming into contact with oxygen in the air-current, suffered an explosive combustion, which generated fresh heat sufficient to distil and dissociate a new body of gas—this reaction occurring again and again, until so much heat had been lost

en route that the process could not repeat itself. The theory is supported by detailed chemical evidence and heat-calculations, based upon the researches of Sir Frederick Abel and Sir Andrew Noble, into the temperatures and products of fired gunpowder, and the experimental investigations of Prof. Victor Meyer, of Heidelberg University, and Prof. Vivian Lewes, of the Royal Naval College, Greenwich, into the temperatures of ignition of various hydrocarbons, and the results of subjecting these bodies to exalted temperatures. Concerning this argument, I will not express an opinion here. The conclusion is certainly novel and startling. Free hydrogen has not been conceived hitherto, so far as I know, to be present in the explosive gases of coal-mines; and the proposition that it may be produced by the simple heat of an ordinary shot, and may become the agent of an aggregate effect far exceeding the mechanical energy of the shot itself, challenges close and skeptical criticism.

Apart from the theoretical difficulties of the problem, the practical question at once arises: If Mr. Stuart's theory of this case is correct, how are we to explain the fact that no previous shot, in these collieries or in any others, is known to have initiated such a coal-dust explosion? In its final report (1886), the first British Royal Commission on this subject says very sensibly:

"If coal-dust were the principal agent in coal-mine explosions, every blown-out shot, occurring in a very dusty and dry mine, should actually be attended by a more or less disastrous explosion or conflagration; and looking, therefore, to the enormous amount of powder expended in shot-firing in this and other countries, and to the not inconsiderable proportion which blown-out shots must constitute in many localities of the total number of shots fired, disastrous coal-mine explosions should be of more than daily occurrence if this view were correct."

Mr. Stuart observes that this conclusion now requires to be revised; and there is no doubt that he has made out a strong case. But it is evidently an exceptional one; and the determination of its exceptional conditions is of the highest importance. This point is emphasized by the remarkable fact that the coal-dust from this district was tested, not long before the Camerton explosion, by Mr. H. Hall, whose report (1893) to the Royal Commission, shows that eight experiments failed to produce explosive ignition. In these experiments, coal-dust was scattered into an old shaft, free from fire-damp, and a cannon, charged with $1\frac{1}{2}$ pounds of gunpowder and lightly tamped with an equal volume of coal-dust, was fired up the axis of the shaft into the dust-laden air.

The only explanation which Mr. Stuart has to offer concerning the

different behavior of the coal-dust in the Camerton collieries, at the time of the accident, is the following:

"Probably, the two important changes at these collieries in recent years have been in the ventilation and the production of coal. Larger currents of air now pass through the ordinary roads at much higher velocities than heretofore. The output of coal has also been considerably increased; therefore, the larger number of loaded trams, in their progress from the coal-faces to the down-cast shaft, are swept by the opposing and accelerated currents of air. Coal-dust is, consequently, disengaged and deposited in the roads, and the air in passing over it evaporates the moisture and raises it to a dry and sensitive condition. The deposits of dust have, therefore, become greater in quantity and more sensitive in character in recent years, and these conditions have been set up without any features to attract notice. The failure of the drastic tests to obtain explosive phenomena with the coal-dust of the district, only a few months before the explosion, would also naturally divert attention from the subject."

The analysis of the Camerton coal shows 33.55 per cent. of volatile products, and the coal yields, per ton, 11,134 cubic feet purified illuminating gas of 18-candle power, when burned in the "London Argand," at the rate of 5 cubic feet per hour. The large volume and high illuminating-power of the gas indicates, as Mr. Stuart says, that "the coal-dust was a favorable material for yielding a gaseous mixture, and contained a good proportion of the higher hydrocarbons in the ascending scale of explosive combustion." But the material does not appear to be exceptional in these respects.

Thorough watering of the coal-dust in the vicinity, before firing a shot, is pronounced by Mr. Stuart to be probably the most effective precaution known at the present time. But he adds, that a mere dampening of the surface would probably not be sufficient.

Whether Mr. Stuart's chemical theory be correct or not, the fact remains, that a serious coal-dust explosion has taken place in a colliery supposed to be entirely exempt from such a peril; and, I heartily agree with the following passage, which I quote from a private letter of the author of this book:

"There is still a weight of opinion, that coal-dust itself is not dangerous, especially in non-gaseous mines, which derives strength from the fact that explosions in such mines are rare. But one has occurred, and we know not how soon others may follow; and with such calamities as the recent Albion explosion, with its death-roll of 290 victims, hanging over well-equipped and well managed collieries, I beg your support in drawing attention to the investigations and thoughts advanced in my volume, so that they may be read and considered, to ascertain if, haply, they may be found to elucidate the condition, and thereby suggest the remedies, for this grave danger to colliery enterprises everywhere."

David T. Day, Washington, D. C.: The book of Mr. Donald

M. D. Stuart, on *Coal-Dust as an Explosive Agent*, is most admirable for the detailed record of the eccentric effects of the explosion with which it deals. This occurred in a bituminous coal-mine at Camerton, England, where fire-damp has been proved absent during the many years since the colliery was opened. Part of the mine was dry and very dusty; part was damp. The explosion took place in a dry portion. There was nothing but coal-dust to cause the explosion. The interesting point to be explained is why a certain gunpowder-blast produced an explosion of the dust, and why countless previous blasts had never caused an explosion.

It is evident that the particular blast which effected the disaster differed from others (1) in being a partially "blown-out" shot, and (2) in having its axis so directed that the blown-out shot was discharged into dry, fine coal-dust at very short range. The studies of Mr. Stuart demonstrate this clearly, and thus add valuable data to this perplexing subject. The writer goes much farther, however, in developing an elaborate theory as to just how the coal-dust behaved during the explosion. This portion of the book adds to the general interest by its ingenuity; but I cannot see that the data bear any proportion to the theory.

No one who reads the book will doubt that Mr. Stuart considers the distillation of the volatile matter of the coal by the explosion of the gunpowder to be the principal condition of the explosion, and, further, that the distilled hydrocarbons were essentially dissociated into hydrogen and soot. He considers that this hydrogen travelled more than one hundred yards and then exploded. There, its explosion distilled more coal and furnished gases for more explosions. This theory is developed from (1) the fact that the explosion was very violent at particular points and of comparatively slight violence at others; (2) the observation that a cotton bag which contained food, and which was left in a refuge-hole, was not so much burnt as to destroy its texture; and (3) the statement that carbonic acid was not found. Mr. Stuart argues that if the bag was not completely burned, there could not have been any flame; and that, carbonic acid not being found, the only product of combustion must have been water; therefore, hydrogen must have been the explosive gas, which is traced to the coal-dust by the hypothesis of distillation. But, as a matter of fact, carbonic acid may have been present in considerable quantity, escaping notice because it was not carefully looked for. The only evidence of its absence is the fact that there was not enough to extinguish the lights of the exploring-party,

several hours after the explosion. The statement as to the absence of carbonic acid is therefore not well founded, and this is disappointing after the laborious citations of authorities on heat-phenomena, gunpowder-explosions, etc.

A simpler explanation of the explosion would seem to me to be that the flame from the gunpowder set fire to the coal-dust in the presence of a considerable amount of air in the well-ventilated tunnel. This burning kept up along the tunnel, and was not very violent, because the combustion was incomplete, and the air and dust were not well mixed. Where the mixing was more complete, the explosions were more violent.

Such a theory would do something toward explaining why this shot produced the explosion when all the others—especially a heavier shot earlier in the same night—had not done so. The axis of this earlier shot was directed more nearly in the line of the tunnel, and it did not blow out, so that its contents were cooled before it struck the mass of dust.

The observation that the food-bag, 35 yards away, was only carbonized, does not seem to me to prove that flame was absent. In the first place, it was out of the direct line of the explosion, in a "refuge-hole" in one side of the tunnel, and its condition is of no great significance, especially if we suppose the combustion to have been incomplete. From such incomplete combustion, carbon monoxide would be expected as the main product. Mr. Stuart ventures the statement that carbon dioxide is an essential product of the explosion of hydrocarbon gases, although it is well known that these products may vary from water and carbon dioxide, as products of complete combustion, to mixtures in various proportions of hydrogen, solid hydrocarbons mixed with free carbon (soot), unburnt gaseous hydrocarbons, carbon in the form of coke, carbon monoxide, carbon dioxide, water, etc., according to the stage of incompleteness of the combustion. We recognize it as impracticable to produce even experimentally any single product such as hydrogen (which Mr. Stuart claims) as the result of distillation, except by heating for a considerable length of time at the highest temperature attainable. It is not necessary to use such a complicated distillation-theory to explain the presence of soot, the common indicator of incomplete combustion, *i.e.*, a smoky flame. As the explosion proceeded, its heat must, of course, have distilled some of the coal-dust; but it should be noted that, after long heat-calculations, Mr. Stuart admits that the slight amount of heat in the gunpowder would only account

for a trifling percentage of the energy necessary to carry the gases to the place where he supposes the first explosion to have taken place. He admits that some combustion must have taken place, but he prefers to leave in the background this source of nearly the whole effect.

It appears to me that Mr. Stuart's theory is unnecessary, and by suggesting a simpler though certainly less interesting explanation of the method of the explosion, I hope that the reader will be less diverted from the valuable lesson of the disaster, which seems to be that a blown-out shot, discharged at short range into a mass of dry dust, may produce an explosion. The violence of this depends upon the completeness of the combustion. It is most violent when the partially burned gaseous products become more thoroughly mixed by diffusion, eddying, etc., and form gaseous detonating mixtures with the usual violent results. This explanation would not necessarily exclude anthracite coal-dust, as Mr. Stuart's theory does.

W. F. Durfee, West New Brighton, Staten Island: I fully agree with Dr. Day's general conclusions in this matter, and, *apropos* of the whole subject, I am reminded of the explosion of flour-dust in the great Washburn mill in Minneapolis, which occurred May 2, 1878.

This mill was one of the largest in the country. The walls were of stone, seven feet thick at the bottom; yet instantly that enormous fabric was a wreck. The proprietor of the mill employed Prof. L. W. Peck to make a careful study of the disaster; and his conclusion was that it resulted from the ignition and explosive combustion of the flour-dust with which the atmosphere of the mill was surcharged. He tried a number of experiments which fully justified that conclusion. He used carbonaceous dust of various kinds, sugar-dust, charcoal-dust and flour-dust, which he placed in a strong box and then fastened down the lid of the box. In every instance, when flame was used for ignition, the lid was blown off with a violence of explosion in proportion to the amount of dust present in the atmosphere of the box.

In a lecture delivered by Prof. Peck he gives the following illustration:

"If a large log of wood were ignited it might be a week before it would be entirely consumed. Split it up into cord wood and pile it up loosely it would burn in two hours. Split it up into kindling wood, pile it up loosely and, perhaps, it would burn in less than an hour. Cut it into shavings and allow a strong wind

to throw them in the air, or in any way keep the chips comparatively well separated from each other, and the log would, perhaps, be consumed in two or three minutes. Or, finally, grind it up into a fine dust, as powder, blow it in such a manner that each particle is surrounded with air, and it will burn in less than a second."

In the matter of the explosion at the Pocahontas mine some years ago (March 13, 1884), Messrs. J. H. Bramwell, Stuart M. Buck and Edward H. Williams, Jr. (a committee appointed by the President of this Institute, see *Trans.*, xiii., pp. 237 to 249), reported: "We have obtained no direct proof of any past occurrence of fire-damp sufficient of itself to account for even a slight explosion and are forced to believe that the explosion was due either to dust alone or to dust quickened by an admixture of fire-damp too slight for detection by any ordinary means."

The statement of this committee (page 248) that in two instances "a body of flame has been described as flashing back 60 to 100 feet from the head of an entry, following the firing of a shot, without injury to the men," calls to mind an explosion which occurred in a confectionery establishment in Barclay street, New York, a few years previous to that in the Washburn mill. The investigation by the coroner did not result in any satisfactory conclusion; but it was in evidence that several times, during a period of a year or more before the explosion, there had been flashes of flame observed to shoot across the floor, but no explosion followed. In the particular instance under investigation it was shown that the atmosphere was very much filled with sugar-dust, and the whole front of the building was blown out. Until they had got down to the bottom of the *débris*, it was supposed that the wreck was the result of a boiler-explosion; but that was found not to be the case. There is no doubt in my mind that the explosion was that of starch and sugar-dust mixed with air.

In 1881, there was an explosion, or rather a series of explosions, in the Albion mine, which was investigated by Mr. Gilpin, the Inspector of Mines for Nova Scotia. The mine was thoroughly ventilated, and an hour before the explosions began was reported to be practically free from gas; yet Mr. Gilpin says:

"The explosions once begun were continued at intervals, until the mine was all aflame and had to be flooded. . . . . On examining the gallery shortly after the original explosion, dead bodies of men and horses were found six hundred yards from the shafts and the wood work was splintered; but nothing bore any mark of fire, and the conclusion was plainly justified that the flame of the explosion had not extended thus far. The walls of the galleries had been swept clear of

timber and had the appearance of having been brushed with a broom. Volumes of coal-dust had been driven along by the force of the blast and lay in waves and drifts on the floor of the levels. . . . . It was found that clouds of the finer particles had been carried to the shaft and beyond it into the main north level, where a secondary explosion had taken place."

This second explosion "was half a mile from the first, with an intervening space of at least a quarter of a mile, known to have been free from flame." The conclusion is drawn "that the fine dry particles were driven on by the force of the first explosion across the shaft into the 'lamp-cabin,' where they were ignited by the lamp, which was kept burning openly, and thus caused the second explosion; and it is probable that the same agency was efficient in producing, or, at least, augmenting, the subsequent explosions that made it necessary to flood the mine."

The illustration of Prof. Peck above quoted, suggests the burning of ground or dust-coal in "the dust-fuel furnace," into which coal-dust is blown and consumed so rapidly as to furnish abundance of heat for melting steel in its open hearth. Any one who has studied one of these furnaces in operation would have no difficulty in believing that a mixture of coal-dust and air, if in sufficient volume, would explode disastrously when ignited. After all, what we call explosion is but exceedingly rapid combustion; and gunpowder owes its efficiency as an explosive to the fact that every particle of charcoal-dust has, in the niter surrounding it, the oxygen to cause its excessively rapid or explosive combustion.

WILLIAM KENT, Passaic, N. J.: Suppose we admit the possibility of the truth of the theory of Mr. Stuart that the hydrocarbon gas could be dissociated into hydrogen and solid carbon, and that the hydrogen went on until it met a body of air and exploded with it. It is reasonable to suppose that that body of hydrogen when it exploded would generate heat which would distill another body of gas from the surrounding coal and would distill it as hydrocarbon gas. Suppose the heat were sufficient to dissociate this gas into hydrogen and solid carbon, yet around the margin where this heated gas must have been cooled by the comparatively cold body of the mine the temperature could not at all rise to that of dissociation. At this point, if the gases were burned at all, carbonic oxide or carbonic acid would have been formed. It is perfectly inconceivable that there could be an explosion creating such an intensely high temperature everywhere up to the margin of the walls, which would absolutely deposit all of the carbon in that hydrocarbon gas and leave none of it to be burned as hydrocarbon. I

think it a sufficient theory to suppose that the solid coal was distilled into hydrocarbon gas, which may be done at quite a low temperature (some hydrocarbon gases are generated at a low degree of heat), that these hydrocarbons were burned in the ordinary way into carbonic oxide or carbonic acid, and that one or the other of these gases must have been there, although it was not found. It seems reasonable to suppose that after the explosion there would be great currents of air rushing in and sweeping out the gases of combustion and that the parties who went to get samples of the gas afterwards did not find carbonic oxide or carbonic acid and that they may have been there immediately after the explosion.

But I think that the successive explosions may be accounted for without the hypothesis of the dissociation of CH_4 as follows:

The first explosion would take place at a point where there existed an explosive mixture of CH_4 and air, and would extend no further than the point at which the air did not contain enough gas to be explosive. The resulting gases of combustion would form a hot wave of CO_2 and nitrogen, which would drive ahead of it the mass of cold air. This intensely hot wave, in passing over broken coal and dust, would distil out a fresh supply of CH_4, which, however, would not immediately burn because surrounded by incombustible gases, but would be swept along with the wave. Finally, by the rolling, tumultuous motion of the wave, a body of CH_4 might become mixed with the air in front, and ultimately be fired by contact with a still hot portion of the gases of combustion. The explosion which would immediately result would generate another body of hot CO_2 and N, which would proceed as before.

(Later communication to the Secretary.) Having had an opportunity to read carefully Mr. Stuart's book, I offer a few additional comments upon it.

On page 44 he says:

"These activities would leave the atmosphere in their rear in an attenuated condition, and when No. 1 gaseous explosion occurred, there would be an instantaneous rush into this attenuated atmosphere, driving the intumescent dust that had not subsided against the thin opposing faces of the timber."

The atmosphere was probably not "attenuated," but rather a *plenum*, due to increased pressure caused by combustion of the gases. The dust would be as likely to be deposited on the off-sides of timbers, through eddies in the currents, as through being driven against them by the current.

Again, on page 53, the author says:

"If it be remembered that the temperature of the exploded gunpowder largely exceeded the highest point touched in Prof. Lewes's experiments, the character of the gaseous body produced from the coal-dust becomes revealed."

The argument is that hydrogen was formed. Prof. Lewes's experiments showed that, at 1500° C.,

Methane is decomposed into 78.66 H, 19.22 hydrocarbons.

Ethane is decomposed into 73.35 H, 20.62 hydrocarbons.

Ethylene is decomposed into 62.68 H, 27.80 hydrocarbons.

The argument depends upon a calculation of the temperature of exploding gunpowder (p. 76), about 2231° C.

Mr. Stuart does not seem to take into account the dilution of the products of explosion of gunpowder by the surrounding air, and their consequent cooling, nor the cooling due to the distillation of the hydrocarbons of the coal, to say nothing of the further cooling by the dissociation of the hydrocarbons, if any such dissociation there was.

The distillation of hydrocarbons from coal, and their dissociation into H and C in a chamber kept at a high temperature, which supplies the heat-units absorbed by distillation and dissociation, is a very different thing from what is likely to take place in a large open chamber, full of cool air and surrounded by cool walls.

On page 38 it is stated that the quantity of heat generated by the explosive combustion of the charge of gunpowder in No. 2 shot was 354.375×516.8 or 183,141 units of heat. This looks large, being expressed in gramme-centigrade heat-units, but translated into British thermal units (1 gramme-calorie = .00397 British thermal units), it amounts to only 727 British thermal units, or less than enough to boil 1 pound of water.

The temperature of the products of combustion of this No. 2 shot "is found to be 2896° C." (p. 37). This is the result of a theoretical calculation, dividing the heat-units due to the combustion by the heat-units required to raise the temperature of *the products of combustion* 1° C. But the products of combustion are not all that is heated by the heat generated in the explosion. As soon as formed, they are discharged into the surrounding air, which is heated by them, while they are correspondingly cooled.

The argument of the book, tending to show that the explosions were the result of the burning of hydrogen dissociated from the hydrocarbon gases of the coal, and not of the direct burning of these gases themselves, rests:

(1) Upon a theory or calculations showing a temperature following the explosion of the gunpowder sufficient to cause dissociation. This theory, I think, the author fails to prove.

(2) Upon the non-discovery of CO_2 after the explosion. The attempt to discover CO_2 does not appear to have been made until eight hours after the explosion (p. 62), the full force of the intake-current having been directed into the body of stagnant gases. The gaseous mixture that filled the roads was suffocating, pungent, and irritating. How would burning H produce such a mixture? It was probably CO, CO_2, and SO_2, with pyroligneous acid, or some such substance produced by partial distillation of the timbers. The failure to discover CO_2 by the appearance of the lights eight hours after the explosion, and after the full forces of the intake-current had been directed into the stagnant gases, does not prove that CO_2 did not exist immediately after the explosion.

While it is difficult to accept the theory of the author that free hydrogen was formed during the progress of the series of explosions, and it is probable that distillation of the hydrocarbons and their burning is a sufficient theory, this work is of great importance in calling attention to the danger of coal-dust, even in a mine supposed to be free from gas.

A. R. Ledoux, New York City: It may be interesting, since the well-known fact of the explosions of flour-mills has been mentioned, to mention an explosion, which occurred at Chicago some years ago, in the Chicago Sugar Refinery, where starch exploded. In one of their departments they were making dextrine by heating starch to a high temperature. The room was full of starch and the dust settled upon the cylinders. It was the duty of one of the men to watch for fire, because when the mixture reached a certain temperature there would be a glow; but no danger was ever anticipated from it. Just before this explosion took place the superintendent noticed behind one of the rollers that the starch which had fallen upon it was glowing, and he took a fire-extinguisher and turned a stream of water upon it. The stream made a small spark, and instantly an explosion followed, and the whole building went up and a number of men were killed. Fortunately for himself and for science, the superintendent was not killed and was able to report this fact.

Charles Kirchhoff, New York City: In the manufacture of lignite briquettes, which is an important industry in Germany, and, I believe, in Austria also, dust-explosions have proved practically

troublesome. A note on the subject will be found on page 1173 of the current volume of the *Zeitschrift des Vereins Deutscher Ingenieure.*

Edward H. Williams, Jr., Bethlehem, Pa.: The point made by Mr. Stuart is not new. After coming home from the investigation of the Pocahontas explosion, I had one of my seniors collect a lot of dusts of coals, with varying percentages of volatile matter, and see how much of that matter was given off at low temperatures. I had formed the idea that the heat generated by a blown-out shot was sufficient to disengage so great an amount of gas as to extend the flame to the dust-deposits on the walls of the chamber and to retain sufficient heat to discharge from those dusts sufficient gas to carry on the work, as in the Pocahontas affair there were distinct separate explosions in the chambers near the origin, although those chambers were closed and had no connection with one another. To be brief, we found that the greater part of the volatile matter was given off below a red heat. This was checked about five years ago by another set of experiments; so that I am sure that certain coals are dangerous from the fact that they give up their gas readily and at once. We flashed the dust on the walls at Pocahontas with a naked lamp, and one of the miners said that he thought for some time that he would try to make gunpowder with such explosive dust, as it would explode in the cold, almost.

There is another fact about dust-explosions that has not been much discussed, and that is that 20 per cent. of the after-gas from exploding gunpowder is explosive, and, in a tight heading, this alone may prolong the flame of a blown-out shot. This was the case at Pocahontas, at a point where there was no dust; yet the flame ran back 120 feet through a heading with no cross-cuts, where the ventilation was by diffusion only, and set fire to a keg of powder and killed a man.

In some of my experiments the greater part of the volatile matter went off at heats so low that they could be called "black." We do not need to suppose any separation into elements. The heat necessary to such a change would show its presence in the neighborhood by the coking of the solid coal on the surface of the rib; and, in fact, if such were to be the case, we would be on the track of perpetual motion, for such changes would go on forever.

CLOSE SIZING BEFORE JIGGING.

Discussion of the Paper of Prof. Richards. (See p. 409.)

(Virginia Beach Meeting, February, 1894.)

Henry Louis, London, England (communication to the Secretary): Prof. Richards's paper has impressed me as highly valuable. Without entering upon any discussion of it as careful and thorough as it deserves, I venture to mention one or two points which a perusal of it has recalled to my mind in connection with my own study and practice of ore-dressing.

I always mentally divide jigging into two phases:

1. A particle at rest on the jig-bed is lifted and carried upwards by the rising current of water, at an increasing velocity, which tends to a maximum. When the upward current is stopped, the particle continues to rise with rapidly diminishing velocity until it comes to rest. This completes the first phase.

2. In the second phase, the particle falls from its position of rest with an increasing velocity which tends again to a maximum, and continues to fall until the rising current of water has reduced its velocity to zero. This completes the second phase.

The second phase will evidently be greatly modified by the effect of suction, as Prof. Richards shows; but I do not see that his experiments very clearly imitate the action of the second phase. And the brief intervals at the end of each phase, when the mineral particle, by reason of its acquired velocity, is still moving, though in opposition to the water-current, yet remain to be investigated. I feel especially, in Prof. Richards's paper, a difficulty which I have felt with regard to all similar previous investigations, namely, that the experiments deal with particles falling a long distance, when the velocity becomes uniform. I question whether this uniform maximum velocity is attained in actual jigging-practice. If so, it is certainly for a comparatively very small proportion only of each stroke. It seems to me that an investigation like that of Prof. Richards, but confined to the first 6 or 8 inches of fall only, and carried out with suitably delicate recording-apparatus, is what we

really require for the satisfactory final solution of the problem involved.

Perhaps my meaning may be made plainer by the accompanying diagram of a jig, in which A B represents the sieve, C D the water-level, and E the point of discharge. It is clear that if the heavier body (say, galena) delivered at D falls so far during the one phase that it cannot be lifted above E F in the other; while the lighter body (say, quartz) either never falls quite through the distance, D F, or falls only so far that the other phase lifts it above E F, then perfect separation will be effected. Obviously, all the separation need only take place within narrow limits about the line E F; and hence, long distances of fall and lift are beside the question. In working, for instance, with particles 4 mm. ($\frac{1}{6}$ inch) in diameter, the jig would probably be run at about 90 strokes per minute, and each phase would last $\frac{1}{180}$ minute or $\frac{1}{3}$ second. According to Rittinger,

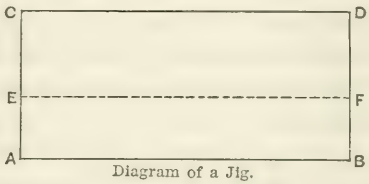


Diagram of a Jig.

a sphere of galena requires fully $\frac{1}{2}$ second to arrive at its maximum falling-velocity, while quartz would reach the same limit in less than $\frac{1}{4}$ second. It seems to follow that conclusions based on uniform maximum velocity only must, in this case, be misleading. What we really need to know, as the basis for any consistent theory, is what occurs during each hundredth of a second for the first second of lift or fall. With that knowledge, we should be able to trace the effect of the jig on individual particles. But I should not be surprised to find that the attraction of small particles for each other had some effect in the action of the jig; and there are probably numerous other disturbing influences.

I fully appreciate and agree with the statement of Prof. Richards that each mineral has its "individual personal equation," and cannot be classified by specific gravity alone. I take it that Prof. Richards does not altogether agree with Prof. Munroe in the condemnation of close sizing. Personally, I have never regarded Prof. Munroe's argument on that question* as conclusive, because it seemed to

* *Trans.*, xvii., 637, *et seq.*

me that the experiments on which it was based differed from actual conditions on the jig-bed in two important particulars: (1) his tubes were fixed, while on the jig-bed the "tube" formed by larger particles must be moving, and moving more rapidly than the particles falling within them; and (2) his tubes were continuous, so that water displaced by the falling body could only ascend in the space between the body and the tube, whereas, in the jig, water so displaced can escape laterally. These two considerations seem to me to invalidate the deductions from the experiments, and recent experience of my own indirectly confirms my impression. I was getting poor results from rough methods of dressing tinstone, and secured excellent efficiency by introducing close sizing. The illustration is not entirely pertinent, because I was rather buddling than jigging; but I think it indicates the direction in which one always looks, and should look, for improved work in ore-dressing. My criticism of Prof. Munroe's argument, however, is purely theoretical, and Prof. Richards's experiments are, of course, free from this objection.

The results he has obtained as to the utility of suction are a great surprise to me. I had always looked upon suction in a jig as injurious rather than beneficial.

Prof. Richards (communication to the Secretary): As to what happens in jigging, including the up-and-down motion of the water and of the sand, while I agree with much that Mr. Louis says, I find some points in which I differ. For example, at the end of the second phase his falling grains are brought to rest, preparatory to rising again, by the action of the pulsion-water. I have never seen a plunger or movable-sieve jig in which the whole sand was not reposing upon the sieve for about half the time. The quick-down-slow-up devices of plunger-movement curtail the period a little. Pulsion-jigs (the *Setzpumpen* of Rittinger) may remove entirely this period of repose, according to the quantity of water used.

It seems to me that Mr. Louis does not notice that I have separated pulsion from suction and have studied each by itself to ascertain, as far as possible, the final effect of each. Let me review, in a few words, the steps taken. In studying pulsion I did, to be sure, use the tall 30-inch tube with continuous stream, which he condemns; but I did it in order to throw light upon Prof. Munroe's interstitial currents, stripped of all disturbing factors. Of this work he remarks: "Conclusions based upon uniform maximum velocity are misleading;" and again: "I feel especially, in Prof. Richards's paper, a difficulty . . . . namely, that the experiments

deal with particles falling a long distance, when the velocity becomes uniform." These views I myself held, and accordingly I tried the next set of tests upon Rittinger's accelerated and retarded currents (pages 458 to 463). In these experiments I used my pulsion-jig, Fig. 8, which can give slow or rapid pulsations, controlled by the speed of rotation of the cock, varying from 50 to 800 revolutions per minute—a far greater range than Mr. Louis asks for—while the grains may be allowed to settle back upon the sieve for a short period of rest during each stroke, or not, as desired, by the regulation of the water. In my tests with more rapid pulsations uniform velocity was never attained. In fact, though the total height of my jig-bed was 30 inches, the extreme amplitude of my vibrations was not over one-eighth of an inch. It was, therefore, smaller in amount and more rapid in occurrence than those of most of the jigs to which Mr. Louis refers. I used variations in both speed and water and found that they all, whether fast or slow, gave me results that to the eye were exactly the same as those of the steady column of uniform velocity. Two results are recorded upon minerals varying widely in specific gravity, namely, quartz and galena (Plate XIV.) and quartz and sphalerite (Plate XV.).

Mr. Louis says: "We really need to know what occurs during each hundredth of a second." This information is very difficult to get, and, if obtained, would be very difficult to put together so as to compute a resultant of all the forces. I believe I had just such a result in my Plates I. to XII., obtained by a practical integration; for I have tried with my pulsion-jig (Fig. 8) slow, large vibrations and also quick, small vibrations, and obtained Plates XIV. and XV. under both conditions. I have also tried infinitely quick, infinitely small vibrations (another name for uniform velocity), and I get Plates I. to XII. (compare Plates II. and XIV.; also Plates X. and XV.).

I come now to a second point upon which my experiments do not bear out Mr. Louis's opinion. He says that in jigging all the separation need only take place within narrow limits about a parting plane indicated at E F in his figure. Let us see if this is true. If a grain of quartz be placed upon the sieve, A B, the effects of pulsion will quickly carry it up above the plane, E F, and if, on the other hand, a grain of galena is placed on top of the sand at C D it will quickly be carried by pulsion to a point below the plane, E F. The zone of action, therefore, clearly extends from the sieve, A B, to the surface, C D, or through the whole height of the column of

sand. To draw an outside analogy, if my mercurial barometer gives me too short a scale for my meteorological studies I obtain a long one by the use of the water barometer. So, in jigging, if the little 3-inch to 5-inch bed of a jig is too low to study the principles of pulsion, I lengthen my scale and go to 30 inches, which gives exactly the same result, only so long drawn out that it may be intimately studied in all its bearings. Mr. Louis remarks that investigations like mine made on 6 or 8 inches of column only are needed. It is true that such investigations could be attempted; but they would, I think, give much less perfect results than those recorded in Plates I. to XV. of my paper.

Mr. Louis concludes with the remark that my results as to the utility of suction are a great surprise to him, as he had always looked upon suction in a jig as injurious rather than beneficial. With this statement I heartily agree (see page 485), provided that closely-sized ore is referred to; but in the jigging of mixed sizes uction is of great value, advancing the work of jigging even on such minerals as sphalerite and pyrite, yet not enough to do away with the advantages of close sizing upon them. But on such heavy minerals as galena, when no serious quantity of middle-weight mineral or included grain is present and the slimes have been removed, pulsion aided by suction can do the whole work without close sizing. I believe that, in a mill where copper pyrites, for example, is being concentrated, close sizing should be adopted down to some point to be determined, let us say $\frac{1}{8}$ inch (3 mm.) in size. The fines will go to a separator with two or three spigots, the product of which will be sent to jigs. We shall then have the jigs which are concentrating the stuff from the drum-screens and those which are concentrating the stuff from the spigots of the separator. The former jigs should have the minimum suction (a pulsion-jig, even, would treat this material, and do it most rapidly, if the sizing were close enough), while the separator-jigs should have the maximum suction that a Harz jig can give, because they are jigging mixed sizes and need to supplement the work of pulsion by suction.

W. B. Kunhardt, New York City (communication to the Secretary): Every one interested in ore-dressing will thank Prof. Richards for his keenly critical and exhaustive investigation of the laws of jigging and will follow with rare pleasure the series of brilliant experiments by which the author has shed so much additional light on the subject. That the extent to which sizing should be carried before jigging has given rise to much discussion—indeed, that it has

been a matter of running debate ever since Rittinger's day—is well known. If the author's endeavor to determine, by independent research, the relative value of the several laws that have been recognized by different authorities as influencing the action of jigging should accomplish no more than to bring theory abreast of the most advanced practice, the arduous labor of preparing this paper would still be well repaid; for the clear insight that is obtained through it into the *rationale* of practice cannot fail to be fruitful in suggestions for further progress.

That portion of the paper dealing with the law of interstitial currents will be the first, possibly, to prompt the reader to an inquiry. The diameter-ratio which keeps galena and quartz in equilibrium, after the interstitial currents have done their work, is placed (p. 451) at about 1 : 6. Manifestly this is only the average ratio; a glance at Plate II. *b* and at Table XXVI. shows us that equilibrium in a rising current is obtained over a much wider range of sizes. In the case of bulb 5, for example, disregarding minute portions of galena, the range extends from 12-mesh to 100-mesh. Since the extreme diameter-ratio may be quite as important in practice as the average ratio, and since, moreover, the ratio is affected by comparatively slight variations in the conditions by which equilibrium is produced, does it not seem probable that the discrepancy to which the author draws attention, between his own and Prof. Munroe's results, is really less than his figures make it appear?

Again, accepting the author's diameter-ratios for grains held in equilibrium by a slowly rising current in the apparatus shown in Fig. 5, is it clear that the experiments made with that apparatus reproduce the interstitial currents obtained by jigging? The author holds that the rising current in these experiments is the equivalent of the upward flow obtained by pulsion in the jig. Can we so consider it? As the water rises through the sieve of the jig it seeks paths of least resistance in its effort to pass through the material on the sieve. Assuming this material to be evenly distributed, the selection, though thereby restricted within narrow limits, will yet result, not in one uniform rising current but in many differentials of this current—so-called interstitial currents, differing slightly in their relative intensities; each being influenced by the character of the grains it encounters and by the distance of these grains from their neighbors. From the size of the author's apparatus (Fig. 5) and the quantity of material experimented with, it does not appear probable that the rising current he obtained could have been sensibly affected by the proximity of the grains to one another or that any true inter-

stitial currents, in the sense here applied to the term, were produced. The conditions of the experiments seem to have been those peculiar to the *Spitzlutte* rather than to the jig. If we do not regard the aggregate of all the interstitial currents as the equivalent of one uniform rising current, but take into account the true interstitial factor —that is, the size of the falling grains as compared with the freedom of passage for the water rising around them—we shall be disposed to attach to the retarding effect of interstitial currents upon the large quartz grains even greater importance in jigging than the experiments seem to warrant, thus placing the average diameter-ratio for galena and quartz considerably below the author's ratio of 1 : 6.

Prof. Richards (communication to the Secretary): With regard to the question of diameter-ratio raised by Mr. Kunhardt, who, observing that my ratio of 1 : 6 (p. 451 of my paper) is manifestly only the average diameter-ratio which keeps galena and quartz in equilibrium, points out that in Plate II. *b*, column 5, for example, the range extends from 12- to 100-mesh, I would say that Mr. Kunhardt is quite right in throwing out the tiny heaps below 100-mesh. These are undoubtedly worn off from the larger grains during the sifting-process. He is right, too, in assuming that the result is an average of diameters (see p. 451, where the method of computing the ratio 1 : 6 is given). But if we pay attention (p. 454) to the physical features of the grains composing the heaps (see Plate II. *b*, column 5) we shall see that the heap of quartz on 12-mesh is composed of flattened grains, that on 14-mesh is less flattened, those on 16 and 18 are rough cubical grains. Looking at the galena we have the heap on 60-mesh composed of flattened grains of galena, that on 80-mesh less flattened, while that on 100-mesh is composed of cubical grains. We may justly assume, then, in this column, that the 12-mesh flattened quartz grains are there to balance the 60-mesh flattened galena grains, while the 18-mesh roughly cubical grains of quartz are there to balance the 100-mesh cubical galena and the intermediate quartz sizes and figures to balance the intermediate galena sizes and figures. Now, Prof. Munroe, in his investigation, discusses particles of like figure, for example, spheres or cubes of galena with spheres or cubes of quartz. If, then, I would investigate his law of interstitial currents, I must do the same. I can get the ratio of the flat quartz to flat galena, and also of cubical quartz to cubical galena, by getting the average of the diameter of each mineral and computing the ratio of the one to the other. This I did and got 1 : 6 as the ratio. It is clear that if my extreme of flattest grain of quartz were mated with my

rounded cube of galena, a few grains could be found with a ratio that might approach somewhat nearer to Professor Munroe's ratio than 1 : 6; but that is not the question at issue. Mr. Kunhardt's point is a good one; namely, that we must consider the extremes in practical jigging, since that is what we have to deal with.

Mr. Kunhardt suggests a doubt whether the slowly rising current in the apparatus, shown in my Fig. 5, reproduces the interstitial currents made by jigging and also whether the rising current is the equivalent of the upward flow made by the pulsion of a jig. And he points out how variable are the currents in a jig, resulting "not in one uniform rising current but in many differentials of this current." In reply to this, I would say that I sought to establish the laws of jigging by studying one current. I could not have studied them upon several varying currents at the same time. I ran the water in my pointed tube (Fig. 5) now fast and now slowly. I gave it every speed from that which separated the particles considerably to that which scarcely gave motion enough to effect a separation. Between these two extremes I could see no difference in the balance of grains in equilibrium. Between these two extremes I must surely have hit upon nearly every variety of interstitial current a jig ever has to deal with. In fact, it would seem from the results obtained that I had the true interstitial currents over the whole range of speeds.

Mr. Kunhardt thinks the currents of these experiments are those of the *Spitzlutte* rather than those of a jig. Before making the investigation I should certainly have endorsed that statement in just those words. I now endorse it with my important qualification, namely, that one of the most significant facts proved by my experiments is that the ultimate effect of pulsion in a jig is identical with the equilibrium in the upward flow of a *Spitzlutte.*

Mr. Kunhardt says that as the water rises through the sieve of a jig it seeks paths of least resistance, resulting not in one uniform current but in many, "differing slightly in their relative intensities; each being influenced by the character of the grains it encounters," etc. This I endorse entirely. One can see it on every jig. In my opinion, any interference of this kind will simply detract from the ratio obtained by my experiments. My ratios are the maximum values that can be obtained on perfectly-working apparatus. The practical jig will be liable to have interferences, which will lessen the ratio upon it so far as the pulsion alone is concerned. The moment the influence of suction is taken into account the whole matter is changed.

Walter Renton Ingalls, New York City (communication to the Secretary): Prof. Richards's paper is one of the most important contributions made to the science of ore-dressing since the publication of Rittinger's monumental treatise. The data which he has presented will be indispensable to every engineer engaged in the mechanical enrichment of low-grade ores. It may be that certain of his figures will need further verification; but in default of a thorough analytical study of them it is impossible to make specific criticisms. Probably the largest variations may be found in those introducing a personal factor, which has been a large element in many of the computations; for example, in the determination of the equal-settling ratios. Although Prof. Richards's preference for timing nearly the slowest of free-falling grains, rather than absolutely the slowest, appears to be well-founded, the gauging of the passage of a certain proportion of grains (say 90 per cent.) past a fixed mark must be largely dependent upon the judgment of the observer, and is, therefore, likely to be more or less inexact. The lagging behind of the last grains of a lot settling in water is well shown in the comparison of the parabolas of fall, where the $\frac{V^2}{D}$ of Rittinger's results for slowest grains is uniformly less than the result of Prof. Richards's measurements, except in the case of native copper. On the whole, the new factors for equal-settling grains seem more likely to be correct than those which have been adopted hitherto. The discrepancy between them, however, is by no means so great as that which appears with respect to the effect of interstitial currents (interstitial retardation seems to be a better expression), which was first calculated by Prof. Munroe and is now recalculated by Prof. Richards; but a comparison of the methods employed in making these deductions leaves little doubt that those of Prof. Richards are the more probable.

Perhaps the most important part of Prof. Richards's paper, inasmuch as it deals with laws which have not yet received much attention from investigators, is that which relates to the influence of suction in jigging. The results which have been attained by these experiments on a small scale are in general the same as have been deduced from jigging on a large scale. In certain cases, notably at Lake Superior and at Bonne Terre in this country, where there is a separation of only two minerals of great difference in specific gravity, the interstitial ratio being large, jigging mixed sizes is carried out successfully without a preliminary sizing by screens. Possibly the most remarkable examples of this principle, however, are to be found in

the work of hand-jiggers at various places upon various kinds of ores. On the other hand, with complex ores, involving the separation of at least three minerals, where the interstitial ratio is low, it is unquestionable that close sizing is necessary, though, perhaps, not to such a degree as has been thought in Germany and elsewhere on the continent of Europe. It is now recognized, however, that the principle has been exaggerated there, and a strong tendency towards its modification has been developed. The extent to which sizing was formerly (and is still) performed is shown by the following screen-sizes of some large German and Belgian works, dressing ores of the galena-blende-quartz type:

Bleyberg: 35, 28, 25, 22, 20, 18, 15, 12, 10, 8, 7, 6, 5, 4, 3, 2.

Silberau: 13, 8, 5, 3, 2, 1, 0.5.

Clausthal: 32, 17.78, 13.44, 10.00, 7.50, 5.62, 4.22, 3.16, 2.37, 1.78, 1.33, 1.00.

Schulenberg: 20, 12, 10, 6.4, 4.2, 2.6, 1.6, 1.0.

Freiberg: 30, 16, 12, 9, 7, 5, 4, 3, 2.

The limitation to the calculation of screen-sizes from Rittinger's formulæ for equal-settling grains lay in the irregularity of the grains. Thus the fastest grain of a light mineral may settle much faster than the slowest grain of a heavy mineral, both being sized between screens, which ought to permit a complete separation if all the grains of each mineral had actually their mean falling-velocities; but, the case being one not of means but of extremes, it is obvious that, in some instances, in order to effect a separation the screen-sizes would necessarily be closer than those at Bleyberg-ès-Montzen, or beyond the range of practicability. Some striking examples of variation are to be found in Prof. Richards's Tables III. and IV. The most noteworthy is the case of native copper (sp. gr. 8.479), where the fastest grains of the 10–12-mesh class (0.07246 in average diameter) fell at the rate of 17.601 inches per second, while the slowest fell but 9.163 inches per second, or less than the slowest grains of magnetite, of which the specific gravity was but 4.987. The fallacy of basing screen-sizes on the mean ratio of equal-settling grains is obvious. Such a determination of screen-sizes is clearly useless, moreover, when it is considered that only one element of jigging, viz., the free-falling of the grains, as the results of the pulsion-movement, is taken into account, while the effects of interstitial retardation and suction are ignored.

It may well be doubted whether the very important factor which Prof. Richards has deduced as the suction-factor, that which makes

a dividing-line between two methods of procedure (*i.e.*, the jigging of mixed or sized products), has been correctly determined. Apparently it has been derived chiefly from a consideration of the estimated percentages of heavy mineral in the tailings from the test-jig, which showed a marked increase in the coarser sizes as the specific gravities of the heavy mineral decreased. It is not easy to perceive from Tables XLVI., XLVII. and XLVIII. that the marked retrogression stops, as Prof. Richards assumes, with arsenopyrite, from the interstitial factor of which he takes the suction-factor, 3.7. Certainly there is as great a difference between the tailings of arsenopyrite and magnetite as between antimony and arsenopyrite. This factor should be confirmed by jigging-tests on a larger scale and by more exact determination of the percentages of heavy mineral in the tailings.

Prof. Richards (communication to the Secretary): Mr. Ingalls's criticism of my method of using nearly the slowest grains, instead of the very last grain, is natural, and I have no doubt it is just for the finer sizes. For the coarse sizes, however, where only about 50 to 100 grains were used, there would be but little difference in the judgment of different observers. The greatest cause of the apparent discrepancies lies in the varying tendencies of the different minerals to split into flat scales. Copper and antimony both had decidedly more of flattened grains than their neighbors, while magnetite had less.

With regard to the factor 3.7, I think it will vary somewhat with different fractures. Let us suppose, for example, that we are trying to separate a lot of galena, with a tendency to break into flattened scales, from quartz, which tends to break into cubes or rough spheres. The interstices would bear a different ratio to the size of the grains than would be the case if the quartz was flat and the galena cubical.

I consider my best determination of this factor 3.7 to be, not in Tables XLVI., XLVII., and XLVIII., but in the jigging-tests recorded on pages 469, 470, 472, 473 and 474. Here there was no estimating by the eye, except with regard to the end-point, in which one observer might have differed a little from another. The figures of Table A, taken from pages 469 and 470, speak for themselves.

Here the galena, as it diminishes in size, becomes harder and harder to jig with much suction, until test 10 is reached, when the time drops to less than one-half its predecessor. Here, for the first time, the galena particles move freely in the interstices of the quartz. In tests 13 and 16 they move still more freely.

TABLE A.—*Jig-Tests of Quartz and Galena.*

Test.	1	4	7	10	13	16
Quartz, diameter	.0683	.0683	.0683	.0683	.0683	.0683
Galena, diameter	.0683	.0429	.0262	.0195	.0095	.0042
Pulsations for nearly complete separation	257	302	748	337	190	86

The turning-point with sphalerite, as shown by the following table, taken from pages 472, 473, and 474, is far more marked than with galena.

TABLE B.—*Jig-Tests of Quartz and Sphalerite.*

Test.	19	22	25	28	31	34
Quartz, diameter	.0683	.0683	.0683	.0683	.0683	.0683
Sphalerite, diameter	.0683	.0429	.0262	.0195	.0095	.0042
Pulsations for nearly complete separation	2129	1676	1759	297	208	288

TABLE C.—*Velocities of Fall (from pp. 414 and 415).*

Test.	19	22	25	28	31	34
Quartz, fastest	8.746	8.746	8.746	8.746	8.746	8.746
Quartz, slowest	4.993	4.993	4.993	4.993	4.993	4.993
Sphalerite, fastest	10.472	8.515	6.261	5.224	2.783	1.437
Sphalerite, slowest	7.245	5.327	3.064	2.259	.800	.291

Here the jump from test 25 to test 28 is most surprising. These grains of sphalerite could not be separated according to the law of equal-settling particles (see Table C.), nor could they be separated by pulsion alone (see test 30); but with strong suction they are separated with a speed that would be called rapid jigging. The factor 3.7 is the quotient of

$$\frac{.0725}{.0195} = 3.718.$$

Throughout the jigging-tests, the figure .0683 in all the tables should read .0725 (see page 413). That is to say, the calculated average diameter for 10- to 12-mesh should be based on the old 10-

mesh screens, not the new ones. This error was not detected until now.

Mr. Ingalls is quite right in saying that the factor 3.7 needs further verification. Here is an instance. Of my two 10-mesh sieves my old one (see p. 413) had 10 meshes one way and $8\frac{5}{8}$ the other; now, what is the diameter of that hole? Clearly, it is larger than $\frac{1}{10}$ inch and smaller than $\frac{1}{8\frac{5}{8}}$. I have assumed it to be the mean of the two = .0830 inch in diameter. Again, what is the diameter of a particle which passes through this 10-mesh sieve and rests on a 12-mesh? Clearly, it is smaller than the first and larger than the second. I have assumed that it is the mean of the two = .0725 inch.

My claim is this: After all these difficulties of definition have been cleared away, there is an equilibrium factor obtainable by the pointed-tube test for every pair of minerals, by which we can decide whether, if slimes are removed and included grains are absent, the minerals can be completely separated by jigging mixed sizes with much suction. They can or they cannot be, according as this factor is above or below the interstitial factor of the lighter mineral. This interstitial factor is the diameter of the grains of any size divided by the diameter of the interstices between grains of that size when they are packed tight upon a jig by suction. This interstitial factor for my quartz is approximately 3.7. It is also clear from Table XLIV. that the nearer the equilibrium-factor of the two minerals approaches the interstitial factor of the quartz, the more can suction help pulsion in jigging mixed sizes.

A personal letter from another source contains the following:

"The results Prof. Richards has obtained show conclusively how the German writers have neglected or given very subordinate position to forces which are such positive factors in successful concentration.

"The great drawback to experimenting on a small scale lies in the difficulty of approximating to the conditions occurring in actual work. If we could so prepare our ores that the crushed material consisted of pieces of gangue and mineral free from each other, we would have comparatively little difficulty in the separation; but where almost every second piece of gangue has adhering to it a particle of mineral, the question of specific gravity becomes a very mixed one. For this reason it has always seemed best to me to insist, under all circumstances, on close sizing from the start, followed by re-crushing and re-sizing between each two sets of jigs. In this way I find we always get good average products, not quite so high-grade, but giving a final very low tailing-value. While the re-crushing may not be so essential on our Lake Superior ores, where the gangue is reasonably constant and the mineral is confined to one metal, it is absolutely essential with our western ores, where these conditions do not prevail; and it was not until this was understood that successful work was done in the west."

It is true that the large mills bring in many conditions that are difficult or impossible to reproduce in a laboratory experiment; but let us look into the case under discussion and see if the remark holds here. I have taken the ground that jigging consists of two distinct actions, pulsion and suction. In my experiments I have isolated pulsion and have proved what it is capable of doing on minerals of a great range of specific gravities. I did the same for suction and pulsion taken together. The excess obtained by the sum of the two over pulsion is clearly the work of suction. This set of tests shows that when the interstitial factor between the two minerals to be separated is greater than 3.7, it will probably be safe to abandon close sizing before jigging and to size according to convenience only (p. 485). But if this factor is below 3.7, the loss from jigging without preliminary sizing will increase as the factor falls, sooner or later condemning the jigging of mixed sizes. I think the critic has placed a different meaning upon one portion of my paper from that which I meant to convey. In reading the list of minerals experimented on (pp. 411 and 431), he has dwelt upon the name of the mineral and has judged that because I found that galena gave certain results in my tests, I would recommend that all ores of galena should be treated accordingly. I intended the series of minerals to be looked at only through their specific gravity; and I should argue in this way: that if in a galena-quartz separation there was a serious quantity of included grains which had a specific gravity of 4.0, then, whether we consider the separation of quartz (sp. gr. 2.6) from included grains (sp. gr. 4) or the latter from galena (sp. gr. 7.5), the interstitial factor is in either case very low (about 2), and the ore should be treated as if it were sphalerite of specific gravity 4, condemning the jigging of mixed sizes and requiring close sizing preparatory to jigging.

LEAD- AND ZINC-DEPOSITS OF MISSOURI.

Discussion of the Paper of Mr. Winslow. (See p. 634.)

(Bridgeport Meeting, October, 1894.)

F. L. Clerc, Chicago, Ill. (communication to the Secretary): On page 681 in Mr. Winslow's paper, he refers to me as having "advocated the view that the ores were derived from the patches of Coal-

Measure shales which are so abundant in southwestern Missouri," and adds:

"As instrumental in effecting this result, he requires a Quaternary submergence, during which the ores diffused through these shales were leached out and deposited in the underlying breccias."

Mr. Winslow must have fallen into some strange error (possibly a confusion of authorities) in making the above statement. So far from ever having held or suggested the hypothesis of such a Quaternary submergence, I have never thought or heard of it until I saw his paper, and it appears to me, from what I have seen of the region, almost grotesquely absurd; so that I naturally object to having my name associated with it. In the only paper I have written on this subject (a short popular article, prefacing the first attempt to publish complete statistics of the mineral output of southwest Missouri) I certainly made it plain that I held radically different views, and I am equally sure that the notion of a Quaternary submergence has never been advanced in any conversation between Mr. Winslow and myself.

Mr. Winslow (communication to the Secretary): My remarks concerning Mr. Clerc's views were based upon a pamphlet entitled "Lead- and Zinc-Ore, Southwest Mo. Mines. Authenticated Statistics, etc., for the Year Ending March 1, 1887. By John N. Wilson, Carthage, Mo. With Contributions by F. L. Clerc and Thomas W. Davey." Pages 4 to 11 are over Mr. Clerc's signature. On page 8 he says:

"Patches of these slates and pockets of coal are found throughout the mining regions, but in no case, so far as I have seen, under circumstances which sugges that they have been deposited in the Coal age as outliers of the regular coa beds."

Referring to the material of these slate-pockets, he says, near bottom of page 9:

"These materials must have been brought to where they are in the form of a very fine mud or silt, as though they were dropped from a slow-moving river *or a broad lake*."*

On page 10, referring to the black slate, he says:

"If the date of this" (*i.e.*, the black slate) "be fixed as the close of the Ice age and the melting of the glaciers, which has plowed deep north and south grooves

* Italics are mine.—A. W.

through the coal-fields of the north, it brings the origin of the present ore-deposits down well into the Quaternary age. If, now, lead and zinc were contained in even the minutest traces in the black slate, it is easy to see how lateral secretion would effect their concentration."

In these statements, and in the distribution of the slate-pockets of southwestern Missouri, the hypothesis of a Quaternary submergence seemed to me to be involved—whether partial or complete, is another question. Mr. Clerc expresses the opinion that the deposits never covered the highest lands. From my observations, a submergence which would include all of the coal-pockets of Jasper county would leave very little, if any, of the surface above water-level.

I have perhaps misinterpreted Mr. Clerc in attributing the leaching out of these slates or shales to the waters of the disputed flood, rather than to subsequent rain-waters. The hypothesis once accepted, it seems probable that both agencies would have been active.

I am glad to accept Mr. Clerc's correction, and trust that he will acquit me of any intention to misquote his views. In any case, the essential point is that, on his hypothesis, the ores were derived from the shales, and Mr. Clerc seems to take no exception to my statements in this regard.

THE DIKES AND REEFS OF BENDIGO.

Continued discussion of the paper of T. A. Rickard on "The Origin of the Gold-Bearing Quartz of the Bendigo Reefs, Australia." (*Trans.*, xxii., 289, 738.)

(Bridgeport Meeting, October, 1894.)

PHILIP ARGALL, Denver, Colo. (communication to the Secretary): Mr. Rickard expresses regret that I have not given more extracts "from the fresh leaves of nature's open book." The quotations used by me in argument were all from nature's open book, and by authorities recognized in the scientific world as being able to decipher nature's unwritten pages, if not lay bare her inmost secrets. The study of nature should be preceded by a knowledge of her laws. Natural science is but the summary of the observations of the past or an organized and accurate knowledge of the order in which things have been observed to happen. One who ignores the knowledge of the past or despises that of the present and approaches nature's open book with a virgin mind but too often finds her

leaves "too fresh." Had Mr. Rickard's theoretical assumptions been new, or even modern, my researches in the "musty shelves of the library" or in the misty past might, indeed, have been unpardonable, but when we have the mobility of lava attributed to water or water-vapor, the hydrothermal fusion of granite made use of to explain a certain well-known phenomenon, the deposition of gold in the oceanic sediment of an Australian sea claimed as the source of the gold in the Australian veins of to-day, can I be censured for pointing out that this is needless threshing of old straw, that Scrope, for instance, attributed the mobility of lava to water-vapor sixty-nine years ago; that Scheerer introduced the hydrothermal fusion of granite in 1847; that Daintree long since applied the oceanic-gold theory to the Australian reefs in form so complete that none of his imitators have improved it during the twenty-seven years that have elapsed.

I admit the accuracy of Mr. Rickard's observation regarding my criticism of his vesicular granite hypothesis so far as to say it is obstructive. The obstruction remains; for he neither explains how the vesicles in the granite could be repaired, or why they have not been repaired in the granite generally, nor will he even deign to enlighten us regarding the "extrusion" of the granite in thin sheets and seams into and between the slate and sandstone beds. I had always believed such granite to be an "intrusive" rock; and now that Mr. Rickard quotes himself to the contrary, I can only hope that my criticism will be, as I intended it to be, instructive.

It appeared to me that Mr. Rickard did make a comparison between volcanic lavas and granite in his paper (*Trans.*, xxii., 295), and that his discussion regarding the apparent absence of vesicular structure in the latter rock did both denote and connote the vesicular structure of the former. However, as he is the best judge of what he really meant to say and what he intended to compare, I gladly accept his explanation.

As regards the Bendigo dike rock further discussion is unnecessary, seeing that the careful microscopic examination of Mr. Howitt and the chemical analysis made by Mr. Stone show the rock to be Limburgite or, at any rate, an ultra-basic rock with glassy groundmass, indicating not only igneous fusion but also rapid cooling.

The analysis* of this dike is, however, of sufficient interest to quote in full, so as to complete the record:

* A. W. Howitt, F.G.S. (*Department of Mines, Special Reports*), "Notes on Samples of Rocks Collected in the 180 Mine at Bendigo," page 4.

Analysis of Bendigo Dike.

SiO_2,	39.32
Al_2O_3,	17.53
Fe_2O_3,	3.07
FeO,	9.12
CaO,	10.38
MgO,	8.00
K_2O,	2.04
Na_2O,	2.44
H_2O,	5.10
Moisture,	2.20
	99.20

Mr. Rickard (in paragraphs 2 and 4, *Trans.*, xxii., 765) thinks I am contradictory of myself. I am obliged, therefore, to point out that even a Bendigo fissure must have two projections, strike and dip; that the former may be a continuous "clean-cut" fissure on a given horizon or in a given bed, while the latter may be a continuous but irregular zigzag. It was the former (continuity of strike) that I referred to in paragraph 2, and the latter (irregularity of dip, and hence not a clean-cut fracture) which I discussed in paragraph 4, following Mr. Rickard's own argument, the whole tenor of which was the "tortuous and very irregular passage" of the dikes "through the over-lying rocks," that "they do not fill a clean-cut continuous fissure," that "the lava was seeking its way upward," etc.

I have carefully avoided discussing the structural geology to which Mr. Rickard has treated us. I shall not do so now. It appears to me, however, that it is from misapprehension of the dynamic geology that Mr. Rickard's difficulties as to the fissuring arise. While he observed of the lava dikes that "where their line of passage takes them across the quartz reefs the crossing is generally effected near the apex of the saddle," he lost sight of the central fact that the dikes occur along the course of most, if not every one, of the anticlinal axes, and have not been noticed elsewhere. It is manifest that the dike-fissures followed the lines of least resistance, breaking through the anticlinals, some of which had reached the point of rupture, in preference to the synclinals, which offered the maximum resistance to fissuring. Turning to *Trans.*, xx., 471, we find the idealization of Mr. Rickard's views. Here he shows two out of three dikes in synclinals, while the third passes through the legs of successive saddles. The occurrence of dikes in such synclinals is contrary to the facts of the case, at variance with geological observations and shows entire misapprehension of the dy-

namic movements that culminated in the fissuring and lava injection.

Mr. Rickard would fain have us return to the strict uniformitarian school which "tabooed" everything catastrophic. The geology of to-day (evolutionism) accepts that which is best, alike from the catastrophic and uniformitarian schools, rejecting, however, their limitations. As Prestwich well puts it, "the forms of erosion, the modes of sedimentation and the methods of motion are the same in kind as they have ever been, but we can never admit that they have always been the same in degree." A short quotation from a recent address of Sir J. W. Dawson is particularly applicable to the Bendigo phenomenon, where the slow and long-continued tangential pressure plicated and folded the rocks, gradually bringing them to the sharp anticlinal folds and catastrophic fracturing: "In short, slow and gradual action inevitably produces catastrophic or critical periods, and these again prepare the way for the recurrence of times of dull uniformity and scarcely perceptible motion."

The reply to my question: "How can the hydrothermal fusion theory account for the intense action of these granite-veins on the country-rock on the one hand, while on the other it is brought forward to prove the feeble action of the dikes?" is insufficient. We are told that "in the former case there was a store of heat close at hand (the main granite mass); in the latter, the main body of lava, of which the dikes were minute branches, was at a depth so great as not to be able to cause any metamorphism of those rocks into which the mine workings have penetrated." Here we have an admission that the small granite veins that penetrated the sedimentary rocks for considerable distances from the granite-contact, and so altered these rocks that it is difficult to distinguish the separate beds, were enabled to cause this local metamorphism by reason of the conduction of heat from the main granite mass, while, as regards the dikes, conduction is not admitted. Nevertheless, we are urged to believe the assertion that the boiling mud or vapor-saturated lava "found its way to the surface slowly and gradually through the manifold fractures produced by the strain to which the rocks were being subjected, not for a few seconds, but for a long period of time." Now it is manifest that without a continuous supply of heat the lava must have consolidated on its way upwards through these minute branches during the "long period of time;" while, with a supply of heat, the enclosing rock must, in the presence of so much water, have been highly metamorphosed, as I have previously pointed out.

Mr. Rickard tells us that he has relied upon what he has seen in the Bendigo mines, and upon the conviction thereby induced "that surface and deep underground action are very dissimilar in kind and in character." One looks in vain, however, for any proof of this assertion in his paper. In speaking of the dike lava, he informs us that "it is identical in lithological character with the basalt, successive sheets of which . . . . overlie the Miocene and early Pliocene gravel of the 'deep leads,'" etc. If the rocks poured out on the surface in thick sheets and those which consolidated as thin dikes under many thousand feet of rock-pressure are lithologically the same, as he claims they are, surface and underground action at Bendigo would, from his observations, appear to be similar (not dissimilar) "in kind and in character." A cursory examination of my discussion will show clearly that I have not limited my remarks "to the surface and open-air phenomena of volcanic activity." I have made extended underground studies of such matters in the British Isles, in Australia, in this country, and elsewhere. The information so gathered, together with Mr. Rickard's description, leads me to the conclusion that the Bendigo dikes are injections of basic rock, few, if any, of which reached the surface—a molten basic rock quickly injected into the fissures and rapidly cooled. It is obvious that the speed at which a molten rock may be injected into a fissure deep down in the earth, or the velocity with which it issues from a fissure at the surface, do not directly affect its lithological character. It is the conditions of consolidation that are the determining cause.

I also hold that surface and underground are terms of little geological significance. The surface of to-day is often the deep underground of the past. It is not necessary to go to Bendigo to get a peep into the earth's crust, or to study the action of intrusive rocks under a few thousand feet of head. At Leadville we can see to-day, on the surface, the effect of igneous intrusions which took place 10,000 feet below the surface of former days. Nor is it necessary to go to the Bendigo mines to study dike-phenomena when we can see and study the matter to much greater effect in the cañons and other rock-exposures of the Rockies and other mountain ranges, and also in the basal wrecks of volcanoes visible at the earth's surface to-day, though once 10,000 to 20,000 feet below. It is from the study of the dikes, necks, and intrusive sheets visible in such places that we have received our present knowledge of the physical characteristics of rocks consolidating under varying conditions of temperature and pressure. It is needless to say that vague and indefinite

assertions, in the absence of any new facts based on microscopic studies or chemical analysis of the rocks, cannot be accepted as evidence against the accurately determined and properly correlated facts laid down in the books.

Mr. Rickard's assertion that "at Kilauea to-day the basalt owes much of its mobility to the steam which it contains," is in direct contradiction of the statement of Prof. J. D. Dana, probably the greatest living authority on this subject, who says:*

"The phenomena of Kilauea are largely due to the fact that it is a basalt volcano in its normal state. By this I mean, first, that the rock material is dolerite or basalt, and, secondly, that the heat is sufficient for the perfect mobility of the lavas, and, therefore, for the fullest and freest action of such a volcano."

I cannot agree with Mr. Rickard that the explanation of Dunn, Ulrich, and others, regarding the filling of the Bendigo dikes, is in any way "based upon the old theory that the earth has a solid crust lying upon liquid matter," etc. This theory is painfully old, like some others advanced in this discussion. I certainly hold no such ideas, nor are these ideas necessary to the proper conception of the Bendigo phenomena. That the earth is as rigid in its resistance to tidal deformation as a sphere of steel, is unquestionably correct. On the other hand, we know it is sufficiently plastic to allow coast-lines and mountain ranges to rise and sink on the great land areas of the globe. The earth, then, while capable of resisting tidal stress, which might be called a momentary force, appears to be equally incapable of sustaining accumulative or slowly applied stresses. This is well illustrated at Bendigo, and very clearly shown in diagrammatic form in Dunn's monograph, where a block of rock 5280 feet in length by 2000 feet thick, as laid down in its original horizontal position, is now represented by a block of folded rock 2250 feet long and 4700 feet thick. The enormous tangential pressure that produced this folding and land elevation was not only available for the injection of molten rock into suddenly formed fissures, but also for the continuation of the stress after the consolidation of the lava in the dikes, as evidenced by their very extensive faulting. This latter phenomenon might be argued as proof against the long period of time required to fill the dikes on the mud-lava hypothesis, but I refrain. To those who do not believe in hydrostatic pressure in volcanic phenomena, or in the almost

* *American Journal of Science*, 3d series, vol. xxxv., p. 28, 1888.

inconceivable power of subterranean energy, the published researches of Emmons on Leadville and Gilbert on the Henry mountains should furnish information as instructive as it is interesting.

We are now informed that Mr. Rickard indicated in his paper (*Trans.*, xxii., 317) that the Bendigo gold was derived more particularly from that portion of the formation "which is beneath the area now being mined." This may be the present opinion of the author; but it cannot be said to be the opinion expressed on pages 306 and 317 of his paper, where generalizations will be found suggesting that the gold came not from below but from its wide and even dissemination in the mass of the surrounding formation, where bedding favored the circulation of mineralized waters and the anticlinal axes furnished arched canals to facilitate the lateral flow, all of which indicates lateral secretion in its modern acceptance, while there is nothing to indicate, much less support, an ascension-theory.

Before entering on a summary of the origin of the Bendigo gold, I ventured to give a brief review of the literature of the subject. In doing so, I had to take the probable and the improbable hypotheses. I dismissed the sublimation-theory of Rosales and of Belt in two lines, while my critic occupies double the space to prove, as it were, a negative. I did not base any argument on the sublimation theory. It appears to me, however, that the iodine-theory resurrected by Mr. Rickard and the sublimation-theory of Rosales have points in common; they are both alike highly improbable, at variance with observed facts and contrary to the laws of chemistry.

Mr. Rickard's statement that I have compiled at second-hand from Lock and not from personal examination of the Australian authorities is entirely beside the point at issue. Apart from this, however, the assertion carries with it the evidence of its own inaccuracy. Almost one-half of the Australian authorities quoted by me are not to be found in Lock's* work; and it is a remarkable coincidence that these are the authorities with which Mr. Rickard appeared to be unfamiliar.

Mr. Rickard (communication to the Secretary): I think I have

* The footnote (*Trans.*, xxii., 754) on which Mr. Rickard bases his sweeping assertion, referred to a written statement prepared by Mr. H. A. Thompson for publication in the work of R. Brough Smyth, page 246. The reference was, therefore, to the original publication and not "quoting at second-hand from Lock." In fact, the statement does not occur in Lock's work. In my brief review of the literature of the origin of gold I did not attempt page-references, as in most cases I gave a *résumé* of the author's views.

stated my views and suggestions with sufficient clearness in my original paper and its discussion; and I am content to leave that statement, as it stands, to the intelligence of competent and candid readers. Neither needing nor desiring to pursue a personal controversy concerning my own consistency, accuracy or fairness, I concede to Mr. Argall the last word in that line, and confine myself to two points in his contribution which seem to deserve attention.

The first is the question whether, in asserting that the basalt of Kilauea "owes much of its mobility to the steam which it contains," I was contradicting the conclusions of Prof. J. D. Dana. That this is not the case, I think can be clearly shown. The passage quoted by Mr. Argall from the *American Journal of Science* does not deny the agency of steam as a factor, but simply says that the heat is sufficient for the perfect mobility of the material. Prof. Dana's book, published in 1890, and quoted in several places in my paper, sustains my view, and on page 708 of his *Manual of Geology* (second edition) he says, at the end of a passage descriptive of Kilauea: "The mobility is also very largely promoted by the vapors rising in the lava, especially the overheated steam."

With regard to the statement that the Bendigo dikes occur along the course of most, if not every one, of the anticlinal axes, and, incidentally, that the ideal section, engraved with a former paper of mine (*Trans.*, xx., 471), misrepresents their position, I ask the reader, bearing in mind that the drawing referred to was offered only as an ideal section, intended, not to illustrate any particular point, but simply to give a rough approximation of the writer's idea of the general geological structure of the district, to judge for himself whether two out of the three dikes therein indicated do or do not penetrate synclinal country. What my conclusions were upon this particular point, is clearly indicated in the numerous drawings* to be found in my contributions upon this gold-field.

I may add, in conclusion, that I do not see how the question of the location of the dike-fissures bears upon that of the condition of the material which filled them.

R. W. Raymond, New York City: One or two of Mr. Argall's points, which Mr. Rickard has not cared to notice, seem to me worthy of comment.

The proposition that, because granite is "intrusive," it cannot be considered as extruded from one horizon as well as intruded into

* *Trans.*, xx., 473, 485, 487, 489, 513, 515, 517, 527; xxi., 699, 701; and xxii., 297.

another, is rather too fine for general comprehension. Geikie says:

"Granite invades and alters rocks of all ages, up to late Mesozoic and Tertiary formations. Hence, it does not belong exclusively to the earliest, nor to any one, geological period, but has rather been extruded at various epochs, and may even be in course of extravasation now, wherever the conditions required for its production exist."*

Prestwich says:

"In South America we find the granite . . . . extrusive through rocks of more recent date, and forming axial masses of great dimensions, but of yet unsettled age, in the range of the Andes."†

Dana says that the term *intrusive* signifies that rocks "have been ejected from below, through fissures intersecting other rocks."‡ In view of these quotations, which might be multiplied if necessary, it seems to me that Mr. Rickard is in respectable company when he speaks of granite as having been "extruded."

Mr. Argall's allusion to the uniformitarian school of geologists brings up a matter of considerable historical interest. The work of Hutton, who is regarded as the founder of that school, was published at about the close of the last century, and Playfair's redaction of it appeared after the beginning of the present century, while the great treatise of Lyell, which really constitutes the authoritative utterance of the school, appeared some thirty years later. It is not easy to show from these authorities that the so-called "Uniformitarians" ever insisted upon the proposition that the processes of nature have always been the same in intensity or rate as well as in kind. Hutton's principle was, that "in examining things present, we have data from which to reason with regard to what has been;" and Lyell proceeds upon that basis to deny the necessity of imagining other causes than those now in action to account for the past. I do not think either of them ever dreamed of declining to admit such a "catastrophe" as the formation of an eruptive dike, or of refusing to accept evidence that the intensity of natural processes was greater in some periods and in some places than in others.

I am not acquainted with any school of geologists which deserves the peculiar name "evolutionary." Cotta, in his *Geologie der Gegenwart*, was perhaps the first to employ that term; but even his

* *Textbook of Geology*, 3d ed., 1893, p. 565.

† *Geology, Chemical, Physical, and Stratigraphical*, vol. i., p. 428.

‡ *Manual of Geology*, 2d ed., p. 63.

brilliant genius could not impress upon geological science any form corresponding with the laws and processes of evolution, as revealed in the vegetable and animal kingdoms. The analogy is a pure rhetorical fancy.

Prof. Prestwich, in the preface to his *Geology* (1886), professes to deem it necessary to furnish a work representing the views of a school "which, until of late, has hardly had its exponent in English text-books," and declares that school to be the one which considers "that the physical forces were more active and energetic in geological periods than at present;" while, according to his statement, the opposite school "adopts uniformity of action in all time." But he admits that Lyell did not go so far as this; and, curiously enough, I am unable to find in his two octavo volumes a single citation presenting the proposition which they were professedly published to disprove. It is natural that the author of a new text-book should seek to justify its appearance by claiming for it a new standpoint; but a really new school is not so easily founded. Prestwich's treatise does not, so far as I can discover, make any use of his new principle, except with reference to certain calculations based on the observed rate of denudation and sedimentation at the present time. This rate, he claims, was greater in former times—a proposition which I do not suppose any school would pronounce impossible, though the evidence supporting it might affect different reasoners differently. But whatever conclusion might be reached would be entirely within the limits of the principles and methods of Lyell, and could not be regarded as the foundation of a new school. Still less could it be called "evolutionary." It is quite the fashion to decry "uniformitarianism," and the name itself unfortunately makes such superficial disparagement plausible. But I think we are all still in the school of Hutton and Lyell, and likely to stay there, however much we may desire to dignify with the rank of new departures our steps of progress on the lines they marked out.

THE GENESIS OF ORE-DEPOSITS.

Discussion, at the Virginia Beach Meeting, February, 1894, of the Paper of Prof. Posepny. (*Trans.*, xxiii., 197, 587.) Including communications subsequently received.

T. A. Rickard, Denver, Colorado (communication to the Secretary): The paper of Professor Posepny was printed so short a time

before the Chicago meeting that it could not receive at that meeting the thorough discussion, based upon careful study, which its great importance and value deserved. In the remarks which I made on that occasion, I could do little more than express, with others, our thanks to the distinguished author for this admirable treatise on a subject of such general and permanent interest. Further examination of it has confirmed the opinion that its appearance marks an epoch, particularly in this country, in the study of ore-deposits, and their origin, and has led me to feel that our appreciation of it will be best expressed in aiding its purpose and widening its usefulness by the free contribution of facts and interchange of views which it invites.

I have, elsewhere,* expressed some dissatisfaction with the new names introduced in this paper; and it has seemed to me, also, that the classification of ore-deposits, which it proposes, is unnecessarily complicated. From the stand-point of a mining engineer, we have had, in my judgment, no classification more practical and sensible than that suggested by Dr. Raymond, twenty-five years ago (outlined in Professor Posepny's paper†). If any modification of it be permissible, I would suggest the following:

I. Surface-Deposits.
  A. Due to mechanical agencies.
  B. Due to chemical agencies.
II. Inclosed Deposits.
  A. Bedded.
    a. Contemporaneous, in origin, with country-rock.
    b. Subsequent, in origin, to country-rock.
  B. Not bedded.
    a. Due to dislocation.
    b. Due to impregnation.

Surface-deposits have no regular form, and are, therefore, distinguished primarily by their origin. Class A would be typified by gold-bearing placers, and Class B by deposits of bog iron-ores.

When we come to inclosed deposits, we find an extreme complexity; but we readily recognize that some are conformable to the bedding of the country-rock, while others are independent of it. We can further distinguish those which are of contemporaneous origin, such as the coal-beds, from those which were formed after the de-

* *Eng. and Min. Jour.*

† *Trans.*, xxiii, 204.

position of the country-rock. To this class belong ore-deposits which have replaced beds of limestone; and another pretty example is afforded by the Bendigo saddle-reefs, which are conformable to the anticlinal curves of the country-rock, but were clearly formed after both the original sedimentation and the subsequent folding.

Among the non-bedded deposits there is no limit to diversity of structure and of origin. We recognize, however, that the fissure-veins which cut across the bedding, but retain a definite position due to their formation along lines of original dislocation, may be distinguished from the irregular impregnations, due as much to the chemical composition of the country-rock as to its structure. These two types, however, are forever intermingled. It is seldom, indeed, that an ore-deposit has not some features, however faint, of form and structure dependent upon those of the country-rock, while, on the other hand, it is not often that a fissure-vein is found which does not exhibit, in places, a lack of definition, due to metamorphic action upon its inclosing walls.

In the discussion of the origin of fissures, Prof. Posepny has touched upon a point which has been the subject of frequent debate. I fully believe that dislocation accompanies the formation of a fissure, and that a movement of its walls is often evidenced by slickensides and striæ. Yet, this has been questioned by one or two members of the Institute who are known to be both accurate and experienced observers. The question at issue is a vital one, if we desire to obtain a clear idea of the mode of formation of mineral veins. It has been denied that the striæ and slickensides observed upon the walls of lodes necessarily prove that movement has taken place; but it has never been clearly shown what other agency did form them. Prof. John A. Church has discussed this matter in a most interesting way,* and has pointed out that slickensides may be formed, not only by rubbing but also by "deformation, as when a plastic substance like clay is forced through an opening," and again by deposition in fine parallel lines. Recently, Prof. Daubrée has experimentally proved that gases under high pressure are capable of producing striæ upon rock-surfaces.† It is true that a distinction is made between striæ and slickensides, but I look upon the two as the work of the same agency. In the former case we have coarse rub-

* *Eng. and Min. Jour.*, April 30, June 11 and 18, 1892.

† *Bull. Soc. Géol. de France*, 3 série, Feb., 1891, t. xix., p. 313. *Compt. rend Acad.*, t. cxi., pp. 767, 857, séances du 24 Nov. et du 8 Dec., 1890. *Compt. rend. Acad.*, t. cxii., p. 125, séance du 19 Jan., 1891.

bing due to large particles, and in the latter, fine polishing due to minute particles. There is no doubt, however, that certain structures are called striæ, which are to be ascribed to causes other than those usually supposed to produce striæ and slickensides. As I write I have before me a large piece of rock, the surface of which exhibits fine parallel lines, which, at the mine (the Hillside, in Yavapai county, Arizona), were called striæ. The rock was part of

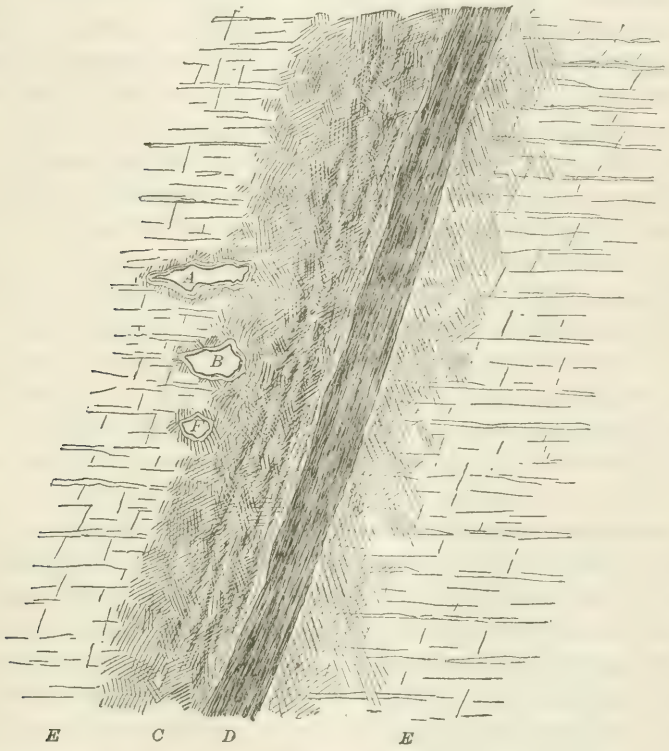


Fig. 1
HILLSIDE MINE, ARIZONA

the casing of a cavity found in the hanging-wall of the lode, which traversed a quartzose talc-schist. Its surface has been covered* by a series of siliceous coatings, doubtless deposited by the mineral-bearing waters which circulated over it. The precipitation took place along certain parallel lines, probably marking the direction of flow of the circulating waters, and the resulting appearance is to be regarded as a pretty example of a variety of crustification, but, com-

* As shown by viewing the broken edges of specimens.

ing as it does from a lenticular hole, cannot have been due to rubbing caused by faulting.

In the accompanying drawing (Fig. 1), reproduced from a sketch made underground, the cavity above referred to is marked A. There are two others, B and F, of the same kind. D is a seam 6 inches thick, of white talcose gouge, lining the footwall, and separating it from C, which is the lode itself. The latter is 15 to 18 inches wide, and consists of quartz, iron pyrites, zincblende, and a little galena, very much intermingled, and carrying gold and silver in almost equal proportions. The lode itself reproduces to a noticeable extent the structure of the country-rock which it has replaced. The cavities in the hanging-wall are also surrounded by talc-schist, which is mineralized to such a degree as to constitute "low-grade ore." The vein cuts clear through the foliation, nearly horizontal, of the talc-schist and the alteration of the country-rock, while most marked in C, extends to a varying distance on either side.

Not infrequently the quartz of a lode has striated markings which are but the negative of those occurring on the wall-rock.* In such cases the quartz is sometimes entirely solid and unbroken, suggesting that it was deposited upon the previously striated surface, and that it has not only replaced the substance but also reproduced the structure of the rock once inclosed by the fissure walls. On the other hand, one instance may be cited where it seems necessary to suppose that movement took place subsequently to the deposition of the quartz. At the 1800-foot level in the Great Extended Hustler's mine† at Bendigo, Australia, the quartz lying against the hanging-wall of the reef exhibited a surface as smooth as polished ivory, but distinctly grooved, and also marked with fine, dark lines, parallel to the grooves. The latter had, I believe, an origin similar to that of ordinary striæ, while the dark lines were due to the grinding of particles of pyrite observable in the quartz. Though this quartz seemed to the eye as hard as adamant, it would readily crumble away when pressed between the fingers. It had been crushed to the consistency of common table-salt, which, save for the presence of occasional crystals of pyrite, and for its highly polished surface, it much resembled.

Objection has been raised to accepting the occurrence of clay, striæ and slickensides as necessary evidence of faulting, because they are occasionally absent where movement may be supposed to have taken place. In such instances, it is reasonable to infer that they

* Instances of such are to be seen in the gold-quartz veins of California.

† See *Trans.*, vol. xx., 512 *et seq.*

have been destroyed by agencies identical with those to which the lode-formation is due, namely, the replacement of country-rock, often in a crushed and shattered condition, by ore, through the metamorphic action of percolating solutions.

There is a fanciful notion current among miners that a smooth wall and a thick gouge are the necessary adjuncts of a productive "true fissure-vein." Experience does not confirm this belief. A defined wall and a soft seam of clay are naturally welcome to the miner, because they facilitate the actual breaking down of the vein-stuff; but they are no more characteristic of productive than of barren lodes.

The irregularity in the dip of some veins has been cited as disproving the possibility of their formation along lines of faulting. Occasionally mine-workings show that the dip of a vein is reversed; and the formation of the fracture which it occupies cannot be referred to a continuous line of movement, because that would have involved the shearing-off of the opposing angle. But it is not necessary to suppose, nor do facts suggest, that lodes are generally formed along continuous or single lines of movement. As Prof. Posepny has well shown, it is the study of the circulation of underground waters which affords the key to much that is perplexing in ore-deposition. In such cases as are here referred to, it is rational to suppose that the mineralizing solutions searched out the easiest way which offered itself. They did not necessarily percolate along a single definite straight line of fissuring, but often deviated from it, whenever it afforded a less ready passage than was offered by other fractures which united with it or crossed it. An instance which occurs to me as I write, is furnished by the Seven-Thirty mine at Silver Plume, Colorado. The lode consists of a system of veins carrying rich silver-ore, the most productive of which is that which bears the name of the mine. It rarely has any considerable width; it is often only a thread traversing the coarsely crystalline granitoid gneiss and porphyritic microcline granite of the region. At the third level, about 280 feet from the surface, there is a very marked irregularity in the course of the vein, presenting some interesting features, which the accompanying sketch (Fig. 2) will help to explain.

From the shaft eastward for several hundred feet (A to B) the vein carries ore; but its width is small and irregular. The lode widens rapidly at B, where it also meets with a sudden deviation in its course. At a first glance, this looks very much like a fault, but subsequent examination will correct such a view. The fissure con-

tinues in a straight line from K to L, after the ore has swerved to the south. Instead of maintaining its eastward course, the ore is disposed in two cross-veins, CD and EF, nearly at right angles with that course, which unite with a fissure, MQ, similar in character and parallel in strike to that from which they sprung, AL. Both AL and MQ are continuous so far as they have been followed in the mine-workings. The walls are well-marked, even after they cease to enclose ore. The cross-veins, CD and EF, lack well-defined boundaries. The western branch, CD, is a streak, about 3 inches wide, carrying ore of a tenor of 300 ounces of silver per ton, while the eastern branch, EF, is larger, about 1 foot wide, and carries ore of lower grade, about 100 ounces per ton. The latter is accompanied by much more galena than the former. The distance between the two is 10 feet; their length is 44 feet. The country separating them is not noticeably altered or mineralized.

This is not an instance of faulting; the ore is found in connection with a system of fractures, AB, CD, EF and MQ, the varied struc-

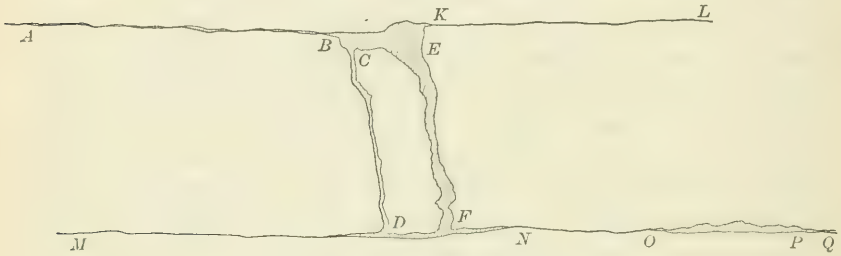


Fig. 2
SEVEN THIRTY MINE, COLORADO.

ture and arrangement of which modified the circulation of mineralizing solutions, and so brought about the irregularity in the deposition of the various minerals comprising the ore. The mineralizing waters met with diverse conditions. From A to B the fissure was tight, and its boundaries were distinct, limiting the circulation to a narrow channel; hence a small streak of ore was found. At B the shattering of the country-rock accompanying the formation of the cross-fractures, CD and EF, offered facilities for the ready penetration of the solutions and for chemical interchanges. From C to D and from E to F the irregular fracture across the foliation of the country-rock produced irregular but rich streaks of ore. On meeting with the other main line of fissure the solutions again found well-defined boundaries which put a check to the metamorphic re-

placement of the country-rock, and it was not till the conditions changed (at O), that a notable width of ore was again deposited.

Many supposed faults found in mine-workings are really of this character. There has been a deviation in the course, and a marked diminution or increase in the amount of ore-deposition, because the mineralizing solutions have circulated along those fractures which presented the easiest passage and offered the conditions most favorable to chemical interchanges.

Returning to the subject of striae, slickensides and clay-seams, I must say, that while the questioning of accepted theories is wholesome, and the views quoted above deserve respectful consideration, it seems to me that observed facts warrant the general belief that these phenomena have usually been produced by the rubbing of two faces of rock which have undergone movement; and I do not sympathize with those who consider that the ordinary explanation is far-fetched. We know that the rock-formations of the upper earth have undergone movement, for this is proved by all geological investigation. Further, we have every reason to believe that movement among beds of rock of unequal flexibility must cause some to break. Facts confirm such a belief. Again, every break must be coincident with a movement; for a fracture can hardly be said to exist until made evident by movement however slight. At any rate a fracture unaccompanied by movement would not give the relief required by a series of beds exposed to such strain as necessitated a rupture. Such movement must be accompanied by friction, due to the tendency to smooth down the irregularities of the two opposing rock-faces. Where movement has once occurred, a line of less resistance is established, and a repetition of movement is likely. The result is to break small particles from off projecting points and so form a dust which water makes into mud or clay, also to scratch the surfaces in contact, forming striæ, and to polish them, forming slickensides. Why therefore deny the probability, even the necessity, of the movement of the walls of a fissure, and why endeavor to give to the markings of rocks underground an origin other than the one which would certainly be ascribed to them if they were found on rocks* at the surface?

The pages which Prof. Posepny devotes to an inquiry into the conditions governing the flow of underground waters are among the most valuable of his treatise. His explanations will do much to

* No one questions, for instance, that the scratching seen on boulders from a glacial moraine are the result of rubbing due to movement.

clarify our conceptions of the mode of behavior of underground waters, and will doubtless suggest further inquiry in the same direction. The word "circulation" is the key to the whole matter. There has been a tendency to speak of descending, lateral and ascending currents, as though the one adjective would cover the manner of movement of all mineral solutions. An ascending flow was supposed to have formed this lode, descending that one, while others again, steering a middle course, have imagined that ore-formations derived their origin from solutions having a lateral flow. In each case a narrow view of the subject is both unphilosophic and unscientific; it has too often been the obstacle to progress in this branch of geology. One great fact confronts us, and that is circulation.

The distinguished author is himself carried away by his prejudices, and in the latter portions of his treatise* allows his ascensionist views to lead him too far and in part to forget the very forcible teaching given in the earlier pages. Much will be done to explain the many puzzling and apparently contradictory features exhibited by the ore-deposits of different regions if we remember that mineral solutions both descend and ascend, that occasionally they may have an approximately lateral flow, and that in each instance their circulation is governed by a diversity of ever-changing conditions.

Water must first descend in order afterwards to ascend. The known density of the earth precludes the supposition that its interior contains any reservoirs of water; the sinking of deep wells and bore-holes has indicated that at a comparatively short distance from daylight the temperature is so high that water could not exist as such, but would be dissociated into its constituent gases; while actual mining exploration has shown that in the deepest mines there is less water encountered in depth than in proximity to the surface. These facts all confirm the every-day observation that underground waters originate from the rain and snow precipitated from the atmosphere.

We may compare the circulation of water up and down, through the earth's rocky exterior, to that of the ordinary heater in a house. The water circulates because, when hot, it rises through the length of pipe, and, when cool, it falls back to be reheated. Using this analogy to explain Nature's operations, we have at one end the condensation and precipitation of moisture due to a fall of temperature,

* Compare *Trans.*, xxiii., 249.

while at the other, and deep down in the earth's rocky confines, we have a heat which sends the water back to the surface. In this matter of ore-deposition we are not concerned with the two ends of the circuit. We have no particular interest for the moment in that part of the water-circulation which intervenes between its elevation by evaporation from the earth's surface and its return as rain; nor, on the other hand, can we see what goes on at the other end of the circuit. We can only guess what conditions obtain and what phenomena occur at depths inaccessible to man. All our investigations must concern themselves with the intermediate stage, that stage which is most particularly marked by the transition from higher to lower temperatures, and, inversely, from increasing to diminishing pressures. It is the nice adjustment of these conditions which, on the one hand, favors precipitation, and, on the other, compels solution. To the miner, therefore, it may appear most important to investigate those factors which bring about precipitation, because to them must be ascribed the immediate agency of ore-deposition. It would simplify his ideas if he could speak of an upper zone of precipitation, where the temperature is low and the pressure light, in contradistinction to a lower region of solution, where the heat is great and the pressure intense. Such attempts to separate the locality of the two processes, however, must not be carried too far. Precipitation has no sooner ceased than solution begins. It is the excess of the one over the other which causes the deposition of ore in one place and its removal to another. Similarly, in our talk of "primary" and "secondary" deposits of ore, while some such distinction may be necessary for the purpose of explaining differences of immediate origin, we must not fail to recognize that all the ore-deposits within the ken of man are essentially secondary. There has been nothing original since the world was first evolved from chaos. We have to deal with a continuous rearrangement of material. The ore of one place came thither by removal from another. Whether it be present in minute microscopic particles or in blocks as big as a house is a distinction more economic and commercial than scientific and philosophic. The decomposition of one mineral is required for the composition of another. Ore-deposits are in their nature concentrations, whether by the mechanical accumulation of disintegrated fragments of older deposits or by the local regathering or segregation by chemical agencies of minerals previously widely and minutely disseminated, or finally by the addition, bit by bit, through mechanical and chemical force, of the matter brought from above or below by circulating waters.

The frequent occurrence of thermal springs in the neighborhood of later eruptive rocks is very properly emphasized by Prof. Posepny, and is of immediate importance to the student of ore-deposition because the eruptive rocks are in turn found so often in close association with lode-formations. That thermal springs, eruptive rocks and ore-deposits are intimately inter-related in their origin is generally accepted. In this connection I may be permitted to contribute some additional facts.

Besides the localities quoted by Prof. Posepny, I would mention the Hauraki or Thames gold-field, in the North Island of New Zealand, where a good opportunity is offered for the study of this subject. In the Coromandel peninsula of the North Island, there is a gold-bearing belt extending for nearly a hundred miles, from Cape Colville to Te Aroha. The prevailing country-rock consists of Tertiary eruptives, through which patches of Carboniferous slate occasionally appear. There are thermal springs scattered throughout the region. At the principal mining center, the Thames, the escape of carbonic acid gas has often caused a temporary cessation of work in the mines. There are soda-water springs in the vicinity of the Thames. At Te Aroha, at one end of the gold-belt, there is a group of celebrated medicinal hot springs. This last locality is connected by a continuous chain of thermal springs with Rotomahana, about 45 miles distant, the famous hot-lake region, the pink and white sinter-terraces of which were known for their beauty throughout the world, until Mt. Tarawera broke out in sudden eruption and destroyed them in 1884.

Veins of gold-bearing quartz, recent eruptive rocks, thermal springs, dying solfataric action, and active volcanic force, are all intimately associated in this corner of the world.

At the Thames, the leading mining town of the island, bodies of gold-ore of unusual richness have been found. In 1871, the Caledonia mine produced 10 tons of gold and paid three million dollars in dividends. In 1878, at the Moanataeri, 5400 pounds of quartz yielded 14,600 ounces of gold. The prevailing country-rock is an andesite breccia, traversed by zones of decomposition, in which the gold-veins occur. At Rotorua, in the hot-lake district already referred to, the plain is in part covered with fragmentary andesite. This material is usually loose and unconsolidated. Near the edges of the fumaroles, which are numerous, it has, however, become cemented, and then very much resembles the country-rock of the mines. The rims of the fumaroles also exhibit products of decomposition, which are similar in character to those observed in the lode-

channels at the Thames, and which, because they are soft and granular, have been termed "tufaceous sandstone." Quartz closely resembling that of the gold-veins of the mines can also be seen to be deposited around certain of the fumaroles and hot springs referred to above. My examination of the ore-occurrence and vein-structure, though incomplete, led me to conclude that the deposition of the gold and its associated minerals had followed certain lines of altered country-rock which had been exposed to the effects of dying but lingering solfataric agencies.*

Another district which affords evidence to help us in studying this subject is that of Pontgibaud, in south-central France, among those volcanic peaks of Auvergne which have been rendered classic by the work of Poulet Scrope. The silver-lead lodes of this district have been very extensively developed, and their geological structure has more than once received notice at the hands of competent observers.† The country-rock consists of gneiss and mica schist, penetrated by dikes of granulite.‡ The lodes are of later date than the dikes, but older than the Pliocene flows of basalt which cover their croppings. The period of their formation is considered to have been between the middle Miocene and the middle Pliocene, very probably contemporaneous with the extension of the acid eruptives of Mont Dore, which took place at the beginning of the middle Pliocene. The lodes generally follow the veins of granulite, and are productive only when so associated. When the dike-rock in which the lode occurs is most feldspathic, the metalliferous filling is most valuable.

In this region mineral springs are abundant, and the escape of carbonic acid gas has frequently put a temporary stop to underground work. This applies particularly to that part of the district through which the river Sioule flows between the town of Pontgibaud and the mines at Pranal. Often, while fishing along the stream, I have noted places where there is a constant escape of carbonic acid gas from its bed to the surface. At Pranal there appears to be an intimate connection between the lode-fissures and the volcanic vents.

* See also "Certain Dissimilar Occurrences of Gold-Bearing Quartz" by the writer, in the *Proceedings of the Colorado Scientific Society*, for 1893.

† *Annales des Mines*, M. Guényveau, 1st series, t. vii., pp. 162 to 188. MM. Rivot and Zeppenfeld, 4th series, t. xviii., 1850, pp. 137 to 257, 361 to 446. Also recently M. Lodin, April, 1892, in a paper entitled, "Etude sur les gîtes métallifères de Pontgibaud," published in the *Annales des Mines*, 9th series, t. i., pp. 389 to 504.

‡ If it were in our West it would be called "porphyry"—a term which has gradually been losing its distinctive meaning through careless use.

One of the mineral veins has been traced to its connection with what appears to be a vent of the extinct volcano of Chalusset. Powerfully carbonated springs exist close to the mines and on the slope of Chalusset.

In both of the two districts above cited, the one in New Zealand and the other in France, note has been made of the escape of considerable quantities of carbonic acid gas. It is scarcely necessary to emphasize the fact that this is a most common and powerful agent in bringing about changes in rocks and minerals. The action of carbonic acid, and of the alkaline carbonates which it forms, have been recognized by all petrographers. To it we owe the salts occurring in ordinary mineral springs; to it are due the pseudomorphic replacement of feldspar with chlorite*, and the alteration of olivine into serpentine, and of limestone into dolomite. Even at ordinary temperatures, carbonated waters extract magnesia from complex silicates. In this way, biotite loses magnesia and iron, becoming converted into muscovite.

The subject of the close association of ore-deposits and igneous rocks is a most important one to mining engineers. The detailed geological surveys of several of the most productive mining districts of the West, carried out during the past few years, have done much to emphasize the relation which seems to exist between bodies of eruptive rocks and deposits of gold- and silver-ore found close to them. It has become the fashion, especially since the publication of Emmons's masterly monograph on the Leadville region, to suppose that the precious metals of the lodes were derived from the leaching of the adjacent eruptives; and some mining engineers have gone so

* And the chlorite afterwards gives place to tinstone. This is a subject much studied by Mr. Richard Pearce, at a time when its importance was not so well recognized as now.—See "The Influence of Lodes on Rocks," *Proceedings of the Mining Association of Devon and Cornwall*, September 8, 1864. Mr. Pearce directs attention to the difference between the granite encasing the lode and that found at some distance from it. He makes note of the joints in the granite, and remarks upon the difference in the minerals found in two well-marked systems of joints having contrary directions. He shows that the changes observed in the rock adjoining the lodes have their origin in the lodes. Emphasizing the metamorphism of the granite he shows that the lodes consist essentially of altered granite, the most important alteration being the replacement of the feldspar by chlorite, by tinstone and by schorl. He discards the idea of an igneous origin of the tin-ore, and declares that aqueous agency alone can satisfactorily account for the changes in the rocks and the formation of the lodes. He expresses the belief that the subject of the metamorphism of the country-rock, if "diligently investigated, must assist in explaining some of the laws which regulate mineral deposits." This was said thirty years ago!

far as to consider the neighborhood of dikes necessary to the occurrence of a productive lode. This latter notion may be classed with the supposition, now slowly passing away, which, not long ago, was so strong, that a "true fissure-vein" was the only permanent depository of the precious metals.

In the United States, in Europe, and in most of the Australasian mining regions, the close association of dikes, or other forms of intrusive eruptive rocks, with lode-formations is so marked, that it is not surprising to find such rocks considered as necessary adjuncts to the occurrence of valuable ore-deposits. But, generalizations are proverbially dangerous; and, that this is an illustration of the proverb, the following facts may show.

The gold-mining region of the province of Otago, in the South Island of New Zealand, is confined, for the most part, to a great series of foliated quarztoze schists of an age considered Archæan by some,* and Silurian by others.† These rocks have an enormous thickness over a large area; the thickness has been estimated at 50,000 feet, while the area is fully 10,000 square miles. This has been a very successful gold-mining region, although the gravel-deposits have, so far, been more productive than the quartz-veins. The lodes have certain well-marked structural peculiarities, resulting from the foliated arrangement of the country-rock which they traverse. In a previous contribution, incidental reference was made‡ to the fact of the remarkable absence, in this auriferous area, of eruptive rocks. It is interesting to recall so marked an exception to what is often held to be a general rule.

That the quartzose schists of Otago are simply altered sedimentary beds of very early geological age, there is little reason to doubt. The quartz folia are arranged along the lines of original sedimentation, and not along cleavage-planes. It is a case of "stratification-foliation," as distinguished from "cleavage-foliation."§ The only rock likely to be a metamorphosed eruptive is the chlorite schist of Queenstown.‖ The mining regions of Otago do not exhibit any of the phenomena of contact-metamorphism; and the changes which

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* "On the Foliated Rocks of Otago," Professor F. W. Hutton, F.G.S. *Trans. of the New Zealand Institute*, vol. xxiv., 1891.

† "The Gold-Fields of Otago." *Trans.*, xxi., 412.

‡ *Trans.*, xxi., 413.

§ Prof. T. G. Bonney uses these terms in the *Quarterly Journal of the Geological Society*, vol. xlix., part 1, p. 95, Feb., 1893.

‖ As pointed out by Prof. Hutton. *Op. cit.*

have been produced may be ascribed to what we call "regional" metamorphism, a vague way of describing those alterations which are forever taking place in rocks wherever there is heat and pressure, alterations which are, therefore, most evidenced by the oldest rocks, which have necessarily been overlaid by a great thickness of later-deposited formations.*

A treatise which covers so wide a field as that of Professor Posepny, can, of necessity, devote but scanty attention to some mining regions which, to those who know them, appear to afford important evidence on the subject of ore-deposition. In this regard, it is to be regretted that Professor Posepny does not seem to have had his attention drawn to certain very excellent geological reports contained in the blue books of the mining departments of Victoria, New South Wales, and New Zealand. Australasia has many object-lessons to offer to the student of economic geology, and the Colonial geological surveys have published several accurate and most interesting descriptions of them.†

In concluding this contribution to the discussion of Prof. Posepny's paper, I may be permitted to express again the belief that his destructive criticism of the lateral-secretion theory is most opportune, and that his investigations into the flow of underground waters will do much to illuminate our views of the methods of ore-deposition. At the same time, I cannot but hold that the accumulation of facts and observations will show that neither the lateral, nor the ascensionist, nor any other one narrow theory can cover the multitudinous diversity of the ways in which ore-deposits are found to occur.

R. W. Raymond, New York City: Concerning Mr. Rickard's proposed classification, I beg to say, while recognizing its convenience for mining engineers, that it cannot be considered as a substitute for that of Prof. Posepny, for the simple but conclusive reason that it is not genetic. Its fundamental division is based upon the

* I do not lose sight of the fact that igneous rocks may become schistose by metamorphism, especially through pressure, as a dolerite becomes a hornblende schist. There is no reason to suppose that such a metamorphism has occurred in these rocks of Otago.

† I would more particularly instance *The Geology of the Vegetable Creek Tin-Mining Field*, by T. W. Edgworth David, and the recently published *Special Report on the Bendigo Gold-Field*, by E. J. Dunn, together with the numerous observations made by R. L. Jack, in Queensland; H. Y. L. Brown, and H. P. Woodward, in South Australia; G. H. F. Ulrich, and F. W. Hutton, in New Zealand; Wilkinson and Liversedge, in New South Wales; Murray, Sterling, and Howitt, in Victoria.

position of the deposits, which should be, in a genetic classification, a subordinate consideration; and the most profound genetic distinction presented by nature, namely, the distinction between contemporaneous and subsequent formation, appears in this scheme as a division of the third degree, affecting only inclosed bedded deposits. If I were inclined to criticize names, as Mr. Rickard has elsewhere done with regard to Prof. Posepny, I might point out that the word "contemporaneous" does not describe coal-beds, which Mr. Rickard mentions as typical examples of it. Whatever may be said of a coal-bed, it is not contemporaneous in origin with the country-rock above it or below it. But this is a small matter. The point I make is much more important, namely, that the classification itself is neither based on genetic distinctions nor on any other logical arrangement. I say this all the more frankly, because, as Mr. Rickard declares in complimentary phrase, he has largely followed the classification given by me in 1869. But that was, as Mr. Rickard's is, merely a convenient miners' arrangement. Now that Prof. Posepny comes forward, proposing for the purposes of science, not of mining, a truly genetic classification, a critic may fairly demonstrate its logical defects and suggest remedies, or declare remedies to be impossible. In the latter case, his contention would be that a genetic system cannot be constructed, and that the attempt had better be abandoned. But to say that one prefers, as a mining engineer, the handy non-scientific arrangement of ore-deposits hitherto in use, is no criticism at all. It is as if a botanist, considering a natural system in botany, should say that it was discouragingly complicated, and that he preferred the simple and convenient arrangement of Linnæus, by which one could identify a species from the number of petals and stamens and other arbitrary signs.

H. V. Winchell, Minneapolis, Minn.: While heartily agreeing with the frequently-expressed opinion that Prof. Posepny's paper is a masterly and exceedingly important discussion of ore-deposits, it still appears that there may be room for differences of opinion on some points. Indeed, they necessarily follow from such decided statements on so important and interesting a subject.

Those of us who live in the Lake Superior region are wont to believe that we have some conception of the meaning of the term "ore-deposits." We can, and frequently do, point with pride to the great value of our production of iron-ore and the fact that we furnish nearly two-thirds of the total product of the United States. It is an industry employing about 30,000 miners and involving

capital to the amount of fully $100,000,000. But when we come to treatises on ore-deposits we are always disappointed. We find that, while speaking generally and theoretically, iron-ore deposits may be mentioned, yet when it comes to critical discussion, and the illustration of theories by examples, they are omitted. We are constrained to protest that "ore-deposit" does not signify merely a vein of gold-, silver- or lead-ore or a stockwork of tin- or zinc-ore, but that hematite and magnetite form ore-deposits of a commercially important and genetically highly interesting class.

The value of the raw iron-ore produced in this country in 1889 was equal to the value of the gold bullion produced in the same year. And if we take the value of the pig-iron, which more nearly corresponds with bullion in the degree of removal from the raw material, we find it equal to the value of the gold and silver combined. And yet our author dismisses the entire subject in a couple of pages, and of Fuchs's and DeLaunay's 2000 pages only two are devoted to the most important iron-ore district on the globe.

It would not be fair to suggest that iron-ores are overlooked because they do not seem to be explainable by the theories adopted for other classes of deposits. If that were the case, all the more need of giving them attention. It is more probable that it is because of the recentness of their development and the comparatively scant literature on the subject in the libraries of our foreign colleagues.

That the circulation of waters carrying different chemical reagents is the all-important factor in the genesis of ores, as we find and mine them, is clearly shown by Prof. Posepny, and is accepted by the majority of writers on the subject. But the prominence which is given to ascending waters and the insignificant effects ascribed to descending solutions will not find such ready acquiescence. It seems likely that ascending waters are the more likely to be effective and to predominate below the ground-water level than in the vadose circulation. But it can be conclusively demonstrated that many of the immense iron-ore lenses of the Lake Superior region owe their present state of concentration, even to the depth of many hundreds of feet, to the action of the descending waters. Aside from the Mesabi range, the proofs lie partly in the following well-known facts:

1. The ore is a product of concentration *in situ*, whether the original rock or lean ore was an oxide, a silicate, or a carbonate, or whether it was oceanically or otherwise precipitated.

2. The ore-bodies have the shape of highly-inclined lenses, and frequently have an unaltered "capping" of jasper partially covering their upper ends.

3. When this capping is present, it can be traced downward into the ore through changes which are clearly the result of oxygenated atmospheric waters.

4. The downward course of the waters is further shown by the protecting action of dikes and other impervious barriers, below which the ore is not found.

5. The ore-lenses lie in basins of greenstone-schists or other rocks, and occur at various depths to at least 2000 feet.

6. At the lower edges of some of these lenses are found deposits of silica, kaolin, etc., which have plainly been removed from the ore-body above in the process of concentration.

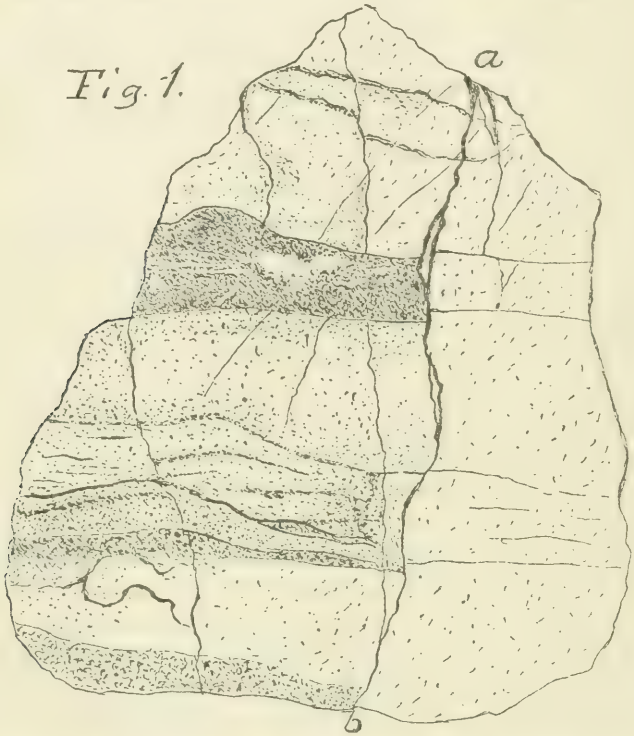
This is much below the vadose circulation, as the immense pumping engines and the rivers of water which they throw the year round testify; but it is an instance of the formation of ore-deposits on the largest scale by descending waters.

The circumstances are somewhat different on the Mesabi range, but the proof is no less clear that the ore has been formed by solutions percolating downwards. There the mines lie along the south side of the continental divide or water-shed, from which waters flow north to Hudson Bay and south to the Gulf of Mexico. They thus occupy the highest regions of the northern part of the State. Moreover, the shape of the strata, and the presence of a conglomerate beneath them, indicate that there was a shore-line there when the rocks were deposited. These facts, with the comparatively undisturbed condition of the strata, lead us to believe that the conditions have remained during many geological ages as they were originally and as they are now, viz., such that the inevitable direction of water-circulation would be downward and following to a certain extent the gentle dip of the rocks to the south.

Although of remarkable magnitude and chemical purity, these deposits are essentially surface-products and are at present largely above the ground-water level. The processes of replacement by the removal of silica, and of concentration by the addition of sesquioxide of iron, can be seen in progress in a hundred places. The rock which undergoes this change is a gray, reddish or greenish chert ("taconite") banded with iron-ore. Figs. 1 and 2, taken from specimens from the Mesabi, illustrate the change mentioned, and show the downward course of the ferruginous solutions.

Since we have here examples of iron-ore deposits, both above and below the ground-water level, which have been formed by descending waters, the thought naturally arises that the solutions may not have been so universally ascending in the case of other mineral deposits, as our author would have us believe.

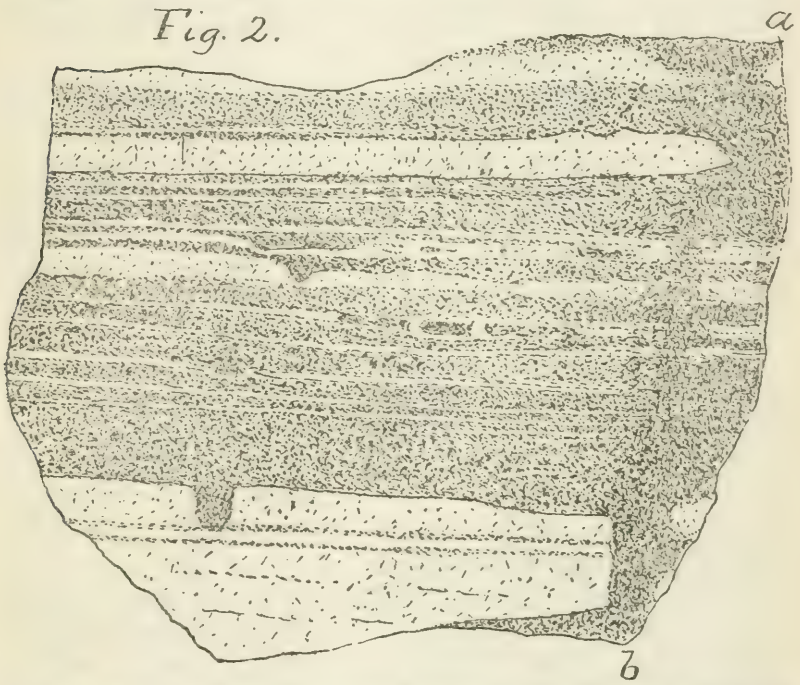
Another idea on which undue stress seems to have been laid is the correctness of the "ascension theory," and the absolute error of



Taconite from the Mesabi range changing to iron-ore by solutions moving from left to right. *a b* is a fault line which conducted the descending waters downward and prevented the right half of the specimen from undergoing the ferrification which is seen in the left half.

that of "lateral secretion." A consideration of these two ideas leaves me with the impression that they are not in reality so diametrically opposite that if one is true the other can have no scintilla of truth in it. In the deep region the circulating waters are supposed to be under considerable pressure, from which they escape by flowing in the direction in which they meet the least resistance. Even if the solution were on the whole ascending, still it must often

happen that cracks and fissures would be encountered, leading in a lateral direction into some main fissure, full of ascending waters under slightly less pressure than that behind the waters which entered laterally. In that case it is also quite likely that there would be a different chemical reaction at or near the junction of these two circulating fluids from that produced by the action of either one of them on the rocks through which it passed. This might result in



Another instance of partial alteration of taconite to ore. There was a joint here along *a b* whence downward moving waters effected a more rapid change for some distance laterally than the solutions percolating toward this joint along the strata from left to right were able to produce in the solid rock. Specimen collected by J. E. Spurr.

the precipitation of certain minerals on the walls of the main fissure near the subsidiary fissure, and thus the resulting ore-deposit would owe fully as much to lateral secretion as to ascension. And if these lateral joints and cracks (or even more porous rocks) were sufficiently numerous, the whole vein, when formed, would be due to the combined actions of lateral secretion and ascension.

Moreover, it seems almost necessary for the ascensionists to borrow aid from the lateral secretionists, whether they will or no. For the

question arises: Where do the ascending solutions come from, anyhow? Is there an inexhaustible reservoir at the bottom of each vein-fissure, which supplies a ceaseless flow of carbonated and mineralized waters carrying precious metals in solution? Or does the water start from the surface and percolate downward until it is forced by heat and generated gases to rise again? If the latter is the true supposition, is it not evident that the fissures which conduct these ascending waters must receive them from all sides through a thousand small crevices and pores, thus making again a combination of both lateral and ascending motions and depositions?

If ascending waters come from a great depth, descending waters must reach to the same great depth, and since the solutions cannot traverse the same path in their ascent that they do in their descent there must be a certain amount of lateral motion at the moment when these solutions are the most dense and carry their heaviest burden of dissolved material. And it is evident that, whatever the depth from which the metallic elements come, there is as much chance for one mode of deposition as for the other.

Prof. Pošepný (communication, translated by the Secretary):* First let me express my warmest thanks to all those who have so favorably judged my paper on "The Genesis of Ore-Deposits," and likewise to those who have taken this occasion to bring forward, whether in support of my views or in opposition to them, various observations and opinions, whereby our knowledge of ore-deposits has been unquestionably increased.

It is exceedingly difficult—indeed, almost impossible—to make a correct comprehensive statement of a subject, the separate fundamental data of which are scattered throughout the world; and my treatise must, of course, be considered as merely an attempt in that direction, inspired by the purpose of contributing to this theme an element not yet sufficiently recognized, namely, the logical application throughout of the genetic principle. As I indicated in my paper (*Trans.*, xxiii., 204), I expected as a result neither a simplification of systems nor a direct benefit to practice. My object was, irrespective of such considerations, to approach more nearly to the truth.

A single observer may be able to establish a few more or less important facts; but the great mass of the knowledge required he cannot personally possess. In the most favorable case, government

* This communication, and the following discussions of the subject in this volume, were published with the papers of the Bridgeport Meeting, October, 1894.—R. W. R.

institutions, established to benefit single nations, or scientific or business associations, may procure accurate knowledge of the mineral resources of separate countries, and these may be combined to increase the knowledge of a considerable territorial complex; but the question still remains, whether the developments and natural exposures in a given region are really typical and conclusive as a basis for general scientific deductions. In this respect, an international union of such endeavors, devoted to the advancement of this branch of geology would be a decisive gain.

When the United States Geological Survey began the study of the geological relations of ore-deposits, there was ground for hope that a new era in the knowledge of this subject would be thereby inaugurated. In fact, several monographs of inestimable value concerning the most important ore-deposits had been published, when, for reasons unknown to me, the whole activity of the survey in this direction was interrupted—an event much to be lamented.

Yet a monograph can give only what is revealed by the developments accessible at the time it is written; and since mining continually makes new exposures, and for the most part obliterates the old ones, a complete scientific inquiry should involve provision for the repeated examination of a given mining district, and for publication, at intervals of, say, five or ten years, of the new knowledge thus acquired.

It is scarcely to be doubted that the investigation of the genetic relations of a thing is necessary to complete our knowledge of it, and that this inquiry is therefore obligatory as a part of the study of anything which we desire to know exhaustively. Dr. Raymond (discussion at the Virginia Beach Meeting, *ante*, p. 956) has defended the introduction of this principle into the science of ore-deposits, for which I thank him heartily.

Messrs. W. P. Blake and A. Winslow have controverted my views concerning the original source of the lead- and zinc-deposits of Missouri and Wisconsin, condemning at the same time the similar views brought forward at the same Chicago meeting in the paper of Dr. W. P. Jenney. Since I am personally acquainted only through a tourist's journey with the relations of these deposits, which extend over so large a region, and am, moreover, not master of the wide literature of the subject, I must leave the defence of the principles asserted to Dr. Jenney, and will here simply refer to his reply, contained in the present discussion.

With regard to Mr. Winslow's observations, I must confess, that

I am acquainted neither with the mine at Doe Run nor with the publications of Messrs. Strong and Chamberlin. But I know that concerning every region where lead- and zinc-ores occur in limestone and dolomite, the two opposite theories as to their origin invariably appear; and that in terranes consisting of structural plateaux, with nearly undisturbed position of strata, the representatives of the view that these ores were deposited simultaneously with the country-rock have the great advantage that the conditions of stratification are in their favor.

Besides the paper here in discussion, I have lately devoted to the deposits of lead- and zinc-ores in soluble rocks a special treatise,* in which I have compared the occurrences of such deposits in plateau-regions with the conditions obtaining in mountain regions, with already disturbed stratification. This publication originated in an address delivered by me at a miners' congress in Klagenfurt, that is to say, in the center of a mining industry based upon mineral occurrences of this class.

In order to counteract a conception based upon local conditions, I have placed side by side the various alpine occurrences of Carinthia with those of the plateaux of Upper Silesia and North America, illustrating them, according to my custom, with drawings of the typical features. Among others, the occurrences in Sardinia and in the North of England are discussed, and use is made of recent literature concerning the Upper Silesian plateau. In this place, I can only remark, that some of these occurrences in the mountain terranes carry evident traces of the subsequent derivation of the ores from below; and that this fact alone is an argument for the similar origin of the plateau-deposits, which so closely resemble the former in all other respects.

The treatise I have mentioned does not include the observations made by me in the spring of the present year, upon the analogous deposits of Laurium in Greece, which are likewise in a structural plateau; but I can assure the reader that the developments of that region also indicate the derivation of the ores from below.

So far as Mine la Motte is concerned, I can attach no great weight to the observations which I made there, upon a hasty journey. Nevertheless, the specimens of ore disseminated in sandy dolomite which I brought away show distinctly upon the surfaces, after polishing, the secondary intrusion of the ore into the country-rock.

* "Ueber die Entstehung der Blei-und Zinklagerstätten in auflöslichen Gesteinen."—*Jahrb. d. k.k. Bergakademien*, xlii, 77, 1894.

With regard to Mr. T. A. Rickard's criticisms, I would observe, that formerly the theories of ascension, descension and lateral secretion were generally spoken of, without the assignment of any cause for the assumed movements of the subterraneous liquids. I think, however, that I have secured some definiteness of conception by showing the actual descent of the vadose circulation and the ascent of the deep circulation, and by interpolating the lateral movement between the two. This gives reality to the processes formerly conceived abstractly, and makes it possible to discuss them.

Mr. Rickard observes that, with reference to the formation of ore, I have laid special emphasis upon *ascending* mineral solutions (*Trans.*, xxiii., 589). I meant to do this, however, only with regard to the sulphides. These certainly were not produced from the *descending* solutions, which carry oxygen now, as they unquestionably did in former geological periods also, and which invariably decompose sulphides wherever (as is the case in the vadose zone) they come into contact with them. With regard to the sense in which I use the terms *ascending* and *descending*, I will say something below.

Mr. Rickard suggests (*loc. cit.*), that since the increase of pressure and temperature favors solution, while their decrease favors precipitation, precipitated ores are to be expected rather in the shallow zone; and that this might explain the circumstance that (as he believes) ores do not continue in depth. Without going into the latter question in detail, I would point out that the conceptions of shallow and deep are only relative, and that in my discussion I could only have in mind the conditions existing at the time of the formation of the ores, and not at the present time. What was once shallow may now lie very deep, and *vice versa*. In this respect, the character of the ores is, I think, the decisive fact. Oxidized ores must have become such in a zone then shallow, and original sulphides must have been deposited in a zone then deep, and beyond the reach of oxidizing agencies. For the present, only the extreme of these processes can be clearly recognized; but it is not impossible that future studies in this direction may distinguish the characteristics of the intervening stages of formation, such as the deposits made during lateral movements of the mineral solutions.

It would certainly be a step backwards, to allow the established characters of the two extremes to disappear under the general term "circulation." In my description of the vadose circulation I have pointed out that, notwithstanding its course at the ground-water

level appears to be almost horizontal, and notwithstanding an actual ascent of the liquid may be locally brought about by siphon-action, nevertheless a decided prevailing descent can be proved for the vadose currents. The terms "descending," "ascending" and "lateral" are not applied to a portion, but to the whole line of the current; and to its cause, as both theoretically and empirically determined. I cannot admit that this is "a narrow view of the subject," likely to hinder progress in this branch of geology; on the contrary, I believe it expresses a series of observed facts, calculated to increase our knowledge.

Mr. Rickard seems to look at every new conception in this department from the sole standpoint of its immediate usefulness in mining, and not to reflect that the scientific investigator has simply to seek the truth, without regard to such considerations. His criticism might have been more favorable in some particulars (*e.g.*, Virginia Beach Discussion, *ante*, p. 950, with reference to *Trans.*, xxiii., 249), if I had taken pains, in many cases in which I was speaking of "ore-deposits," to explain that under this phrase, used for brevity, I was referring to deposits carrying metallic sulphides.

Mr. H. V. Winchell complained, at the Virginia Beach meeting, that under the head of ore-deposits, the deposits of iron-ore are too often either meagerly or not at all considered. This complaint would be well-founded as against a report on the mineral resources of a given region, in which the economic importance of the deposits is a controlling element; but it is scarcely just in its application to a paper like mine, which was intended only to give single instances in illustration of certain genetic theories. The reason that iron-ore deposits generally receive comparatively little attention in genetic discussion is, I think, the simplicity of their conditions, the knowledge of which is to some extent assumed to be familiar, so that authors interest themselves much more in the discussion of the more complicated occurrences, which have rarely, as a rule, been correctly interpreted.

I am indebted to Mr. Winchell for making good my omission by adding to my paper his account of iron-ore deposits known to him. Since the deposits he cites consist of oxidized ores only, they may well have been formed by an originally vadose circulation. I must, however, point out that some iron-ore deposits may be of idiogenous origin. Thus, I consider the oölitic structure of some deposits (*e.g.*, those of hematite in the Silurian of Central Bohemia) as a sign of their original deposition in the basin. I have had, however, far

too little to do with these deposits to be able to determine more closely the significance of the remains of brachiopods (*e.g.*, orthis shells), which occur, transformed into hematite, together with the oölites.

The iron-ore beds of the Silurian basin of Bohemia have a certain analogy with those of the Huronian basin of Michigan, especially as regards the length and continuity of their outcrops and their connection with tufas of the eruptive rocks. In the latter, as is indicated by the beautiful pseudomorphs of chlorite after garnet, considerable metamorphosis must have taken place.

Concerning the Mesabi iron-ores, I am indebted to this critic for the illustrations of two specimens which he has published. They, indeed, suggest reflections as to their probable genesis, upon which, however, I do not trust myself to venture at this time.

In reply to Mr. Winchell's criticism that, while laying unnecessary emphasis upon the correctness of the ascension-theory, I appear to concede to the theory of lateral secretion not an atom of truth, I beg to observe:

1. That I deem lateral secretion, in the sense in which it is defined by Professor Sandberger, to be possible only in the zone above the ground-water-level, and, therefore, in the formation of oxidized ores only, and not for sulphide-ores.

2. That I am, indeed, obliged (as I have shown, *Trans.*, xxiii., 222) to assume a lateral movement of liquids in the deep zone. But this is a region in which present processes cannot be directly observed, and, therefore, no clues to the conditions of deposition are found. Hence, I was not able to describe such conditions in my paper. It is possible that, in the course of time, conditions of deposition may be discovered which can best be explained in this way. I have not yet encountered such a case.

The same is true as to regions in which the two extreme branches of the subterranean circulation take on a lateral course. The case supposed by Mr. Winchell, in which a deposit can be ascribed to ascension and also to lateral secretion, I do not clearly understand, since a physically weaker current is not capable of displacing a stronger one. While the extreme forms of circulation—that is, both the ascending and descending branches—possess a pronounced character, it must be expected that the character of the branches connecting these extremes will be less distinct.

Mr. John A. Church does not agree with me regarding ore-deposits in open spaces as a very frequent phenomenon, and expresses

the opinion that open spaces cannot exist at great depths (such as 3 to 5 kilometers). I must remind him that in order to establish the first proposition the most important observations of a great number of observers for more than a century must be disproved. He cannot have failed to notice that ore-deposits of that form which has been relatively most thoroughly studied, namely, fissure-veins, consist predominantly of separate crusts, often marvellously distinct, covering what were once the walls of the fissure-space. Even if his proposition be confined to deposits of great thickness and extent in depth, which are deemed to have been formed (as, for instance, the Comstock lode, which he has studied) by substitution, replacement or metasomasis, he cannot possibly deny the existence of other thick and deep deposits, the structure of the ores of which evidently represents the filling of open spaces. For instance, some of the Przibram veins, which have been worked to the depth of more than 1100 meters, and the ore of which often exceeds 10 meters in thickness, must certainly be reckoned as wide and deep; yet the ores from their deepest portions do not differ in the least, so far as structure is concerned, from those which occur in the shallower parts. Both regions present fragments of the country-rock of all sizes, surrounded by the vein-material. Moreover, these fragments surrounded by quartz usually predominate in one or the other of the crusts of the vein-filling.

Mr. Church seems to allow small value to the observations which it is possible to make upon the ores themselves and the adjoining country-rock. This is equivalent to the rejection of the only means of obtaining data concerning their probable genesis. It is difficult to discuss such an objection, particularly in its bearing upon the phenomenon of crustification, which I consider one of the most important genetic factors, and concerning which I will speak further in connection with my reply to other critics.

Mr. Church declares the Comstock vein-mass to be the product of substitution—that is, of metasomatic alteration—and denies entirely that it is a fissure-vein. He says I have misunderstood him in saying (*Trans.*, xxiii., 281) that he found crusts of quartz, alternating with calcite, in the Justice mine. The passage to which I referred was the following: *

"The ore of the Justice is not quartz but calcite, with but an insignificant amount of silica, and it is noteworthy to find these two components of the lode disposed

* *The Comstock Lode*, etc., by John A. Church, New York, 1879, p. 173.

in that banded arrangement, which is another of the accepted proofs of a true fissure-vein. The quartz is always on the propylite and the calcite on the quartz; but there is no comparison in respect to quantity. The quartz is always insignificant in thickness, never reaching a layer more than an inch or two wide, so far as noticed, except in the dyke-vein, while the calcite forms masses which are several yards in thickness," etc.

Why is this not what I call crustification? It is certainly conceivable that the Comstock was formed by the opening of a space of discission at the contact of diorite and diabase, the filling of this space by the deposition of silica and carbonate of lime from solutions, and the repetition of these processes until the deposit had attained its present thickness. There is, for example, in the collection of the University of Vienna, a large slab from the Adalbert vein at Przibram, showing a series of thin galena-veinlets, the crystals of which meet in the axis of each several veinlet, showing that each was separately filled, and hence that the process of opening and filling, regarded with reference to the Adalbert vein as a whole, was repeated many times, until the aggregate thickness of about one meter, shown by this slab, had been attained. The Comstock might have been formed likewise by repeated opening and filling, only the several fillings would have to be thicker, and (since the material varied little) the result might be too indistinct to attract the attention of the miner.

Mr. Church regards the ore-body of the Justice mine as a deposit separate from the Comstock; but it is, nevertheless, a branch of the Comstock lode, and certainly has an analogous origin.* The occurrence of a crustified portion, which I think the text of Mr. Church's description indicates, possesses, therefore, significance for both branches of the Comstock.

By crustification, however, I do not mean merely a "banded structure." This may indeed originate, as Mr. Church says, in various ways, but crustification cannot; for true crusts are predominantly chemical precipitates, the crystal-aggregates of which present a certain arrangement. For instance, the quartz-crystals usually stand perpendicular to the former cavity-wall, directing their pyramidal surfaces towards the central druse. Incrusted fragments exhibit the same crusts as the cavity-walls, which is, at the same time, an additional proof of the existence of an open space, etc. It is true, that among these chemical precipitates there sometimes occur mechanical sediments, such as frictional detritus, which may be en-

* See Becker's *Geology of the Comstock Lode*, p. 30.

veloped by one or another of the crust-substances; but this is by no means a case under Mr. Church's statement (*Trans.*, xxiii., 596):

"Certainly, a banded structure can arise from the replacement of fragments arranged in layers by pressure and friction, as well as in many other ways, and does not prove deposition in a cavity, whether filled by water or air."

Pressure and friction can give rise to no arrangement of xenogenites in separate crusts; in other words, no crustified quartz and calcite filling, such as I suppose to exist in the Comstock. I possess, for example, besides the ores from the Consolidated Virginia bonanza, mentioned in my paper (*Trans.*, xxiii., 281), some quartz specimens from the 1500-foot level of the Belcher mine, in which separate dark ore-bearing zones may be distinguished, running parallel with each other, even to the repetition of minute undulations. This is, I confess, not such a convincing case as that of the specimen shown in Fig. 53 of my paper, which exhibits numerous successive crusts of barite, fluorite, etc., no thicker than paper; or those of the Raibl specimens, which consist of thousands of thin layers of zincblende (whence the name *Schalenblende*); but it indicates, at least, the probability of a similar origin. It is, of course, not in every ore-deposit that such incontrovertible proofs as those last mentioned are found and preserved for science.

Mr. Church points out (*Trans.*, xxiii., 594) that metasomatic processes effected in limestones through the expulsion of the carbonic acid by a stronger acid, may also explain the exhalations of carbonic acid so frequent in certain localities. I much prefer, however, to avoid the adoption of such a purely speculative standpoint, and would only suggest that, upon that view, the enormous volume of such exhalations in volcanic regions would require us to conclude that in those regions immense masses of limestone are undergoing the metasomatic process referred to.

As regards, finally, the subsequent alteration of the original ore-deposit, which, according to Mr. Church, partially passes into hysteromorphism, it is undoubtedly true that mineralogists, devoted to the study of pseudomorphs, have collected already valuable data in this field; yet, I think, prolonged investigation will still be required before general deductions can be profitably discussed.

Mr. S. F. Emmons, whom I have to thank warmly for his favorable judgment upon several portions of my paper, naturally does not concur in the views I have expressed concerning Prof. Sand-

berger's lateral-secretion theory, to which he was himself at one time more or less committed.

He objects, for instance, to my reference to the barysphere. This is a part of my conception of our planet as consisting outwardly of several successive, and more or less connected, spherical envelopes—atmosphere, hydrosphere, biosphere, lithosphere, and barysphere—of which only the exterior ones are open to our direct observation. In discussing the mutual reactions of these great geological factors, which we may briefly call aggregate-spheres, it is unavoidably necessary to refer to the barysphere, which is beyond our observation; and, according to my habit, I have used this term in speaking of the source of the heavy metals. It is true, the term is only a device to avoid questions still unsolved; but the same may be said concerning the phrases "unknown depths," or "unknown sources in depth," which have a similar meaning.

It seems to me, that Mr. Emmons and others of my critics have not correctly understood my statements concerning the several branches of the underground circulation; and I therefore beg permission to make my meaning clearer, even at the cost of a little repetition. For this purpose, I will take for illustration, not an ideal case, but conditions actually existing, namely, those developed at Przibram, concerning which there exists an abundant literature, shortly to be increased (in the second volume of my *Archiv für praktische Geologie*) by a monograph of my own.

The Przibram district lies, in round numbers, about 500 meters above sea-level, and the mine-workings extend, as is well known, to more than that distance below sea-level. The ground-water level is but a few meters under the surface. The deepest adit drains the mines to about 100 meters; and everything below that level is strictly deep workings, from which the water is lifted to the adit-horizon. A comparison of the water raised from different levels shows that the largest quantities come from the upper ones, and that the amounts diminish with increasing depth, so that at about 300 meters below sea-level no water remains to be raised, the ruling rock- and air-temperature of about 23° C. (74° F.) at that depth sufficing to evaporate the small existing quantity of water. This is certainly a striking proof that the water encountered in mining is of atmospheric origin.

The ore-deposits are steeply-dipping fissure-veins, which are mined by reason of their richness in silver (about 5 per cent., or 50 kilos per metric ton—or say 1458 ounces Troy per ton of 2000

pounds avoirdupois). Even in the neighborhood of the surface the sulphides predominated, but were mixed with a great variety of beautiful minerals, which have made Przibram famous among collectors, and most of which, according to the results of the investigations of F. A. Reuss and others, are of secondary origin. It cannot well be doubted that this alteration is due to the oxidizing properties of the liquids coming from the surface. But this variety of minerals is confined at Przibram to the upper zones. Since mining has penetrated to lower levels, its product has been mainly only rich argentiferous galena, with accompanying zincblende, etc. The diminution in secondary minerals, so far as it can be determined, seems to follow closely the progressive diminution, in depth, of the quantity of surface-waters.

Concerning the origin of the secondary alterations, there is (as Mr. Church may be pleased to know) no doubt at Przibram. The only question at issue concerns the explanation of the original vein-filling, consisting of sulphides. This must have come, of course, from some rock as a source; and on this point views are at variance.

1. Professor Sandberger at first conceived that this filling came directly from the country-rock (*Nebengestein*). The technical term *Nebengestein* is more definite, perhaps, than "country-rock." It means literally the rock alongside, or the country-rock or wall-rock immediately in contact with the deposit. In this sense, it is impossible to conceive of any other process than that of lateral secretion, which could make the *Nebengestein* the source of the filling; and I have attempted in my paper to show the improbability of such a lateral secretion of such a filling.

2. Mr. Emmons, in his paper on "The Geological Distribution of the Useful Metals in the United States," read at the Chicago meeting (*Trans.*, xxii., 53), has connected the derivation of the various metals of different deposits with the observed geological conditions of that country, discussing the metals, iron, manganese, nickel, tin, copper, lead and zinc, mercury, and gold and silver separately. In his criticism of my views in this field (*Trans.*, xxiii., 598), he has taken occasion to express a general statement for all ore-deposition. According to his opinion, the metallic constituents were derived by lateral secretion from rocks within "reasonable proximity;" and "ore-bearing currents may in such cases have had an upward, downward or lateral motion, according to differing local conditions of rock-structure." This latter expression I would like to amend in accordance with the fact that, while the local conditions of rock-

structure indeed influence the movements of liquids, the true causes of the upward, downward and lateral motion, as explained in my discussion of this point, lie outside the particular rock-structure.

I would invite Mr. Emmons to take the standpoint sketched in my paper (*Trans.*, xxiii., 248), in the depths of the Przibram mines, and see how he would get along with his assumption of lateral movement. And I must repeat that it is not so much the local direction of the currents, as the general character and cause of the flow which should be kept in view.

The general phenomenon of descending currents in the Przibram mines is clearly subsequent to the formation of the ore-deposits; and the existence of lateral movements of the vadose circulation which could form these deposits is inconceivable. Let us see, then, whether such movements could occur in depth, in the sense defined by me (*Trans.*, xxiii., 222), and quoted by Mr. Emmons.

We should be forced to assume that the open vein-channels had not extended much deeper than the point (500 to 700 meters below sea-level) at which I have invited Mr. Emmons to stand, and also that there was no special upward tendency of the waters filling these channels. A lateral continuous movement would be only possible if there was something "in reasonable proximity" which would consume the moving current, or force it back to the surface. To expect this phenomenon in a terrane already traversed by channels reaching to the surface is irrational. In the only conceivable sense, it would merely make the lateral movement an incidental part of a general upward circulation. But this favors my view of the ascent of mineral solutions from greater depths than have yet been reached in mining, *i.e.*, from "unknown depth," as Mr. Emmons expresses it, or from the barysphere, as I have expressed it. He also, by the way, assumes the origin of the heavy metals from the barysphere (or "from the depths," as he prefers to say), and goes so far as to intimate that I would make the theory more plausible by allying it with that of Vogt, according to which a process of so-called differentiation, during the cooling of the eruptive rocks, has concentrated their metallic contents in certain regions more or less accessible to our observation. For my part, I must wait until Vogt's ideas have assumed a more solid form; but I cannot help suspecting that Mr. Emmons favors them principally because they bring the concentrated metals in eruptive rocks within the reach of lateral secretion, as a forming process for ore-deposits.

Mr. Emmons doubts my conclusion, based upon Nöggerath's ob-

servations, that waters rising under pressure are capable of creating a channel for themselves in soluble rocks. In this connection I must refer to the difficulty encountered in explaining the cavities containing pipes of ore in soluble rocks. In my monograph on Rézbánya,* published when Nöggerath's work was unknown to me, I was forced to assume, as the cause of the formation of the cavity, the downward vadose currents, and as the cause of the filling, on the other hand, the ascending currents of the deep circulation; in other words, two processes, representing the extremes of circulation, and successively acting along the same line. Such a dilemma may be presented by any ore-deposit in limestone. Indeed, I became acquainted subsequently with instances indicating that the two processes of cavity-forming and cavity-filling may have been sometimes almost simultaneous.† I was greatly pleased when I learned of Nöggerath's observations and deductions, and I took pains at that time to acquaint Mr. Emmons by letter with the consequent change in my own views. The observation, as I convinced myself in 1885, cannot now be verified, for the whole place at Burtscheid is completely built over; but Nöggerath was a highly conscientious observer, and there can be no doubt of the correctness of his statement of the facts. Moreover, the phenomenon is, *a priori*, inevitable. If the highly dilute currents of the vadose circulation, descending by gravity, can eat out their own channels in salt or limestone (as is shown at considerable length in my paper), all the more might such effects be expected from waters ascending under pressure and more highly charged with reagents. Fig. 9 of my paper, showing the wedge-shaped spaces of corrosion described by Daubrée from Bourbonne-les-Bains, with their summits directed upward, gives actual proof of this.

My reference to the wedge-like form of certain deposits at Laurium was based on an ideal profile. In the spring of the present year (1894) I personally visited the district, and strove to secure more accurate drawings of the position and form of the deposits. I must confess that I was not able to find any such drawings, and I must therefore submit to the rebuke of Mr. Emmons. So far as I know the literature concerning the Laurium district, the only accu-

* *Geologisch-montanistische Studie der Erzlagerstätten von Rézbánya, in S. Ungarn.* Published by the Hungarian Geol. Soc., Budapest, 1874.

† See my paper in *Jahrb. der k. k. Bergakad.*, xlii., 1894, p. 94, "Ueber die Entstehung der Blei- und Zinklagerstätten in auflöslichen Gesteinen," especially Fig. 14, Pl. III.

rate drawings are those of the French company in the treatise of A. Cambresy.* (I take this opportunity to correct a typographical error in the pamphlet edition of my paper. Fig. 87 was taken, not from Cordella but from Huot.)

With regard to the essential difference of opinion concerning the Leadville deposits, I may observe that the reason I ventured to discuss that district without having personally studied it, is to be found in the magnificent monograph of Mr. Emmons, the interesting conditions which it describes, and its contradiction of current views as to the origin of the Leadville ores. Passing by all corrections and criticisms on points of minor importance, I wish only to keep in view this essential difference of opinion, and to inquire what were the convincing reasons which led Mr. Emmons to assert in this case a descent of the mineral solutions.

He separates the sources of the metallic substances into "immediate" and "ultimate." The latter, by reason of their purely speculative nature he does not discuss, but devotes himself to the former. Without being able to doubt that these substances originally came from great depths, and without being willing to assert that they came wholly from the country-rock actually adjoining the deposits, he believes:

1. That they came from above.

2. That they were derived chiefly from neighboring rocks.

With regard to the first of these propositions I can find in his elaborate monograph no tangible proofs whatever, only conclusions deduced from certain observations. The shape and position of the ore-deposits, whether of those at the contact between porphyry and lime, or those in the limestone, afford no conclusive proof of descending mineral solutions as their source. Indeed, this is disproved by the fact that the deposits were originally sulphides (as they are now shown still to be at greater depths), and such sulphide-deposits cannot be asserted to have been formed by solutions descending from the surface (unless such an application should be made of the case cited by me, *Trans.*, xxiii., 294, namely, the reduction to sulphides by means of organic matter). The interior structure of the deposits and of the country-rock, so far as they are described in the publications on the subject, likewise fail to furnish any conclusive proof of this assumption.

In his re-examination of the mines in 1890, Mr. Emmons found,

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* "Le Laurium," par A. Cambresy, *Rev. Univ. des Mines*, 3 ser., t. vi., 1889.

even in the original, unaltered sulphide-ores, no crustification, from which he concludes that in this case there has been no deposition of ore in open spaces, but a metasomatic replacement of the limestone. It is to be hoped that investigations on this point will not be wholly abandoned in future. Mr. Emmons mentions also his recognition of the granular structure, joints and cleavage of the original limestone in the sulphide-ores of the A. Y. and Minnie mines, and speaks of the cracks in the top of the ore-body, "through which the ore-bearing solutions had descended." This is clearly, as stated in this form, an expression of opinion. A detailed and purely objective description, particularly if accompanied with drawings, would be highly valuable, and might constitute the tangible proof, the absence of which I have pointed out. Mr. Emmons gives us ground to hope for further observations in this direction, based upon the latest developments of the mines. For the present, however, it cannot be said that we have any decisive proof from the interior structure of these deposits.

The facts described in the literature concerning Leadville may be equally well used in support of the ascension-theory. As I have remarked (*Trans.*, xxiii., 294), the ores were at first conceived to occur at the contact between porphyry and limestone, or confined to the lime; but afterwards it became clear that not the whole contact-surface as such, but only certain zones of it, could be regarded as the principal centers of the accumulation of ore. These ore-shoots, lying in and near the contact-plane, were the channels of which the mineral solutions availed themselves. A parallel is thus furnished to various other ore-deposits; for instance, the zinc- and lead-deposits of the Alps, the shoots of which are near a contact of soluble with insoluble rock, and pursue the same direction as the stratification.* For the establishment of this analogy, credit is due to the mining engineers who have published their observations at Leadville, and, as Mr. Emmons observes,† have rendered valuable assistance in enlarging our knowledge of the facts as developed by mining.

The text of Mr. Emmons's great monograph on Leadville shows plainly (p. 572) that, under the impression produced by the first publication of Professor Sandberger, the author deemed the as-

* See my treatise (1894), already cited, on the "Origin of Lead- and Zinc-Deposits in Soluble Rocks."

† *Trans.*, xxiii., 601. See also "The Mining Work of the United States Geological Survey," *Trans.*, x., 412 *et seq.*

cension-theory to have been already completely overthrown. He assumes that the type of a vein, as described by earlier authorities, is a purely ideal conception, and does not exist in nature. To show that these writers had before them, on the contrary, a real condition, I have cited the developments at Przibram. If we substitute, in that case, for the space of discission the spaces occupied by the Leadville deposits, the situation, as concerns the question of the direction of the ore-bearing circulation, is not altered. The flat dip of the ore-shoots and the solubility of the country-rock at Leadville are scarcely decisive as to this question. Nor does the depth thus far attained in Leadville mining afford conclusive evidence. In my judgment, therefore, notwithstanding the differences between Przibram and Leadville, the same inference must be drawn in both cases as to the direction of the ore-bearing circulation. In other words, Leadville must be declared to be no exception to the general rule that ore-deposits carrying metallic sulphides have been formed by ascending solutions.

Whether the metallic contents were derived wholly or predominantly from the eruptive rocks adjacent to the deposits or occurring within a certain distance, is an independent question.

Mr. G. F. Becker's criticism (*Trans.*, xxiii., 602), having been prepared without opportunity for a thorough combination of authorities, is considered as preliminary only. It deals, as does that of Mr. Emmons, in the main, with metasomatic formations, without reference to formations in open spaces, and, contemplating the former exclusively, seems to disparage the emphasis which I have laid upon crustification as a clear proof of the filling of open spaces. According to his view, the recognizability of successive deposits is dependent upon incidental local circumstances, but the instances he gives do not appear to me adapted to prove his proposition, that crustification may be produced by other causes than that which I have assigned.

The banded structure of agates, so far as I have had opportunity to study it, is a genuine crustification. It exhibits incrusted nuclei, stalactites, and other formations characteristic of deposition in an open space, quite independently of the question whether changes in concentration or rapidity of circulation or in the substances contained in the solution were the occasion of precipitation. In like manner the precipitate formed upon a piece of iron immersed in a solution of copper sulphate is a genuine crust, the iron serving as the cause of the precipitation; and the circumstances of such a precipitation

in a space filled with solution, though the process take place above ground, present some analogies with underground conditions.

A party of mine-thieves once entered by night an old and extensive mine in Transylvania for the purpose of blasting off and carrying away an exposed mass of gold-ore. The shot opened a hole into an old working (*coranda*, in the Roumanian language), and one of the miners crawled through. The immensity of the space in which he found himself astonished him greatly, but his exclamations of wonder were cut short by the crowing of a cock, which revealed to him that he stood under the night sky, in a great surface-*coranda* or open quarry, which covered the whole area of the mine. Under some circumstances, therefore, it is clear that underground and above-ground are not so very far apart!

A mineral solution standing in a laboratory-beaker, exposed to the air, may practically represent, from our standpoint, a subterranean space, the lower part of which is filled with liquid and the upper part with gas, as I conceived it in my paper (*Trans.*, xxiii., 218).

Mr. Becker doubtless means, by the examples he cites, to argue that the banded structure may originate also through replacement of the idiogenites by xenogenites. This may be true, but his instances do not support the hypothesis; for the pseudomorphosis of galena after *calcite* is not a replacement of *limestone* by galena. Moreover, not every "banded structure" is a crustification.

Mr. Becker names two sorts of indications of replacement, namely, crystalline pseudomorphism and the irregular enlargement of fissures in the replaced mass. I beg to say, that in my paper (*Trans.*, xxiii., 210 and 211), I have mentioned several other signs, such as the retention of the structure of the original mass; the transformation of fossils into ore; the occurrence of remaining nuclei of the original rock, etc., and that I also suppose a metasomatic process to have taken place, when the evidence is merely negative, that is, where indications of cavity-formation, in other words, crustification, are absent. But I have found deposits where the indications of both processes occur side by side, as, for instance, at Rodna, in Transylvania. It was at this place that I had the opportunity, thirty years ago, to demonstrate the metasomatic origin of an ore-deposit. Since that time, however, I have never visited the locality, and have received only superficial data concerning further developments. Outside of calamine-deposits, I have not encountered in my later explorations any cases of metasomatic formation; and I have been led to

attach ever-increasing importance to the deposits formed in open spaces, the list of which, as known to me, has been continually growing, while their definite characteristics have become more and more unmistakably clear. Any difference of opinion which has arisen, as a consequence, between my American colleagues and myself, must be left to the judgment of investigators who are equally familiar with both classes of ore-deposits.

My statement, "It is difficult to believe that metasomatic processes could produce such pronounced ore-shoots as those described at Leadville," must be explained from the standpoint I have taken as to the origin of cavities in a soluble rock. In my paper (*Trans.*, xxiii., 215) I have shown that, before the origin of the cavity, the rock-pores or interstices are filled with saturated solutions, and that a line of maximum flow must be subsequently set up between the point of entrance and some point of minimum resistance, along which line solutions not yet saturated, finding access to the rock, may ultimately dissolve out open channels or cavities. These will then possess a shape extended in one general direction, such as we encounter almost always in ore-deposits in soluble rocks. The Leadville mining engineers have established such a form for the Leadville deposits; and Mr. Becker has also found it in the quicksilver deposits studied by him. If I have correctly conceived the formation of these ore-shoots, they should show some indications of free cavity-formation, even when they have been produced in part by the replacement of the original rock.

Finally, as regards the Eureka deposits, I seem to have been misunderstood. I did not assert that the spaces originally occupied by the Eureka ore-deposits had been formed by surface-waters. I merely said (in accordance with Mr. J. S. Curtis) that this was the case with the caves, which accompany the ores altered and redeposited by the action of surface-waters.

Mr. F. M. F. Cazin has called attention to an American example, furnished by the Vermont copper-mine, in which graphite (or organic matter, the remains of which are now represented by graphite) may have reduced the ore-bearing solutions. Mr. Cazin cites the fossil palms converted into copper-glance, in the Trias of Mexico, as proof that the copper was originally dissolved in the Triassic ocean, though perhaps in too small a proportion to injure animal life. With regard to that I must observe that these palms probably occur in a fresh-water basin, from which the character of the ocean of the period cannot be inferred; nor, *vice versa*, can the traces of copper

found in corals be adduced as indicating the probable presence of copper in such a basin.

R. W. Raymond, New York City: The labor and pleasure of translating Prof. Posepny's contributions having fallen to me, I have taken special interest in the discussion which they have elicited; and I venture to believe that an attempt on my part to summarize the results thus far attained may be useful as a help to the further discussion which I trust will ensue, and will not be deemed an arrogant assumption of the position of a judge, which is as far from my intention as it is beyond my capacity.

No amount of latitude in such a discussion is reprehensible if it elicits new facts; for the accumulation of accurate data is really more important than the mere iteration of argument, and a new fact, however remotely collateral in its bearing, may turn out to be of inestimable value. In this connection, however, it should be noted that the fact is valuable in proportion as it is not merely the expression of an opinion. When we are told by some authority that he "found unmistakable evidences" of this or that, we are simply asked to accept his conclusion, which might or might not have been our own upon the same phenomena; and the weight we give to the fact of his opinion as indicative of the real facts behind it, which are what we want, depends upon our confidence in him, not only as an observer, but also as a reasoner. In my judgment we should be grateful to Prof. Posepny for the emphasis he has laid, not only in this paper but in many preceding publications, upon the supreme importance of what he has called *rein objective Darstellungen*, a phrase which I have weakened in my translations by rendering it "accurate descriptions," in the fear that the term "objective," used in that sense, would be misleading. In this connection I may remark, that when the admirable paper of Prof. Posepny was sent to me, it bore a title which would have been, literally translated, "Subjective Views on the Genesis of Ore-Deposits," the author meaning thereby to indicate modestly that he offered his paper only as an expression of the opinions to which he had been led by his own studies, and not as a statement of the settled results of science. I took the liberty of objecting to this title, on the ground that "subjective" views might be construed as opinions simply "evolved from the inner consciousness," without any foundation whatever in observed facts; and as a result of this correspondence, Prof. Posepny permitted the use of the simpler title, accompanied with such introductory explanations as would relieve him from the imputation of dogmatism.

Accepting, however, his use of "subjective" and "objective" as connoting statements respectively affected or unaffected by individual opinion, we cannot but appreciate and share his desire for "purely objective" reports of observed facts in the field of his studies. And, since it is extremely difficult to convey an "objective" description in writing, the superiority of a careful drawing (not an "ideal" diagram, though that has its uses, and is often a better vehicle of description than words) is clear. Prof. Posepny has practiced his own doctrine by illustrating his paper with numerous drawings, and, I may add, he has unconsciously enforced that doctrine by betraying his own doubts and difficulties in the interpretation of mere verbal and partly "subjective" descriptions, given by other authors.

The misunderstandings thus occasioned may be left to settle themselves through mutual explanations, such as have been made, more or less fully, in the course of this discussion. It need only be added here that Prof. Posepny's conscientious and frank declarations as to the limits of his personal observation and his careful references to all authorities cited, constitute a safeguard against error, a full guide to further investigation and a model for our imitation.

But the chief questions of interest to us, I think, are these: What are the characteristic and valuable contributions made by this paper to the theory of the genesis of ore-deposits? and, What are the definite issues on which Prof. Posepny's views differ from those of other observers, as the latter have been represented in this discussion?

Under the first head I think we may regard as pre-eminent the masterly exposition of the subject of underground circulation and the distinction established between the vadose and the deep circulation, the former actuated mainly by gravity and conditioned upon the relative position of the surface-outflow, the latter complicated by the effects of capillarity and pressure due to heat. This distinction supersedes the vague terms "ascending" and "descending," though the author has employed these terms, in accordance with popular usage, and has thereby incurred some unnecessary criticism. For it is really not of the slightest importance to the general theory of this subject whether a given mineral solution was moving horizontally, or up, or down, when it produced a given precipitate. The only significant question is whether it was *on the way* up or down; that is, whether it belonged to the one or to the other branch of the underground circulation. The third view, namely, that such a solution

might belong neither to the vadose downward circulation nor to the deep upward circulation, but to a "lateral secretion," Prof. Posepny practically declares to be inconceivable. As I understand his argument (or rather, perhaps, as I would state my own view, which I think to be in substantial accordance with his), it may be expressed as follows:

1. The aqueous solutions underground must be conceived either (*a*) as moving on a general downward course, as parts of the vadose circulation, above ground-water level, or (*b*) as penetrating still deeper into the rocks below drainage-level (the barysphere), or (*c*) as rising from those depths under pressure, overcoming gravity, towards or to the surface; or (*d*) as standing (held by capillarity or otherwise) in rocks, whether above or below the drainage-level, and not participating in the circulation at all.

2. Concerning the condition (*a*), which is most open to our observation we know a great deal. We know, for instance, from an overwhelming number of observations, that the solutions of the vadose circulation are oxidizing, and that (apart from the, probably rare, re-formation of sulphides by the action of organic matter) they do not precipitate sulphides, but, on the contrary, attack and decompose them.

3. Concerning (*b*), we know nothing by direct observation, but are forced to believe, and justified (by Daubrée's experiments, etc.) in believing, that such a movement actually takes place.

4. Concerning (*c*), we have the evidence derived from hot springs, etc., which has convinced all observers that there is in fact such an ascending circulation, whatever may be their conclusions as to the depth of its origin or the degree of its agency in forming mineral deposits. The ascension-theory postulates concerning it only that it comes from depths below drainage-level, and is not moved merely by siphon-action, ultimately due to gravity.

5. Concerning (*d*), it may be said that solutions thus held without participation in the general circulation, while they may affect internal changes in the rocks they occupy, cannot begin, *until they begin to move*, a process of redistributing and concentrating by precipitation elsewhere the substances they hold in solution.

6. Moreover, solutions in the condition (*d*), though not participating in the general circulation, must have reached their *locus* by means of that circulation. They must be conceived as having been a part either of the downward or of the upward branch, or, in other words, as arrested portions of the circulation.

7. Whenever they begin to move, they must join one or the other branch of the circulation; and the deposits they may make must be the result of the laws of that branch, operating upon the nature of the solutions, this in turn being partly dependent upon their original source.

8. There is, therefore, no room for a hypothesis of ore-concentration and deposit in bodies of considerable size by "secretion," independent of circulation, or for a cycle of circulation, complete in itself, yet not participating in the general phenomena described. For continuous currents must come from somewhere and go somewhere; and neither inflow nor outlet is provided, except by the conditions of the general underground circulation, as described.

9. From this standpoint it is clear that the source of the substances carried in solution by a current must lie somewhere in the path which that current has traversed. If the theory of lateral secretion means no more than the assertion that the mineral solutions which have precipitated ore in a given fissure or space have traversed and leached some rock before entering that space and that this rock adjoined or lay in "reasonable proximity" to the space of deposition, it would mean, as to the first proposition, nothing that anybody denies; while, as to the second proposition, it would be a somewhat vague assertion, requiring definite proof in each case, and not entitled to the dignity of a general theory.

10. But the theory of lateral secretion, however it may have melted away under the fire of criticism, originally claimed more than this. Prof. Sandberger says:*

"The so-called descension-theory of Werner is purely neptunic, and regards veins as exclusively filled from above downwards by the deposition of ores from liquids, without answering the question, whence these liquids derived their metallic contents. The descension-theory remains good to-day for all cases where, in higher-lying rocks, those substances can be with certainty traced, which have collected as ore-deposits in cavities and fissures in lower-lying rocks, not originally containing them. If the ores are accumulated in fissures, they possess all the characters of fissure-veins. So far as my knowledge of ore-deposits goes, the filling of fissures by ores which can be clearly proved to have filtered in from above is not very frequent; but such fillings of irregular cavities are common."

After mentioning as an excellent instance the lead- and zinc-deposits of Raibl (which Prof. Posepny has discussed with very different conclusions), and declaring that he is at present concerned specially, not with such deposits, but with true fissure-veins, Prof.

* *Untersuchungen über Erzgänge*, von Fridolin Sandberger. Wiesbaden, 1882, *Erstes Heft*, pp. 3, 4.

Sandberger proceeds to state as follows the ascension-theory, which he says, "still counts many adherents," and which he proposes to controvert:

"The ascension-theory assumes in all cases that the ores occurring in a vein-fissure were derived either not at all, or only in part, from the immediately adjacent country-rock (*aus dem unmittelbaren Nebengestein*), but on the contrary, from greater depths, and have been introduced into the fissures either by ascending mineral springs or by sublimation. The substances deposited in the veins should therefore be different from those of the adjacent rock, and should only occur in the latter as lateral impregnations from the fissures."

Confining himself to the supposed agency of ascending mineral springs, the author asserts that such springs would not, and in fact, do not, deposit minerals in their channels, and discusses at some length the case of Sulphur Bank in California, which he declares to be the only instance apparently contradicting his view. He argues against the conclusions drawn by others from this instance, and concludes as follows (p. 17):

"If, then, the only region in which it has been deemed possible to assume the filling of vein-fissures by ascending mineral springs as a process now going on, furnishes no trustworthy proofs for this assumption, what remains? In my opinion, only the leaching of the country-rock which bounds the fissures by seepage-waters which have penetrated it, and which deposit the dissolved materials as ores and gangue in the fissures of the same (or, in exceptional cases, the nearest neighboring) rock.* This is the so-called lateral-secretion theory in its most prosaic form; and it is this to which I have been so distinctly led by many years of observation and investigation that I am forced to consider it applicable to most ore-veins."

11. It is clear that this theory contemplates the exclusion of the agency of waters rising from below drainage-level. That there are such waters, is an admitted fact; and it must be also admitted that they are under pressure great enough to overcome gravity and friction. All fissures accessible to such waters, they must necessarily occupy; and it seems to follow inevitably that all fissures extending below drainage-level must be filled, up to that level at least, with waters either in actual circulation on their way upward, or temporarily arrested and confined. "Seepage" into such spaces is inconceivable.

12. On the other hand, currents under pressure would necessarily

* "Nach meiner Ansicht nur Auslaugung des die Spalten begränzenden Nebengesteins durch Sickerwässer, welche dasselbe durchdrungen haben, und die gelösten Stoffe als Erze und Gangarten in den Spalten des gleichen oder ausnahmsweise auch in solchen des nächsten Nachbargesteins."

penetrate into the pores and interstices of the rocks bounding their main channels, and the deposit in such rocks of minerals carried from the fissures is more probable *a priori* than the deposit, in the fissures, of minerals dissolved from the adjoining rocks. The opposite would be true if the fissures did not contain water, a condition which can only be assumed when there is a lower outlet, that is to say, only in the zone of vadose circulation.

13. The advocates of lateral secretion must state, at least, their conception of the way in which "seepage" can take place from a porous solid holding water into an adjoining space also filled with water, and under higher pressure. That practically no interchange between the two will take place, even if the pressures are equal, is shown by the occurrence of fresh-water springs along our coast, separated by a few feet of sand only from the salt waters of the sea. It is often popularly supposed that the sea-water has been deprived of its salt by "filtration" through the sands; but the real fact is, that the mass of the sea bars the path of a circulation which would carry the spring-water into it, and the spring seeks another way to the surface, where it emerges perfectly fresh. The intervening sands are doubtless filled with brackish water, but this takes no part in the circulation, and therefore carries no salt into the channel of the spring. If the Atlantic Ocean cannot "seep" salt into a spring of fresh water, how could a rock, not included in the path of a continuous circulation, impregnate any portion of that path by its "seepage?"

14. Again, it is conceivable that gash-veins, and other spaces wholly within a given rock-mass, may receive concentrations of mineral by "seepage," though even in this case, if the process is to result in considerable accumulations of mineral, it must be a long-continued one, supported by an inflow and outflow; in other words, it must be a part of a general ascending or descending circulation. And since the ascending circulation involves a pressure from the fissure towards the wall-rock, that is, in the wrong direction for "seepage," it follows that, except in the vadose region, and apart from highly exceptional conditions, the products of the leaching of any given rock-mass are not likely to be found predominantly in adjoining fissures.

15. The theory of lateral secretion, therefore, is essentially confined to the region of the vadose circulation; and those who would apply it to the origin of deposits containing sulphides must be prepared to maintain that those sulphides have been deposited from

solutions moving downwards or laterally, under the influence of gravity, in other words, surface-waters. Prof. Sandberger does not hesitate to accept this alternative, although he does not perceive, apparently, how it confines the sphere of his theory. According to his view, the metals are disseminated in the country-rocks and silicates, and these rocks contain also sulphate of soda, and other soluble alkaline sulphates, as well as chloride of sodium, all of which, he supposes, are converted by organic matter into alkaline sulphides, which transform the metallic silicates into metallic sulphides.

16. But this explanation encounters two serious difficulties. In the first place, it is opposed to the overwhelming evidence that the downward circulation does not characteristically deposit sulphides, but attacks them ; that it does not characteristically contain alkaline sulphides, but alkaline carbonates and free carbonic acid and oxygen. In the second place, the explanation breaks down in the presence of fissures filled with sulphides, extending far below any present or conceivable past drainage-level. The sulphide ore-deposits in such fissures, at the greatest depths attained by mining, show no structural differences or other indications of a different origin, as compared with sulphides in the levels above. There is a change at water-level, but it is notoriously a change from oxidation above, to absence of oxidation below, that level.

17. The lateral-secretion theory, therefore, so far as it is true at all, is no more than a subordinate division of the theory of the formation of deposits in open spaces above drainage-level ; and even here, it is neither necessary nor plausible, as the explanation of deposits which continue downwards, and must be referred, as regards their lower portions, to a deep source. Such deposits may have been altered in character and even in form, in the vadose region ; they probably originated in the deep region.

18. On the other hand, the hypothesis of ascending waters as the vehicle of solution and deposition does not exclude the idea of the leaching of any rock traversed by such waters. It indeed assumes such a leaching as having taken place somewhere. But, as opposed to the theory of lateral secretion (modified to lateral circulation) it assumes the rock immediately adjoining a vein-fissure (when the fissure continues deeper) to be the least likely, not the most likely, source of the metallic ores. And on this point it appeals to the phenomena of crustification. Nothing is plainer than the evidence afforded by the successive crystalline crusts of an amethyst geode, for instance, that the deposition took place first upon the walls of the

cavity, afterwards upon the crust thus formed, and so on towards the central druse. The very first deposit evidently covered the wall with an impermeable layer; and the material for all succeeding deposits must have come (as the sections of many geodes show visibly that it did come) through a passage from without the mass of the geode. In like manner, the crustified filling of a fissure-vein cannot well have come from the walls of the vein at the place where the first crust deposited would necessarily close those walls. The crusts have been deposited from a solution between them. The central druse was not first formed, and then pushed out by successive deposits behind it, as the bark of a tree is thickened. The solution depositing the crystals in successive crusts must therefore have been part of a current; and its entrance and exit can scarcely be sought, as a rule, in the walls it has crusted. A side-fissure, entering through either wall, is, of course, not impossible or uncommon. But it cannot be assumed to exist, without proof. And when such a thing is actually found, its effect upon the vein is so marked as to raise a strong presumption that the normal source of the vein-solutions was not in that direction.

19. Prof. Posepny has laid much emphasis upon crustification, as he has defined that term. I think he is right in so doing; and I may remark incidentally that his use of new special terms (which has been objected to by some), is justified, in this case, as in other cases, by the greater precision of thought thereby secured. The disadvantage of a preference for ordinary and familiar words, when such words may have many meanings, is illustrated by the manner in which Prof. Posepny, on the one hand, and his critics, on the other, have been misled by the ambiguity of "banded structure." He interprets "banded structure," or equivalent expressions, in some of the authorities he cites, as meaning crustification, and they say that banded structure may arise in several ways, intimating thereby that crustification is not a sure proof of deposition upon cavity-walls. The verbal misconception being corrected, it seems to me that there is no difference between the parties on this head.

20. The assertion that a current is necessary for the deposition of such crustified accumulations is not to be construed as excluding variations in velocity, or occasional stoppages and intermissions. The objection of Prof. Sandberger, that mineral springs do not, as a fact, deposit solid substances in their channels, seems to be based upon the conception of such springs as ascending with unvaried velocity, as if through pipes of uniform diameter. Even pipes, as Professor

Posepny reminds us, have been known to receive interior incrustations; but the probability of such deposits is much increased when the effects of variations in the nature and size of the channel are taken into account. *Mutatis mutandis*, the analogy of the deposition of sediments by a running stream is available here. As sands and clays, carried in suspension where the current is most rapid, are dropped where it is checked through widening of the channel, or from other causes, so the deposits of a mineral circulation will naturally be greatest where the movement is slowest, or is even temporarily arrested altogether; and they will be reduced to a minimum, other things being equal, where the current is most rapid. The phenomenon of distinct crustification, in fact, requires the hypothesis of a relative quiescence of the menstruum. And instances are not wanting underground in which the widening of the vein-fissures, or the change to a flatter dip, has apparently favored the deposition of ore.* The ascension-theory does not exclude these obvious considerations. All it asserts is, that the portion of solution entering a given space, and depositing therein a precipitate, must thereafter escape and give place to another portion of solution, if the process is to be repeated; and that, with regard to deposits of sulphides, formed below drainage-level, the only escape is ultimately upward. But the phenomena of crustification in veins afford, in my judgment, another argument against the theory of lateral secretion. Namely, it is well known that the crustification, even in typical fissure-veins, is not everywhere distinct. If it can be observed, with its characteristic central druse, in one part of a vein, it is held (properly, I think) to be (in the absence of evidence to the contrary) a proof that the similar ores of other parts of the vein have been similarly deposited. The absence of crustification in some places may be explained, on the ascension-theory, by the varying speed of the current, and the varying nature and dip of the walls, as affecting the deposition of adherent crystalline crusts. The chemical or physical causes inducing precipitation may simply produce a suspended

* On the other hand, increased width of "vein-matter" has often been due to a splitting of the fissure, and the enclosure of fragments of country-rock, which is afterwards more or less transformed into gangue, or remains as horses in the vein. Or, such increased width may be (as in the Cornwall tin-mines) the result of a mineralization of the country-rock beyond the limits of the original fissure, producing a mass of altered rock impregnated with ore (the *Zinnzwitter* of the Germans). In such cases, while the aggregate of mineral deposited is doubtless much greater than it would have been had the solution passed through the narrow fissure only, the richness of the material is reduced by the admixture of gangue and rock.

precipitate, to be subsequently deposited as a sediment. But if lateral secretion has produced crustification, such as is observed in fissure-veins (as I think, with Professor Posepny, that it has not) then that structure, it seems to me, should be more uniformly distinct in such veins than it is. For the conception of lateral secretion into a fissure excludes the conception of a current under higher pressure, already occupying that fissure; and the local interference of such a current with the quiet process of crystallization is therefore out of the question.

21. The comparatively small amount of mineral matter contained in the ascending springs of the deep circulation, originating below drainage-level, is to my mind some indication that they have already deposited somewhere the larger part of the substances they have held in solution. They are never saturated solutions. As we find them, they contain what we may suppose to be only remaining traces of the metallic constituents which they may (we may almost say must) have carried at greater depths, temperatures, and pressures. Is not the presence of these minute remainders really an evidence of the larger amounts once present, and therefore of a precipitation *en route?* In connection with this question, the probable conditions of the deep zone must be borne in mind, such as, not only the increased solvent power of the waters of that zone, but also the probable slowness of their downward progress, which is practically (according to Daubrée) a seepage, and which must favor the formation of saturated solutions.

22. In reply to this suggestion, the question may be raised, how the deposition of ores, extending almost or quite to the surface, is to be accounted for, if the solutions now encountered below drainage-level, are already so nearly exhausted as to be capable of comparatively little further precipitation. Without forgetting that the most dilute solutions may still give precipitates under chemical or physical changes of condition; and that such precipitates, however insignificant, may attain a considerable aggregate amount by long-continued repetition, I think the more comprehensive answer to the above question is found in the conclusion to which we are led by the ascension-theory, that deposits carrying metallic sulphides, though they reach the present surface, were formed mainly below the influence of the vadose circulation, and therefore under conditions such as may now obtain at depths beyond our observation.

23. This suggests another point, to which Prof. Posepny has called attention, and which was acutely recognized by Cotta, many years

ago,* namely, the fact that speculations upon the relation between the contents of mineral veins and their depth are largely vitiated by the vagueness and uncertainty of the element of depth, as estimated by comparison with the present surface. In most mining regions there is unquestionable evidence of great denudation, which has probably removed from the surface a larger mass than has been penetrated anywhere by mining. It seems impossible, therefore, to argue as to the continuance of ores "in depth," meaning thereby beyond 1000 or 2000 feet from the present surface, when that surface itself may have been 10,000 feet underground at the time the ores were deposited. We may imagine that the ascending waters in a vein now rich in metallic deposits "from the grass-roots down," once continued their upward course to the *former* surface, emerging as dilute solutions; or never reached that surface intact, but encountering the vadose circulation, became a part of it; and, in either case, precipitated less and less metallic matter as they ascended. Conversely, we may reasonably imagine that, if we could retrace the course of a mineral spring coming from the deep zone, it might lead us back to the region where it had deposited the treasure of which it now exhibits, at most, only faint remaining traces. And what we might thus fairly imagine concerning an actual spring might be equally true of the channel of a former spring now closed altogether, or occupied only, under changed conditions of altitude and drainage, by the vadose circulation—that is to say, of a fissure-vein, comparatively barren or lean at the present surface. In other words, the present surface is an arbitrary section, cutting off the veins. Those which it happens to intersect in their richer portions, are naturally the ones which are developed by mining. Those which it shows to be locally barren, are naturally not thus developed, unless local experience supports the hope that they will improve in depth. Such a local experience is doubtless the foundation of the maxim which Cornish miners have carried throughout the world, that "a fissure-vein grows richer in depth," a proposition for which, as a general guide for mining (apart from the effects of surface-waters, which may be sometimes impoverishing), is without foundation in experience. For although a comparatively barren fissure may be, and has often been shown to be, the upper part of a vein carrying rich ores below, there is no general law that it must be so; and, moreover, there is no way of determing *a priori* the depth of the barren zone, measured from the present surface.

* *Die Lehre von den Erzlagerstätten*, Freiberg, 1858. Part I., p. 129.

24. On the other hand, while the varying positions of the present surface prevent generalization as to the relations of ore to "depth," it is unquestionably possible that there may be, in a given fissure, a relation of that kind. The ascension-theory neither asserts nor denies such a supposition. Mr. Rickard's suggestion that the deeper zone must be the region of solution, and a higher zone the region of precipitation, is speculatively reasonable enough; but it amounts to a proposed subdivision of the barysphere into two regions; for the deep zone which Prof. Posepny has called the barysphere includes everything below our observation, and it is in that zone that both solution and precipitation are supposed to have taken place to form the deposits of metallic sulphides. In our ignorance of the conditions of that unknown region, it is scarcely possible or necessary to frame hypotheses concerning them. The practical bearing of Mr. Rickard's suggestion lies in his connection of it with an alleged general phenomenon of the impoverishment of veins in depth, as shown by experience in mining.

25. As to this alleged general phenomenon, I would say first, that even if it were proved, it could hardly be ascribed to the cause suggested by Mr. Rickard, namely, the predominance of solution in lower zones and the confinement of precipitation to higher ones, because the depths reached in mining are not great enough to warrant such a deduction, and also because the instances (such as Przibram) of rich ores continuing for great vertical distances, and down to levels among the deepest ever opened by mining, contradict the hypothesis.

But it must be confessed that there is much evidence which seems to corroborate Mr. Rickard's statement as to the exhaustion of mines in depth. This evidence needs, however, to be carefully collated and critically sifted, before it can be accepted as the indication of a natural law.

a. In such an inquiry all cases must be rejected in which oxidized surface-ores have been mined down to water-level, and the mine has been abandoned by reason of reaching the refractory sulphides. In many such cases the oxidized ores are actually richer (*e.g.* in gold) by reason of the alteration they have undergone; but this is not pertinent to the question of original deposition.

b. The abandonment of mines by reason merely of the increased cost of deep mining must be also set aside as affording no evidence on this subject.

c. The fact that in mining a bonanza is traversed, and a relatively

barren zone occurs below, does not necessarily indicate a relation between barrenness and depth. The occurrence of a bonanza very frequently involves barrenness of the neighboring portions of the vein. That this is the case on a horizontal line is abundantly proved. An instance in point is furnished by the Bullion mine situated on the Comstock lode, between mines which have produced many millions. The expenditure of millions on the Bullion never produced, so far as I know, a ton of profitable ore. Why should not a similar alternation of rich and barren places occur in the vertical line? The cost of exploration in depth, and particularly in sinking, naturally discourages mine-owners; and the abandonment of an operation under such circumstances really proves nothing.

d. In any case of alleged impoverishment of a vein in depth, not only the actual depth below the present surface, but also (so far as it can be estimated) the probable amount of denudation which the surface has undergone, should be taken into account.

e. The nature of the ore also may have a distinct bearing upon this inquiry. It is my impression that of the loose and vague evidence thus far accumulated, a large part refers to gold-ores, and particularly to free gold in quartz, as "giving out" in depth. I remember that in my last conversation with the late Joshua E. Clayton, a close and conscientious observer, he told me that he had personally examined numerous quartz-veins, occurring all along the flanks of the Sierra Nevada, and had found in every case that the veins, as exposed in the deep cañons cross-cutting them, hundreds of feet below their outcrops on the mountains, were poorer in gold than at the higher level. This testimony is valuable, and it may be that it indicates a general law as to such gold-veins; but it must be borne in mind that some of the California gold-mines have been worked deeper than any cañons have cut the veins. Yet, on the other hand, many of the deep gold-mines of the State have been ultimately abandoned.

26. Mr. Rickard's suggestion has a practical side of great importance. Namely, although, in my judgment, there is no established general law, discouraging the exploration of a vein in depth, so long as the fissure continues well-defined, and especially if it carries any thread of ore, it is undoubtedly the case that mining explorations are too much confined to sinking and drifting, and that there is too little cross-cutting for parallel fissures and ore-bodies. To some extent this is one of the results of our absurd United States mining law, which lays so much stress upon the "apex" and the "lode;" but

the mistaken practice of neglecting cross-cuts into the country-rock is not confined to mines operated under that law.

27. Another important point in Prof. Posepny's paper is his proposition (based on Nöggerath's observations in the main, but not lacking other support) that open spaces of dissolution may be formed by ascending as well as descending currents. Since the process of solution depends upon the character of the liquid agent, this is only saying that some ascending waters may be able to dissolve portions of the rocks they traverse; and that if such rocks belong to the class represented by limestone, such currents may produce in them caves and channels, comparable to those notoriously produced by the descending waters. I confess, this seems to me a reasonable proposition, however meager may be the proofs thus far adduced. And I cannot understand, at all events, how opponents of the ascension-theory should object to it; for they do not deny that there are such things as ascending mineral springs, and that these springs hold in solution such substances as carbonates and free carbonic acid. What they deny is that these springs deposit anything in their channels. In that case, they must dissolve without redepositing; and the evidence that they have actually excavated channels underground is afforded by their constitution. They bring the evidence of their guilt with them. To reply that they are part of the vadose circulation only, and hence, no matter what their local direction, belong to the descending branch, is not permissible; for springs encountered at great depths in mining have the composition required to make them active solvents. How can it be doubted that the hot waters of the springs encountered in the Bohemian mines (see Nos. 1, 2 and 3 of Prof. Posepny's table, *Trans.*, xxiii., 234), which contain "a notable quantity of free carbonic acid," would, if they traversed limestone, excavate cavernous channels in it?

28. Moreover, there is reason why a liquid solvent under pressure, occupying a space in a soluble rock, should eat its way upward rather than downward or laterally—namely, because the insoluble portions of the rock, loosened by the action of the solvent, fall away from the roof of the cavity most easily and completely, leaving fresh surfaces open to further attack. Whoever has visited, as I have done, the salt-mines of the Salzkammergut, in the Austrian Tyrol, where salt is extracted by *Sinkwerke*,* and has observed how the great underground rooms, repeatedly filled with water under

* Described in Serlo's *Leitfaden zur Bergbaukunde*, 4th ed., Berlin, 1884, vol. i., p. 611 *et seq.*

pressure, *travel upward* through the mass of the saliferous rock, as their roofs are attacked and dissolved, while their floors are relatively protected by the fallen insoluble *débris*, can scarcely doubt the possibility of the formation of spaces of dissolution by ascending waters. One variety of this extraction—*viz.*, the so-called "continuous watering," employed in some of the mines—presents a still closer analogy. In that method the water is not introduced periodically into each *Sinkwerk*, to be withdrawn when saturated, and wholly replaced with fresh water for further solution. On the contrary, the flow of water is made continuous, fresh water being admitted at one point while saturated brine is conducted away at another. It is true that the actual flow of the current is downward, the fresh water being admitted above and the brine drawn off below; but this is not an essential feature of the process itself. The actual progress of excavation by solution is upward, and the essential condition is the presence of a pressure sufficient to cause the solvent to penetrate the roof. That being secured, the roof is mainly attacked, the side much less, and the bottom scarcely at all.

29. Prof. Posepny's "Theory of the Sinking of Heavier Constituents," as applied to the distribution of gold, etc., in placers, is a valuable addition to our knowledge of such deposits. It is highly desirable that our members engaged in placer or hydraulic mining should give us the results of careful observation upon the conditions presented by the gold-deposits of this country. Few of them have done so thus far, and the field is full of interesting data not yet put into shape for preservation. I am inclined to think, for instance, that "the hypothesis of a natural concentration in running water," which Prof. Posepny disparages, and for which he proposes to substitute the theory just mentioned, would find some support in the phenomena of many American placers, where the gold is concentrated not only on a false or true bed-rock, but in distinct channels along that plane, so that the placer-miners, for many years, have pursued the tortuous channels of "pay-dirt," leaving large areas unworked, which, for some reason or other, did not pay, though they were equally "in the gulch," and had the bed-rock under them, like the rest. I do not mean to deny the possible agency of such a concentration by gravity in loose sands and gravels as Prof. Posepny has postulated, but I fancy it would be hard to explain the distribution of the gold in many of our American placers except by including among its factors the action of running water. If I am correct in this impression, I may venture to consider the case as one in

which Prof. Posepny's heaviest artillery can be turned back upon him; since his theory of "settling" may be called a sort of dry "seepage" or secretion by gravity, and my view may be considered as the assertion that, here as elsewhere, there is no deposition without circulation.

Concerning the differences of opinion developed by this discussion, I think it may be said that, upon closer examination, they are not important, except as to the explanations of certain districts and ore-deposits which Prof. Posepny has rather deduced from the writings of others than based upon observations of his own.

With regard to nearly or quite all of these instances, our own experts are not agreed, so that Prof. Posepny has respectable backing for his views, whether they turn out in the end to be correct or not. Certainly he has presented them with a conspicuous absence of dogmatism, and they have been received on the part of our members, I am happy to say, with the respect due to the merits of a veteran authority, and with gratitude for the generosity which has enriched the *Transactions* of the Institute with one of the most important contributions to technical science ever made through that medium.

F. M. F. Cazin, Hoboken, N. J. (communication to the Secretary): Bergrath Posepny rejects my assumption of the presence of copper in the Triassic sea, claiming that the evidence adduced does not hold good, and observing in support of his view, "that these palms," the cuprified fossils of which are found in the "coarse yellow sandstones and conglomerates overlying the red beds of the Trias," "on the junction of the Trias with the Cretaceous,"* "probably occur in a fresh-water basin."

The fossil in question is identical with "Podozamites crassifolia," described† as occurring in the State of Sonora.‡ Palms cover at

* J. S. Newberry, *Report on the Expedition of* 1859, pages 117 and 118.

† *Ibid.*, p. 145.

‡ It is one of the various strange things observed in geological reports that are the compound work of many, that, although J. S. Newberry prominently and repeatedly refers to the palm-fossils of Nacimiento, his plates show naught under that head, but do show a true image of these "palm-leaves," described as collected in "quite a number" by Mr. Remond from "the Triassic strata at Los Bronces, Sonora," a locality not visited by the expedition of which the report is made. I may mention as an amusing coincidence, that at Prof. Newberry's and at my time there was at Nacimiento a silver-smith, who hailed from Los Bronces, Sonora, and who, whenever in his trade he needed copper, smelted it in a miniature crucible on a miniature Mexican forge with accordion-shaped bellows, using as his material for copper the fossil palm-leaves of Nacimiento, of which within easy walk from his door he could pick up all he was in need of, and of which he kept on hand "quite a number."

this date a narrow belt along the northern coast of South America, disappearing in the interior. The location of the Nacimiento copper-belt is one from which the Cretaceous sea retired last of all on this continent. Its waters at this date are shed into the Gulf of Mexico, with no indication anywhere of a pre-existing barrier. I am acquainted with the English and North German Wealden formation, having mined in it; but, as J. S. Newberry did not, so I did not, find a trace of evidence for assuming a sweet-water formation at the Nacimiento copper-deposits.

My assumption, therefore, stands on proper ground, unless more than a probability to the contrary be offered.

If ever J. S. Newberry's and my own observations as to the geological position and normal character of the deposits in question have been objected to on the ground of actual local observations, I am ignorant of the fact.

Joseph Le Conte, Berkeley, Cal. (communication to the Secretary): All geologists, but especially students of the phenomena of metalliferous veins, are under deep obligation to Bergrath Posepny for the very lucid exposition and abundant illustrations of these phenomena contained in his admirable treatise on the "Genesis of Ore-Deposits." Like the previous treatise of Sandberger, though taking an extreme opposite position, it must powerfully revive the interest of students and observers in the purely scientific theory of metalliferous veins. Although read at the International Engineering Congress of the World's Fair at Chicago, in 1893, it has only very recently fallen under my eye. As I have thought much, and published somewhat on this subject, I beg leave to say a few words in the way of criticism on this masterly work.

All, I think, will agree that one of the chief merits of the work consists in the clear distinction which the author draws between what he calls the *vadose*, or superficial, and the *deep* circulation of underground water; the water in the one case containing air, and therefore oxidizing; in the other, destitute of air, and therefore, non-oxidizing; the one circulation driven by gravity alone, the direction of the current being determined by the place of outflow, the other driven largely by heat received in the lower parts of its circuit, and the direction of its current being mainly upwards.

We are all, I think, especially pleased with the significance he finds in, and the importance he attaches to, the oxidizing and non-oxidizing effects of these two circulations respectively. It follows, from this view, that metallic sulphides are not deposited from the

waters of the vadose circulation, unless under the exceptional conditions of the presence of excess of organic matter; and therefore, that the presence of metals in the form of sulphides is usually a sign of deposit from ascending currents of the deeper non-oxidizing circulation.

Most of us, I think, too (and I among the number), will agree with him, as against Sandberger, that since great deep fissures are not empty, air-filled spaces, but are necessarily filled with water, deposits in them cannot take place by seepage or oozing, or lateral secretion from the immediate bounding-walls. Also, that the phenomena of crustification or ribbon-structure of vein-contents seem to negative such a mode of filling as is supposed by Sandberger; that this structure does not indicate a filling by oozing and trickling of waters containing soluble matters, down on a free surface, but rather a deposit in successive layers inward from water contained in the fissure.

For all this, and very much more which I cannot repeat here, we are under many obligations to Bergrath Posepny. Nevertheless, I cannot but think that he carries his ascension-views much too far; that in his zeal against the extreme lateral-secretion views of Sandberger, he has gone to the other extreme of ascensionism; and that a truer view than either may be found in one that shall combine and reconcile these two extremes.

The evidence of the extremeness of his views is found, and indeed, is embodied, in his use of the term *barysphere*. As contrasted with *lithosphere*, this term can only mean a region in the interior of the earth, the materials of which are heavier, because more metalliferous, than the superficial lithosphere visible to us. From such a metalliferous barysphere, he thinks, all the metals of ore-deposits (with trifling exceptions) are derived. It is true, that in his reply to objectors, he speaks of his barysphere as only the equivalent of the "unknown depths" of other writers; but, it must be remarked, that this latter term, while open to the objection of indefiniteness, does not, necessarily, carry with it any implication of a region peculiar in its density and in the abundance of its metallic contents, although it is doubtless often used with this implication. The word barysphere, on the other hand, fixes definitely an idea which has long floated vaguely in the minds of many writers on this subject. It will, therefore, form the central point of my criticism.

I.—Is there a Barysphere within Reach of Circulating Water?

It is true, that the earth increases in density from the surface towards the center, and probably to the very center itself. This is shown by the fact that the mean density of the earth is more than double that of the superficial parts. It is true, also, that the increasing density, while certainly due, in part, to condensation by increasing pressure, is probably also due, in part, to difference of material, and especially to the presence of metals, as sulphides or native, in greater abundance in the interior parts. It is true, therefore, that the deeper parts of the earth are certainly heavier, and probably more metalliferous, than the superficial parts. In a word, it is true that there is a barysphere, and probably in the sense used by Posepny, as being more metalliferous. But how deep must we go to find this barysphere? Let us see.

Taking the density of the superficial parts of the earth (or what Posepny would call the lithosphere) at 2.5, and the mean density of the earth as a whole at 5.5 (Posepny accepts these figures), and assuming the simplest rate of increase, viz., a uniform rate, then an actual density equal to the mean density of 5.5 would be reached at the depth of 1000 miles, and the central density would be 14.5* This is an increase of 3 in 1000 miles. At the depth of 100 miles, therefore, the increase would be 0.3 and the density only 2.8. Is it at all probable that we ever have circulating water coming up from any such depth as 100 miles? And yet, 2.8 is only about the density of our more basic eruptives, and therefore, wholly undeserving the name of a barysphere. Circulating water may possibly come up from as deep as 10 miles, but, at the same rate of increase, the density there is only 2.53—an increase over the superficial density wholly inappreciable. Dr. Raymond, interpreting Posepny, defines the barysphere as all that interior region, the circulating water of which would not come up at all without the aid of heat. Does this mean all but the superficial region traversed by the vadose or oxidizing circulation? If so, it cannot be far from the surface, and the term barysphere, as applied to it, is surely wholly inappropriate and misleading.

But it may be answered that all this reasoning is based on the

* By mathematical calculation based on the above conditions, an actual density equal to the mean density of 5.5 is reached at depth of $\frac{1}{4}$ radius from the surface. Multiplying this gain of 3 by 4 and adding the surface density of 2.5 makes a central density of 14.5 ($3 \times 4 + 2.5 = 14.5$).

assumption of a *uniform* rate of increase of interior density; while in fact the great mean density of the earth may be explained by the existence of a highly metalliferous shell at no great distance beneath the surface and therefore within easy reach of circulating waters. To this view I make the following objections:

1. All our general reasonings concerning the cause of the great mean density of the earth, whether (*a*) condensation by increasing pressure, or (*b*) arrangement of materials of a primal fused earth according to their relative specific gravities, would make the increase progressive to the center. In fact it is hard to conceive the conditions under which a dense metalliferous shell a little way beneath the surface could be formed.*

2. We have abundance of materials coming up in eruptions from depths as great as circulating water is ever likely to reach, and yet these materials show no such density and metalliferousness as is implied in the term barysphere.

But again, it may be objected that I greatly underestimate the depth which may be reached by underground water. This brings up an important but difficult question. Is there any limit to the depth to which meteoric water may penetrate? If so, what determines the limit and where is it? These are questions which science is probably not yet prepared to answer definitely. I once thought, that since the pressure of a water-column increases uniformly with the depth, while the elastic tension of steam in contact with water increases with increasing heat at an increasing rate, so as to develop a logarithmic curve, there must be a depth at which the tension of steam would be equal to the downward pressure and that at that depth would be found the limit of underground water; and I expressed this conclusion in my *Elements of Geology*, page 99. Further reflection has convinced me that the conclusion is unwarranted. Such a limit would undoubtedly be reached if the increase of tension continued to follow the same law indefinitely. But it is now known that at a certain temperature, called the critical point, steam has the same density as the water from which it is formed. At this point, therefore, it may be regarded as either steam or water indif-

* Of the two causes mentioned above, the first would probably produce increase at an increasing rate and put the place of density equal to mean density deeper than ¼ radius down. The second might give rise to any kind of rate according to the relative amount of the different kinds of metals; but not improbably to a decreasing rate and put the place of mean density higher. The combination of these two would make an indeterminable rate; but something like a uniform rate is as probable as, perhaps more probable, than any other.

ferently, and under the slightest change of temperature it takes the one form or the other. Beyond this point it is no longer steam in contact with water, but dry steam, which we know follows an entirely different law. Now the critical point of water is about 700° F. and the tension of steam at this point is about 200 atmospheres. Taking the increase of underground temperature at 1° for 53 feet, or 100° per mile, the temperature of 700° would be found at the depth of seven miles. But the pressure of a water-column there would be about 1100 atmospheres. The tension has not yet even nearly reached the pressure; and, as the law changes here, it would seem that the tension would never overtake the hydrostatic pressure at all. Therefore, if the underground water is limited at all in its downward course, as is probably the case, it must be limited in some other way, probably by increasing compactness of material, under the increasing pressure of superincumbent rock, which, by closing up the pores, would inhibit further penetration, or would make it easier for the water to come up again in ascending currents.

I think we may reasonably conclude, therefore, that whether there be a limit to underground water or not, it is certain that below a certain moderate depth, say 8 or 10 miles, such water cannot be circulating; for beyond this the compactness of rock under superincumbent pressure would be such, that while capillarity and weight of water-column might still urge further movement, passages sufficiently open to allow currents of circulation could not exist.

We may assume, then, that the limit of circulating water cannot be more than 10 miles in depth. Below this, water may indeed penetrate by capillarity and by weight of its own column, but such water does not enter into ordinary circulation, although it may come up in volcanic eruptions and indeed supply the force of such eruptions. Still, below this again, and even to the very center, there may possibly be what Fisher calls *constituent* water, *i.e.*, original water occluded in the primal fused magma of the earth, still present in the interior and coming up in volcanoes and (according to him) the cause of their eruptions. If there be such, it is not circulating water in the ordinary sense, and therefore may be left out of account in this discussion.

Underground water may be conceived, therefore, as existing in three possible conditions, but more and more doubtfully in the order named:

1. Circulating meteoric water. This of course is certain. It probably extends but a few miles (8 or 10) below the surface.

2. Meteoric water, but not circulating. The existence of this is probable. I have been accustomed to call it "volcanic" water, because it is a probable source of the eruptive force of volcanoes.

3. Constituent water, originally occluded in the primal magma of the incandescent fused earth, and still occluded in the materials of the interior. This, Fisher thinks, is still escaping, and in doing so, fuses its way towards the surface, and finally emerges in volcanic eruptions. This, of course, is very doubtful.

Of these three, if they all exist, we are concerned, I believe, with the first only.

We have assumed 10 miles as the limit of circulating water, and therefore the limit of depth from which metals may be derived. But at that depth, as already shown, there is no "barysphere" in any intelligible sense of that word. For the difference in density and in metalliferousness between the rocks there and those at the surface is quite inappreciable. We have, in fact, much material coming up from this very region, and therefore know its density. Our more basic rocks are indeed far denser and more metalliferous than the average of that region, having acquired greater density by differentiation from an average magma representing that region.

I believe, therefore, that the greater abundance of metallic ores in solution in ascending waters is the result, not of the greater abundance of metals in their lower courses, but of the greater *heat* which they take up in that part of their course and the greater *pressure* to which they have been subjected there. Both heat and pressure greatly increase the solvent power of water upon the feebly soluble metallic sulphides. Thus heavily freighted, the waters lose, in ascending, both heat and pressure, and therefore deposit abundantly in their upward course. In a word, ascending waters are rich in metallic contents, not because they have traversed a barysphere, but *because they have traversed a thermosphere.* With equal heat and pressure, I am convinced, they would get as much metal from our more basic rocks here at the surface as they now do from the hypothetical barysphere. These ascending waters are non-oxidizing, not because they have never seen the air, *i.e.*, are not meteoric, but because they have exhausted their oxidizing power by previous oxidation of metals, of organic matters, and other oxidable substances in the upper parts of their downward course.

II.—Vadose *vs.* Deep Circulation.

Again, I think, Posepny draws much too sharp a distinction between his two kinds of circulation; not indeed as to their oxidizing

and non-oxidizing properties, but as to the force of circulation in the two cases respectively. In his anxiety to distinguish them sharply, he speaks as if the forces of circulation in the two cases were entirely different, being gravity or hydrostatic pressure in the one case and heat and capillarity in the other. Now nothing can be more certain than that hydrostatic pressure is the *fundamental* cause in both cases alike; although heat, by lightening the ascending column and thus disturbing the hydrostatic equilibrium, is the *immediately determining* cause in the latter. As Mr. Rickard, in the discussion, has justly pointed out, the effect of heat in the underground circulation is exactly like its effect in determining circulation in a system of house-warming pipes.

Again, Posepny lays much stress on *capillarity* as an additional force urging forward the circulation. But surely this cannot be so. Capillarity is indeed a powerful force, urging water to where there is none, but an equally powerful force fixing it where it is. So far from assisting, it powerfully impedes circulation, and, where it is strong enough, inhibits it altogether. Dry clay is a powerful absorber of water, but, when once wet, it becomes impermeable to circulation.

In fact, Posepny sometimes speaks of the deep barysphere circulation, as contrasted with the vadose circulation, in such terms that one is left in serious doubt whether he regards the former as meteoric water at all; and yet he speaks of it as circulating. Sometimes it seems as if he regarded his vadose water alone as meteoric and his barysphere water as some other kind of water coming up from the deep interior of the earth, like, for example, the constituent water of Fisher. Such water, if there be any such, might indeed be conceived as coming up from a metalliferous barysphere, such as he supposes. But this would be escaping water, not circulating water. If he means anything like this, it ought to be distinctly stated, for it changes entirely the ground of the discussion, and much that I have said above would be wide of the mark. For my own part, unless we adopt Fisher's view, I believe that we never have any water coming up which has not previously gone down. This is what is meant by circulation, but I cannot think Posepny can mean that his deep circulating water is not meteoric; and I therefore say nothing more on this head.

III.—Leaching of Wall-Rock.

Again, although I fully agree with Posepny and his brilliant expositor, Dr. Raymond, that crustification, when it is well developed,

indicates deposit from within, by ascending waters already occupying the fissure, and not by laterally incoming water depositing in the act of incoming (in the manner of seepage-water in empty cavities), yet I cannot agree with them in thinking that the pressure of such ascending water would necessarily or even usually prevent the incoming of lateral currents from the wall-rock. It is doubtless true that the ascending water in the fissure is under higher pressure than *precisely similar* water on the outside; for, in addition to the hydrostatic pressure determined by the height of the outlet, it is also under hydraulic pressure in proportion to the velocity of the upward current. But the water saturating the wall-rock is also, of course, under heavy hydrostatic pressure. And when we remember the slowness of the ascending current (which is a necessary condition for deposit), and therefore the slight excess of the pressure over that measured by the height of its outlet; and when we remember further that the ascending water is *hot* while the wall-water is *cooler*, and therefore denser, we may well doubt whether the pressure of the ascending or the lateral waters will be the greater, and therefore whether the current will set outward or inward. The pressure of the ascending water is greater by virtue of its motion, but that of the wall-rock is greater by virtue of its greater density. It seems not unreasonable, therefore, to conclude that sometimes and in some places the current would set outward, and sometimes and in some places it would set inward. In many places, doubtless, the wall-rock is not saturated. In such places, of course, the current would set outward by capillarity, as well as by pressure, until saturation is reached. Of course, also, impediments to upward flow, brought about by filling of the fissure by deposit or otherwise, would increase the interior pressure, and would cause an upward ramification and outflow in many places at the surface.

Although the analogy is by no means perfect, yet, by way of illustration, the ascending fissure-current, with its freight of dissolved matters and its tributary drainage from the country-walls, may be roughly compared to a main river with its freight of suspended materials and its lateral tributaries. In such a stream, the tributaries usually discharge freely into the main river, increasing its volume, though perhaps diminishing its percentage of freight; but sometimes, by the greater pressure of flood-waters, the main stream may back up the tributaries until equilibrium is restored. So in the case before us, the main ascending fissure-stream, with its freight of dissolved matters, usually receives tributaries from the wall-rock,

although, by defect of pressure of the latter or increased pressure of the former, the main current may overflow into the wall-rock. Again, in both cases, the percentage of freight is usually greatest in the main stream, and therefore the deposits by diminished velocity and carrying power in the one case and by diminished heat and pressure and solvent power in the other, are heaviest there, although, sometimes, heavy deposits occur also in the back waters. Again, in both cases, while the tributaries increase the volume of the current, they usually diminish the percentage of freight, although sometimes the reverse may be the fact. Finally, as rivers, when obstructed by their own deposits, may reach their final destination by inverse ramification and through many mouths; so ascending fissure-currents, obstructed by their own deposits, may branch upward and reach the surface by many exits. This, however, can be seen only in ascending currents still depositing, as in the cases of Sulphur Bank and Steamboat Springs. In most cases this part of their course has been carried away by erosion.

In a word, there seems no reasonable doubt that while usually the main deposits have been brought up from below, yet the tributaries from the country-wall do contribute, and sometimes in an important degree, to the metallic contents of the veins. This seems well-nigh proved in those cases given by Sandberger and Becker, in which analyses, especially selective analyses, find notable quantities of the required metals in the more basic minerals of the country wall-rock. To discredit the obvious inferences from the results of a method so much in accord with modern science and substitute a roundabout process of secondary leachings by vadose circulation of primary impregnations derived from a hypothetical barysphere, as Posepny does, must be regarded as a return to the speculative methods of early writers. Again, in cases like the lead-ores of Missouri and Wisconsin, where there is no evidence of disturbance or of igneous agency of any kind, is it not more rational to derive the metals from the wall-rock, though probably from its deeper parts, than from an unknown barysphere?

IV.—A More Comprehensive Theory Needed.

In conclusion, I cannot but think that the views brought forward in 1883 in my paper on the "Genesis of Metalliferous Veins" (*Am. Jour. of Sci.*, vol. xxvi., p. 1, 1883), although I would perhaps now modify them slightly on some points, still represent well the present condition of science on this subject. Those who have read that

paper will remember that it is an attempt based partly on my own investigations of the phenomena of metalliferous vein-formation now going on at Sulphur Bank and at Steamboat Springs, and partly on a general survey of the whole field, to embody a comprehensive and rational theory, avoiding extremes on both hands. In it I devoted considerable space to combating the extreme lateral-secretion views of Sandberger. I did so because, on account of the recent appearance and signal ability of his treatise, it seemed likely to do harm by carrying scientific opinion too far in one direction. If it had been Posepny's treatise instead of Sandberger's, I should have felt equally compelled to combat it, and on the same ground. Posepny quotes freely from my papers on "Sulphur Bank" and on "Steamboat Springs," but not from that on "Genesis of Metalliferous Veins." Whether he has seen it, I do not know.

There has always been, and still is, a strong tendency to extreme views on this subject. On the one hand, ascensionists would derive all metals from a mysterious metalliferous region—a "barysphere," and so strong is their advocacy that even when analysis finds the required metals in notable quantities in the wall-rock, they discredit the obvious inference by suggesting a secondary leaching of materials deposited there by primary baryspheric currents. On the other hand, the lateral-secretionists would derive metals not from ascending currents at all, but wholly from direct secretion from the immediate bounding-walls; and so strong is their advocacy that even when the deposit of metals from hot ascending currents is proved by direct observation, as at Sulphur Bank and at Steamboat Springs, they seek to throw discredit on the obvious inference in regard to all metalliferous veins, by giving many cases in which hot springs do not deposit any metals. My paper was an earnest attempt to combine what is true in each, and thus to reconcile these extremes by a more comprehensive view, which explains their differences.

According to my view, the source of metals is, indeed, on the one hand, by leaching, but not by lateral secretion; on the other hand, not from a hypothetical barysphere, but from the wall-rock; though, again, not from all parts alike, but mainly from the deepest parts, and even from below the deepest parts, of sensible fissures. As in the case of many other disputes, I believe both sides are right and both are wrong. Ascensionists are right in deriving metals mainly by ascending currents from great depths, but wrong in imagining these depths to be an exceptionally metalliferous barysphere. They are wrong also in not allowing subordinate contributions by lateral

currents from the wall-rock higher up. The lateral-secretionists, on the other hand, are right in deriving metals by leaching from the wall-rock, but wrong in not making the main source the thermosphere.

In the uncolored light of a more comprehensive view, many of the difficulties and obscurities of the subject disappear.

1. Ore-deposits, using the term in its widest sense, may take place from many kinds of waters, but especially from alkaline solutions; for these are the natural solvents of metallic sulphides, and metallic sulphides are usually the original form of such deposits.

2. They may take place from waters at any temperature and pressure, but mainly from those at high temperature and under heavy pressure, because, on account of their great solvent power, such waters are heavily freighted with metals.

3. The depositing waters may be moving in any direction—up-coming, horizontally moving or even sometimes down-going, but mainly up-coming, because by losing heat and pressure at every step, such waters are sure to deposit abundantly.

4. Deposits may take place in any kind of water-ways—in open fissures, in incipient fissures, joints, cracks and even in porous sandstone, but especially in great open fissures, because these are the main highways of ascending waters from the greatest depths.

5. Deposits may be found in many regions and in many kinds of rocks, but mainly in mountain-regions and in metamorphic and igneous rocks, because the thermosphere is nearer the surface, and ready access thereto through great fissures is found mostly in these regions and in these rocks.

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[NOTE.—In this Index the names of authors are printed in small capitals, and the titles of papers in italics. Casual references, giving but little information, are usually indicated by bracketed page-numbers.]

ERRATA.

On page 76, in the engraving of coal-sections in Wise County, Va., the totals under Section 21 given as "Seam, 8 ft., Coal, 7 feet 11 in.," should be Seam, 7 ft. 11 in., Coal 7 ft. 8 in.

On page 582, line 5 from the top, "Refining Company" should be Rolling Company.

On page 897, in the third paragraph from the bottom (marked "1"), the words "dolomite" and "limestone" should be transposed, the percentage of sulphur being 0.125 in the limestone, and 0.0645 in the dolomite.

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